Vol. 55. Jan. 1820. No. 261.

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 CCLXI,

For JANUARY 1820.

WITH A PLATE BY PORTER
Exhibiting Sketch of the Comet's Path of July 1819.

BY ALEXANDER TILLOCH,

M.R.I.A. M.G.S. M.R.A.S. MUNICH, 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; Highley; Sherwood and Co.; Harding; Underwood; Simpkin and Marshall; London: Constable and Co. Edinburgh; Brash and Reid; Duncan; and Penman, Glasgow: and Gilbert and Hodges, Dublin.

Vol. 55. FEB. 1820. No. 262.

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 CCLXII.
For FEBRUARY 1820.

WITH A PLATE BY PORTER
Illustrative of the Annular Eclipse of the Sun on the 7th of September next.

BY ALEXANDER TILLOCH,
M.R.I.A. M.G.S. M.R.A.S. MUNICH, 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; HIGHLEY; SHERWOOD and Co.; HARDING; UNDERWOOD; SIMPKIN and MARSHALL; London: CONSTABLE and Co. Edinburgh: BRASH and REID; DUNCAN; and PENMAN, Glasgow: and GILBERT and HODGES, Dublin.

ENGRAVINGS.

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 Embryos the of the Seeds are formed in the Root alone.—A Plate to illustrate Mr. John Walter's Improvements in Naval Architecture.—A Quarto Plate on the Roots of Plants.—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.—A Quarto 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 of Mr. Stephenson's Safe-Lamp for Coal-Mines.

Vol. XLVII. Illustrated with a Quarto Plate describing the Planisphere of Dendera.—A Quarto Plate to illustrate Mr. Brown's Paper on Architecture.—A Plate to illustrate Sir George Cayley's Paper on Aërial Navigation; and Mr. London's Hydrometer.—A Quarto Plate of the Temple of Kournou at Thebes.—A Plate of the new Baths at Ramsgate.

Vol. XLVIII. A Quarto Plate of the Strand or Waterloo Bridge erected over the Thames at the Savoy, London.—A Quarto Plate to illustrate Mrs. Ibbetson's Anatomy of Vegetables.—A Quarto Plate to illustrate Mrs. Ibbetson's Paper on the Physiology of Vegetables.—A large Quarto Plate to illustrate Mrs. Ibbetson's Paper on a new View of Vegetable Life.—A Quarto Plate to illustrate Mrs. Ibbetson's Paper on the Physiology of Vegetables; and Mr. Mornay's Mass of Native Iron found in Brasil.

Vol. 55. March 1820. No. 263.

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 CCLXIII.
For MARCH 1820.

WITH A PLATE BY PORTER

Illustrative of Mr. Lane's Instrument for gathering Fruit; Mr. Young's Mode of preparing Opium from the *Papaver somniferum;* and of Captain Forman's Essay on a Property in Light which hitherto has been unobserved by Philosophers.

BY ALEXANDER TILLOCH,
M.R.I.A. M.G.S. M.R.A.S. MUNICH, 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; Highley; Sherwood and Co.; Harding; Underwood; Simpkin and Marshall; London: Constable and Co. Edinburgh: Brash and Reid; Duncan; and Penman, Glasgow: and Gilbert and Hodges, Dublin.

Vol. 55. APRIL 1820. No. 264.

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 CCLXIV.
For APRIL 1820.

WITH A PLATE BY PORTER
Descriptive of Mr. CUTHBERT's improved Hydro-pneumatic Apparatus, &c.

BY ALEXANDER TILLOCH,
M.R.I.A. M.G.S. M.R.A.S. MUNICH, 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; HIGHLEY; SHERWOOD and Co.; HARDING; UNDERWOOD; SIMPKIN and MARSHALL; London: CONSTABLE and Co. Edinburgh: BRASH and REID; DUNCAN; and PENMAN, Glasgow: and GILBERT and HODGES, Dublin.

ENGRAVINGS.

Vol. XLVIII. A Quarto Plate of the Strand or Waterloo Bridge erected over the Thames at the Savoy, London.—A Quarto Plate to illustrate Mrs. IBBETSON's Anatomy of Vegetables.—A Quarto Plate to illustrate Mrs. IBBETSON's Paper on the Physiology of Vegetables.—A large Quarto Plate to illustrate Mrs. IBBETSON's Paper on a new View of Vegetable Life. —A Quarto Plate to illustrate Mrs. IBBETSON's Paper on the Physiology of Vegetables; and Mr. MORNAY's Mass of Native Iron found in Brasil,

VOL. 55. MAY 1820. No. 265.

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 CCLXV.

For MAY 1820.

BY ALEXANDER TILLOCH,
M.R.I.A. M.O.S. M.R.A.S. MUNICH, 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; HIGHLEY; SHERWOOD and Co.; HARDING; UNDERWOOD; SIMPKIN and MARSHALL; London: CONSTABLE and Co. Edinburgh: BRASH and REID; DUNCAN; and PENMAN, Glasgow: and GILBERT and HODGES, Dublin.

TO CORRESPONDENTS.

Capt. FORMAN'S Paper was received at too late a period to be inserted this month.

Mrs. IBBETSON on Botanical Physiology has been received, and will be inserted in our next. The other Communications alluded to by Mrs. I. will meet with every attention.

ENGRAVINGS.

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.—A Quarto 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 of Mr. STEPHENSON'S Safe-Lamp for Coal-Mines.

Vol. XLVII. Illustrated with a Quarto Plate describing the PLANISPHERE of DENDERA.—A Quarto Plate to illustrate Mr. BROWN'S Paper on Architecture.—A Plate to illustrate Sir GEORGE CAYLEY'S Paper on Aërial Navigation; and Mr. LONDON'S Hydrometer.—A Quarto Plate of the Temple of Kournou at Thebes.—A Plate of the new Baths at Ramsgate.

Vol. XLVIII. A Quarto Plate of the Strand or Waterloo Bridge erected over the Thames at the Savoy, London.—A Quarto Plate to illustrate Mrs. IBBETSON'S Anatomy of Vegetables.—A Quarto Plate to illustrate Mrs. IBBETSON'S Paper on the Physiology of Vegetables.—A large Quarto Plate to illustrate Mrs. IBBETSON'S Paper on a new View of Vegetable Life. —A Quarto Plate to illustrate Mrs. IBBETSON'S Paper on the Physiology of Vegetables; and Mr. MORNAY'S Mass of Native Iron found in Brasil.

Vol. XLIX. A Plate to illustrate Dr. EVANS'S Communication on Terrestrial Magnetism; a new Electro-atmospherical Instrument; and Mr. ANDREW HORN'S Paper on Vision.—A Quarto Plate to illustrate Mrs. IBBETSON'S Physiology of Vegetables.—A Plate to illustrate the Solar Spots which appeared during the Year 1816;—and Mr. BEVAN'S Improvement on the Sliding-Rule.—A Plate descriptive of Mr. EMMETT'S Instrument for the Measurement of the Moon's Distance from the Sun,

VOL. 55. JUNE 1820. No. 266.

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 CCLXVI.

For JUNE 1820.

WITH A PLATE BY PORTER

Illustrative of Capt. FORMAN's Essay on the Reflection, Refraction and Inflection of Light, &c.; and Mr. CHAS. BONNYCASTLE's Communication respecting the Influence of Masses of Iron on the Mariner's Compass.

BY ALEXANDER TILLOCH,
M.R.I.A. M.G.S. M.R.A.S. MUNICH, 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; HIGHLEY; SHERWOOD and Co.; HARDING; UNDERWOOD; SIMPKIN and MARSHALL; London: CONSTABLE and Co. Edinburgh: BRASH and REID; DUNCAN; and PENMAN, Glasgow: and GILBERT and HODGES, Dublin.

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. M.G.S. F.S.A. EDIN. AND PERTH, M.R.A.S. MUNICH, M.S.E.I.N. OF FRANCE, &c. &c. &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. LV.

For JANUARY, FEBRUARY, MARCH, APRIL, MAY, and JUNE, 1820.

LONDON:

PRINTED BY RICHARD AND ARTHUR TAYLOR, SHOE LANE:

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

CONTENTS

OF THE FIFTY-FIFTH VOLUME.

CONTENTS.

CONTENTS.

CONTENTS.

THE

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

I. *Remarks on Damp Decks, and other Circumstances in the Medical Œconomy of Ships.* *By* A Naval Surgeon.

To Mr. Tilloch.

Sir, — In allowing in your valuable miscellany [The Philosophical Magazine] immediate publicity to the following remarks on Damp Decks, &c. you will, in all probability, particularly in such humid weather as the present, render an essential service to that useful class of men for whose benefit they are solely intended.

I have the honour to be, sir,

Your most obedient and very humble servant,

Portsmouth, Dec. 22, 1819. A Naval Surgeon.

Damp Decks, &c.

"The ship, in the first place, by frequent drying and ventilation, is preserved from damp and foul air."—Dr. Parr's London Medical Dictionary, art. *Scorb.*

A ship in the midst of the deep, having by far its greater part immersed beneath the surface, is a circumstance that suggests to us the natural formation and collection of vapour in it, and likewise the difficulty of its extrication, on the additional considerations of the nature of the one and of the form and structure of the other.

Besides, water insinuates itself more or less into the holds of all ships, which, with the water in casks, &c. must in a greater or less degree be productive of vapour. If to this be added the great portion of steam which arises at the meals, &c. and the great quantity of exspirated vapour thrown out by several hundreds of men so closely situated, the quantity of vapour thus collected, is so great as to need an incessant extrication of it, particularly when we consider its intermixture with what is implied by the foul air of ships; namely, air consisting of a redundancy of carbonic acid and an excess of azote, or deficiency of oxygen from the partial renovation of its consumption; and likewise, in a certain degree, noxious gases, arising from animal *effluvia*, especially that contained in unaired bedding, unwashed clothes, &c. and the putrefaction, however slow, of provisions; all which render it the more noxious.

Reflections on this invariable tendency to moisture, and on the abovementioned particulars, point out the extreme necessity of keeping the decks and cabins of ships as dry, yet clean, as possible. Experience and science grant, that the internal cleanliness of ships is not only conducive to health, but absolutely essential to the prevention of disease.

The mode of effecting cleanliness is, however, often extremely injudicious, or betrays inconsideration, or something like ignorance. What honest candour extorts, surely will not be ascribed to presumption. How many instances have there been of washing decks, and barely swabbing them after, when the entire atmosphere has teemed with fogs and moisture, without a breath of wind, when stoves have been deemed unnecessary, or not so much as thought of! To say the truth, under such peculiarities of the atmosphere, common stoves can be of very little use; for the rarefaction produced by their use tends but little, from its slowness, to the extrication of moist air, when wind-sails can be of little or no use. It causes an ascent not entirely to the hatchways, but in part to the beams and planks overhead, where the air thus partially rarefied insinuates itself, or is attracted; and on becoming somewhat condensed, accumulates, until its dispersion by change of weather or alteration of temperature. Hence it is that decks washed in damp weather are often not dry for two, three, four, and even sometimes seven days after. In such states of the air, an exact scrape and clean dry sweep is perhaps the only mode necessary to be adopted.

In frosty weather, or when the temperature is very low, some estimate can be made of the surrounding vapour from its collection and condensation every morning overhead, on the planks and beams of the lower decks and confined cabins.

In the navy, the system of washing or scrubbing hammocks or decks is, like every other system of it, rigid in the extreme. Clothes, as shirts, stockings, &c. are generally washed or scrubbed twice a-week, oy which changes of the same are as often afforded, and are as conducive to comfort as absolutely essential to health. Hammocks, as far as my experience authorizes me to say, are also scrubbed twice, or, at least, once a-week all the year round. In my humble opinion, once a-week in summer or fortnight in winter, and relatively in hot or dry, and cold or moist climates, if not carelessly left about the decks, is adequate to the purposes of cleanliness. Is the frequency beyond this indispensable?

sable? or, does it arise from a mere ostentatious principle of useless refinement in bleached hammocks, which, through self-conceit and contrast, glut the eyes of such as virtually have the superintendence of them?

The quarter and main decks are washed every morning at four o'clock, or at day-break, by those who slept-in during the middle watch, or from twelve until the same hour; and the lower decks of frigates, and orlop decks of line-of-battle ships, are washed twice, or, at least, once a-week throughout the year.

I will not presume to determine how often, or precisely when the decks of ships should be wetted; but, by the way, simply observe that the time should be regulated by intervening circumstances; and the less the necessity, and the drier the ship, the healthier it is for ships' companies.

In ships of-the-line, the œconomy of the sick-bay rests with the surgeon. But as it is comparatively small for the number on an average sick, the patients who have not room to sleep in it, must lie between decks, with the regulation of which he has nothing to do. In frigates the sick are also under the necessity of sleeping on the main deck, unscreened from the strong partial draughts, as well as unprotected from the rude jostle of the thoughtless. These are very sensible of the great alteration of temperature from the early opening of the ports and wetting or washing of the decks. This sudden alteration of temperature is frequently as much or more than ten degrees. Those who perchance are partially uncovered, instantaneously awake, from the sensations induced by the change, or, if they do not, they are likely to suffer by it; because an immediate alteration of temperature to, or beyond this degree, is known to be generally productive of inflammation.

In detached services, and in long cruizes and voyages, while such a system of washing and slopping prevails, and to which every sacrifice is made, need the dreadful consequences be pointed out? It is nonsense to say that it makes men hardier, or obviates an unnatural tenderness. A learned philosopher and ingenious moral writer, in his inimitable essays, succinctly points out, with his usual discrimination, the dangers arising from the fallacy of such notions.

There are many on board ships variously affected, who yet do not apply to the surgeon for relief, through the fear of being stigmatized as sculkers, or the greater fear of having their allowance of wine or spirits withheld. Such have their hectic exacerbations, and have to turn out of their hammocks, as the phrase is, on these occasions, and strut about the decks for two hours or more, in cold water half way up their legs, handing or dashing it along by buckets full; by which the perspiration becomes suddenly suppressed, and the body has to encounter the ill effects of the sudden transition from heat to cold, particularly from its long continuance. These observations are in every sense applicable to the predisposed or susceptible to tubercles, and those diseases by which it is generated.

Spirits and the stronger acids evaporate speedily, and are, to use vulgar language, not vehicles of cold; therefore it is that they obviate the implied effects of it, in stimulating or astringeing the exhalants and integuments of the head, when the hair is cut, or the head or other parts of the body become wet. That the muriatic acid, &c. contained in sea-water, thus stimulate or astringe, in a certain degree, the exhalants and skin, and thereby overcome the antagonizing agencies of their vehicle, cold water, and the consequent sudden and continued abstraction of calorific from the feet and legs, is in many cases very reasonable to believe; but the privation too long continued counteracts the salutary effects of the stimulating qualities of the acids, induces a change of action in the vessels, and disturbs the circulation.

Moreover, the state of the air of ships from damp decks opposes the cure of acute inflammatory diseases, by unremitting aggravations, and by counteracting the intended effects of a most efficient class of medicines, sudorifics. It is the bane of all chronic complaints, especially chronic rheumatism, the inseparable concomitant of old grog-drinking sailors. It is unfavourable to the cure of ulcers, and really inimical to the cure or palliation of tubercles, and the pulmonic inflammations by which, for the most part, it is produced.

It is judiciously ordered that partial draughts of pure atmospheric air be communicated to the lower decks and places of ships in the navy, and that this should at all times be attended to, even though the decks are not washed. This perhaps is the only practice which can be entirely depended on for the extrication of foul air, and therefore should, when practicable, supersede the use of fumigations, which, in my humble opinion, rather tend to conceal than to decompose or dispel it.

I cannot have done with this subject without introducing the following strange remarks, from the recent work of an excellent writer on that grand portion of the earth:

> " ——————— corusco
> Semper soli rubens et torrida semper ab igni;"

nor can I refrain from offering a doctrine different from his, after the liberty which he has expressly and courteously given in the Sabine strain, viz.

> " ——— Si quid novisti rectius istis,
> Candidus imperti: si non, his utere mecum."

"In

"In a ship of-the-line on the home station, I have been in gales of wind during the winter when the decks were floated with water for weeks together, without the possibility of drying them. I have listened night after night to these torrents rushing and roaring from one side of the lower deck to the other, as the ship rolled, while five or six hundred men in hammocks were suspended nightly over the moving current, in the most advantageous manner possible for feeling its influence. And what was the consequence? Why, that there was not a man on the sick list."

"But the scene changes;—the gales subside—the wind veers 'round to the eastward—we regain our station off the coast of France, where the sea is smooth, the air keen, and the decks perfectly dry. And what is the result? Why, that half the ship's company is laid up with pneumonias."

Taking the truth of both these remarks and replies for granted,—though I cannot but candidly confess they bear to my mind the easy implication of a certain figure of speech—might not this gentleman, with his usual sound reasoning, ascribe the former, should it actually happen, to that certain capability of resistance, for a limited time, in the powers of the healthy constitution to the prejudicial influence of wet and confinement? and the latter, should it actually follow, to that state of susceptibility induced by this circumstance? thereby particularly admitting the subsequent agencies implied in the changes of the scene, when the orthodox doctrine of the ingenious and learned Dr. Brown can be so easily referred to.

Here it is very plainly intimated, that off the coast of France, in winter, an easterly wind, though not strong, has alone been productive of pneumonia, although the previous circumstances of living for weeks together between wet decks, of blowing and wet weather, and of a change of wind and weather, are at the same time admitted. The east wind is notorious for its dispersion of moisture; and moisture followed by a wind which disperses it, has a particular influence on temperature. It is thus that the human constitution is acted upon by an easterly wind, and that the brute creation feels its influence. According to the laws of the animal œconomy, the system, from its reaction to the agencies implied in these antecedent circumstances, becomes predisposed, so as to be particularly susceptible of the subsequent agency of the keen easterly wind and of the change of temperature. A sharp easterly wind that has lasted any time, abstractedly considered, without any reference to the weather preceding it, or to the change itself, implies a comparative regularity of a low temperature, which, though it may from its arid qualities aggravate, cannot, in my humble opinion, be solely productive of pneumo-

 nia.

nia. I beg leave to refer the reader, who must judge for himself, to the text for the *præmissa et consequentiæ*.

For my own part, warm dogmas or positive assertions cannot produce in my mind any conviction, without, at least, some train of reasoning, or axiom of common experience by way of proof; and I cannot but subscribe my mite to exonerate "some naval surgeons," who have admitted the mischievous consequences of wet or damp decks, from the unjustifiable imputation of embarrassment, and the unbecoming implication of ignorance; since, in addition to what I have just explained, there is no one, either in civilized life, or in the least acquainted with the internal state of a ship, so inexperienced or void of sense, as not to know the very different effects arising from sleeping or remaining in a damp laundry and a dry comfortable apartment, or between damp decks or in a wet galley or manger, and the commodious and ventilated cabin of a commissioned officer or captain, independently of the occurrences of contagion in discipline or drunkenness. Besides, it is observable that laundresses, however accustomed to dampness, are still susceptible of its influence. They are seldom long livers; for, in general, they die of premature old age, either affected with chronic rheumatism, or affections of the pleura or lungs, produced and repeatedly aggravated by it; nor are their husbands, children, or inmates in winter, scarcely ever entirely free from the same complaints, in consequence of washed clothes being hung up to dry in the rooms in which they sleep or sit.

II. *Report from the Select Committee appointed to consider the Validity of the Doctrine of Contagion in the Plague.*

[Concluded from vol. liv. p. 439.]

Dr. William Pym.—WAS formerly an officer at the quarantine establishment at Malta. To be infected by the plague, contact or very near approach to the person under the disease is supposed to be necessary. Believes that it is independent of any disease of the atmosphere. Considers insulation by the means of quarantine, the most effectual step for preventing it. Knows one instance of the plague having been communicated at sea; some French gun-boats were taken by the Theseus man-of-war in the year 1800, they were ordered alongside, and while lying there, the person ordered on board to issue provisions, &c. received the infection of the plague. Supposing, as is the case in England, the quarantine regulations should have been established, and in force for 12 or 14 years, with care, would not the contagion

gion of the plague, if brought by persons or goods on board ships from foreign countries, show itself, if at all, in these quarantine establishments?—Considers the length of the voyage, and the care in opening the bales of goods, particularly their being opened in the open air, as one means of preventing the appearance of plague in this country. The lazarettos are certainly the most likely places for the plague to show itself.

Being asked to inform the Committee, on his own knowledge, what the usual practice of the expurgators is, in the personal performance of their duty, with respect to ships coming with foul bills of health?—answers, "The first step is, with respect to bales of cotton, to get a certain number of bales on deck in the importing ship; the bales are opened at one end, and a certain quantity of cotton drawn out, by the person employed pushing his hand and arm in as deep as he can (in this country they do it with their hands; at Venice and Marseilles, when cargoes are infected, they do it with iron hooks.) The bale remains in this state for three days, as well as I can recollect, when the other end is opened, and undergoes the same operation for three days longer; the bales are then removed into the lazaretto, where they are again opened at both ends, the cotton pulled out, and exposed to the air as much as possible for forty days." Is at present confidential adviser to the Privy Council in matters relating to quarantine. Was sent to Standgate Creek in 1813, and then thought the duty of the expurgators was done negligently. Supposes it is better done now; the officers have had their line of duty pointed out to them, and understand it better than they did formerly. Believes now that all goods, coming with foul bills of health, are exposed to ventilation; and that many cargoes have been embarked when no plague prevailed; but one accident of plague occurring before the sailing of the vessel, makes it necessary to have a foul bill of health. Considers the fever at Gibraltar to have been a much more serious disease than the plague; during the fever of 1804, when the population of Gibraltar was not 20,000, 6,000 persons died.

Sir James MacGregor.—Has been on the medical staff of the army twenty-six years. Has seen the plague in Egypt, and (as director-general of the army medical department) is in possession of the details of the plague in Malta and the Ionian Isles. Was in Egypt in 1801. Few cases of plague came under his own inspection. Was at the head of the medical department of the army which came from India to Egypt; it was his duty to apportion the attendance, and frame general arrangements. In this army 165 cases occurred. The two first cases that appeared were hospital servants. These two, and four other men that slept in the room, all proved fatal. It never could be traced

how

how the first patient caught it; but it was discovered that the plague existed then in Rosetta where hospitals were situate. The hospital in which the first cases of plague appeared, viz. that of the 88th regiment, was situate in Rosetta, and the plague was there at the time. Believes the plague to be contagious by contact; should have some doubts of a very close atmosphere, but has no direct evidence of that; could often trace it to contact. Should think most cases are by actual contact. Means actual contact both with the body, and with clothes infected with miasmata. Gives as his reason for considering it contagious, his having traced it clearly from one subject to another, on the first appearance of the disease in Egypt in 1801. As before stated, there were about 165 men affected in the army; immediately after the discovery of the plague, all the cases of it were sent to a pest house; the suspicious cases, those with symptoms of fever, were placed in observation rooms; and then the whole of the remainder of the 165 men were, without loss of time, removed to another hospital prepared for them. Before any man was removed to the new hospital, the strictest precautions were used; his hair being cut off, he was put into a bath, and the whole of his clothing left outside the hospital and destroyed; each man was then provided with new and fresh clothing. The consequence of these precautions having been rigidly enforced was, that after entering the new hospital, no new case of the disease appeared. Has known instances where contact with plague patients has taken place, but no disorder has occurred; on the first appearance of the disease in the months of September, October, November, and December, when the disease was virulent, knew of very few cases where the disease did not follow contact; but afterwards, when the most rigid precautions were taken, and towards the end of the season, and when the disease was in a mild form, people went sometimes together with impunity; a case of plague has been detected, and people in the same tent have not had it. Of those afflicted with the plague soon after the first part of the season, none recovered; indeed the subjects of the first part of the season were mostly native troops, the Sepoys; and many unfavourable circumstances along with the virulence of the disease attended. The Indian army consisted of 7,886; of those 3,759 were Europeans, and 4,127 natives of India. There died of the plague, of Europeans 38, and of natives of India 127. The inhabitants of Egypt have an idea that the plague generally ceases on St John's day, the summer solstice. The general belief is, that the extremes of heat and cold arrest the plague. It subsides gradually; from perhaps February or March till June. It is a pretty general opinion that the disease is never entirely extinct in the country, but that it is called into action by causes which

which we are perhaps not very well acquainted with. Thinks it probable that the circumstance of no plague occurring in England may be owing to none having ever been imported; but there are many circumstances to be weighed, between the present and the time the plague prevailed in England; improvements in the structures of the houses and towns; cleanliness, ventilation, and police regulations, are enforced in a degree unknown in the 17th century. Medical science stands now on different grounds; and we have acquired a knowledge of the disease, its treatment, and prevention. In Egypt, as well as in Turkey, many existing circumstances tend to the propagation of contagion, and rendering it more virulent. Were typhus fever to appear in those countries, it is difficult to conceive how it ever could be eradicated. From the nature of fomites, considers the risk would be increased from having no quarantine. The risk in this country would be less, compared with a country where the intercourse was short and by land, with the country where the plague raged.

Dr. James Curry, Senior Physician to Guy's Hospital.—Considers plague as contagious, to a certainty; and for this reason, that all persons who are apprized of its contagious nature, may, by keeping apart from those persons who labour under the disease, or are suspected of labouring under the disease, be perfectly free from it, even at a very short distance from them; and probably, if they were to keep to the windward of them, they might even almost touch them with impunity. Thinks the plague attends, and is usually incidental to, a particular state of what is called by Sydenham and others, *constitution of atmosphere;* and the various changes which take place in the air at different times, appear to be produced by an interchange of electricity between the earth and atmosphere, which occasions that particular state of the human constitution which renders it liable to some one certain species of disorder, for the time it exerts its influence, and not to others. For instance, the small-pox will prevail under one state of atmosphere; the measles under another, and scarlatina under a third; and we scarcely ever find that two of those disorders prevail at the same time: each has a particular or appropriate state of atmosphere which especially favours it. Considers the cause of plague as two-fold; the plague is in itself a highly malignant fever, arising from a peculiar and very virulent morbid poison, generated by the bodies of the sick whilst labouring under the disease, and capable, when applied to the bodies of those who are in health, in sufficient dose or intensity, of exciting the same kind of fever in them: the spread and diffusion of the plague, however, seems to require the co-operation of a *malaria*—*malaria* produced by the state of the soil and the atmosphere

atmosphere operating upon each other: hence, at one time it may be particularly rife and violent, and at another time it may not appear at all. *Malaria* is the Italian word for a morbific vapour arising from the earth, under the influence of the sun's heat, and of the electric interchange already alluded to; it is found every season all along the coasts of the Mediterranean, but in a much greater degree in some places than in others: the essential cause of the plague is double; first, the specific poison, which is generated or multiplied in the bodies of those who labour under the disease, without which it cannot exist; and secondly, a state or condition of the atmosphere, which gives a strong tendency to support the disease among the people at large; it is virtually occasioned by a state of the atmosphere, and it is communicated by the infection, either in clothes, wearing apparel, or bed linen: animal matter, such as feathers, silk, or wool, will preserve it much more readily than any thing else. The contagious principle of the plague cannot be communicated the same way as that of small-pox, that is, by inoculation: if the matter be introduced into the system, it does not entirely introduce the plague, but it will introduce a disease which is nearly similar. Has never understood that there is any difference between that form of the plague which affects a person who has touched another afflicted by it, and that which afflicts those who receive it from the odour or vapour arising from a person who is so diseased. The same remark may be made with regard to the small-pox; the contagion of the small-pox is communicated in two ways; one is by the palpable mode of inoculation, or contact; and the other is from the gas or vapour arising from the infected person. It is the case with some diseases, that they cannot be propagated by contact—Does not say it is always so with respect to plague; but, for instance, measles, with all the experiments that have been made on the subject, never have been palpably communicated by inoculation. Each poison may be supposed to have a peculiar mode of existence, and a peculiar mode of propagation. The same case occurs with regard to the hooping-cough. The hooping-cough has never yet been communicated, except by the breath, or by coming near the person infected with it. Doubts whether a person removed from the place where he was taken ill of a fever, either in England or Ireland, and carried even to a short distance, would communicate it to anybody, even under the very worst state that he could have that fever. Gives the following reason for thinking so: At one time it was a great question, whether the yellow fever at Philadelphia was of domestic origin, or originated from importation; and after we began to take patients from Philadelphia to a place called Bush Hill,

not

not a single example took place of their communicating the disease to other persons. Considers the plague much less contagious from goods than from persons; because the clothes of a person affected with the plague become saturated with the poison in the person, and are much more liable to communicate the disorder than goods. Besides, it is scarcely to be supposed that bales of goods, coming from a country affected with the plague, would have been handled by persons who had the plague, because persons so affected would be incapacitated from labour. Believes, as to goods which have been imported from a country that is infected with the plague, that if they communicated it at all, (which he doubts) it would be from those goods having been extremely confined. It does not meet with a suitable soil or sun here. Is of opinion that the plague which prevailed in London in the year 1665 was more probably generated here, than imported. Is disposed to believe that every plague, even in the Levant, is oftener generated, than propagated by contagion. Thinks, if the plague were imported into England, it would affect but few; and the cases would be so insulated, that it would be very soon cut off. From the circumstance of Holland never having been at all affected with the plague, although admitting goods from the Levant without difficulty, concludes it would be the same with regard to this country, and with an equal immunity. Holland admits goods from the Levant, even without quarantine.

Being asked, Is there not some beneficial consequence arising from touching *terra firma;* is there no electrical effect?—answers, " Nothing but that which would occur equally on board of ship. The question, taken in its general bearing, would make rather against the supposition inferred by your question. Several years ago, when I was lecturing on the subject of contagion and fever, and the influence of *malaria* arising from the land, the late Captain Pelly happened to be present at the lecture; and I was mentioning the fact, that in the West Indies, *cæteris paribus,* the inhabitants to the windward of the islands have not the fever so much as those persons who reside on the leeward side; because those persons who reside on the leeward side of the islands are necessarily exposed to the fever arising from the land which intervened between the windward side and the leeward side. In corroboration of that, he mentioned a very curious circumstance, which had been repeatedly observed by himself, and by many other officers who were in the Channel fleet, that as soon as they got out of sight of land, and had an entire sea breeze, the men and boys might go to sleep in the tops, and wake after an hour or two without any injury; but as soon as they come up the Channel, so as to receive the influence of a land wind from the English or French coast, they always wake with a cold upon them

if

if they have been so to sleep, or have a cold a short time afterwards; so that it would appear, that all land wind has something contagious attached to it, in a greater or less degree. Is of opinion that epidemic diseases, under any circumstances, may become contagious by a very near approach to the bodies of persons labouring under the disorder. The vapour arising from the bodies of persons infected, would produce the disorder readily and violently; but in all epidemics they arise in such a great many points at once, that it would be almost impossible to say that they were produced by contact with infected individuals. In a conversation about four years ago, with a very intelligent Spaniard, who was secretary to the embassy going from Spain to Sweden, he mentioned a very curious fact, with respect to the town of Medina Sidonia, which is about twelve miles from Cadiz. The first time it was affected with the contagion of the epidemic of Cadiz, only a few houses on one side of a street were affected; and the next season, when it occurred in Cadiz, it attacked the greater part of the town of Medina Sidonia; but it was insulated in the first instance.

Being asked, Whether he attributes that to a current of air? answers, "There are many circumstances which escape common observation, and which cannot be accounted for by many, that have yet an influence upon human health. It may be illustrated by a fact, which is commonly observable in this country, by the excessively offensive smell arising from our sewers on particular occasions, which has usually been attributed to the wind setting in a particular quarter, or blowing up a particular grating, and so up into our houses. But it appears to have no connexion with the *direction* of the wind, nor any *sensible* state of the atmosphere; but rather depends on some peculiar change that has taken place, apparently of an electric nature, between the earth and atmosphere. The point may be further illustrated by a variety of phænomena, that are constantly occurring before our eyes, but which are little attended to, and still less inquired after, as to their causes. Frequently during the summer in this country, you may observe a puff of warm air come against your face as you are going along, without any circumstance that can explain it, except that owing to some local cause acting upon such portion of the general atmosphere, as to render it sensibly *hotter* than the general mass: it is what is commonly called a *hot gleam*. This appears to be only a lesser degree of the same phænomenon which produces the sirocco of Sicily, the hot winds of India, the harmattan of the Coast of Guinea, the samiel winds of the Desert. You will also frequently observe, in a day when the air is perfectly still and calm, that in travelling along the road, or crossing a plain, like that of Salisbury for example, without a tree, a wall,

or

or a hedge being near, which could account for it, a whirl of leaves and dust will rise in the air, and travel from a few yards, perhaps to a quarter of a mile, and cease there; and then another perhaps will arise, and go on. These are only so many lesser degrees of what is known in other countries, especially North America, the West Indies, and still more the Eastern Archipelago, by the name of tornadoes and tiffoons; and they can only be explained satisfactorily by supposing, that a change has taken place in the relative electrical state of the earth and atmosphere, and occasions either a sudden and considerable condensation or expansion of the air over the particular spot, in consequence of which the circumambient air rushes in to fill up the void; and these whirls or tornadoes travel in various and even opposite directions at the same time, no doubt according to circumstances, which are dependent entirely upon some electric interchange which determines both the force and the extent of the motion.

Dr. Robert Tainsh.—Was Surgeon of the Theseus, when in the Mediterranean, at the siege of Acre. Had five cases of plague on board, three Englishmen and two Frenchmen. They all recovered, and there was no communication of the plague to any other persons on board the Theseus.

Mr. Edward Hayes.—Was born at Smyrna, and resided there nearly all his life. Has seen very frequent occurrences of the plague. The merchants always avoid it by shutting themselves up from communication with the town, and only communicating through purveyors, who are generally people who have had the plague themselves. Has often known instances of persons having communication with persons who were afflicted with the plague, without catching the infection themselves. Considers the plague to be absolutely contagious, that is to say, the atmosphere must be in a state when it will spread a great deal more than at other times. When an European dies of the plague, they generally destroy the clothes; that is, such as they cannot wash. But when the natives die of the plague, the clothes are sold again; that is, *until lately*. They now discover their folly, and begin to take the same precautions that they do with respect to Europeans' clothes; at least with regard to the higher order of Turks, but it is by no means a general custom. The greatest part of the Turks make no hesitation in putting on the garments of a person who has died of the plague, nor in sleeping in the same places; but that arises, in a great degree, from their being Predestinarians. Great cold or great heat generally destroys the plague in Smyrna. It is erroneously supposed that it must cease about the 24th of June, because the 24th of June is St. John's day; but it is quite absurd to suppose that, because it has been seen to cease sometimes before; and has continued sometimes as late as July or August,

August, when the great heats destroy it. The virulence of the disorder is almost invariably over by August. It begins in March or April. Goods are shipped promiscuously, whether the plague rages or not, to all parts of Europe. Thinks that goods shipped during the time the plague is raging, convey the contagion; and that every country that receives those goods, receives in a degree the contagion of the plague. Has repeatedly known cases to occur, from which he is satisfied in his own mind that the plague was introduced solely by goods, without contact.

Sir Robert Wilson, M.P.—While in Egypt with the army, saw many cases of plague, and gives the following as the result of his observations. The army that invaded Egypt was divided into two corps. One was stationed at Alexandria, and the other moved on against Cairo; a part of the army which remained stationary at Alexandria had a detachment at Aboukir, where the preceding year many thousand Turks had been put to death in consequence of a defeat in an action with the French, (and where several hundred British and French had recently been interred;) every precaution was taken to prevent the introduction of plague into that part of the army which blockaded Alexandria and was stationed at Aboukir, and from particular local circumstances all communication with the country was successfully intercepted, except under authorized regulations; notwithstanding which precautions, plague broke out three distinct times, beginning amongst the troops occupying Aboukir, and extending to those stationed before Alexandria. That part of the army, Turkish and British, which moved against Cairo, passed through the country where numerous villages were infected with the plague; and during the march the soldiers had constant communication with the inhabitants of those infected villages. At Menoof, where the plague had raged with the greatest violence, a bakery was necessarily established for the use of the army; but none of the persons who attended that bakery were infected with the plague. At Rahmanich there was a lazarette or plague hospital; several men were lying infected with the plague, and many were brought out already dead; others were dying in the environs of the town of the same disorder. The Turks stript the bodies of all, indiscriminately, of their clothing, and there was no restraint whatsoever in the communication with the inhabitants, who had also free access to the camps; yet no plague was communicated to the troops. The city of Cairo had lost a great many inhabitants the same year, by the plague. When the army arrived at Cairo and united with the Grand Vizier's army, many of the graves in which the inhabitants had been buried who had died of the plague, were opened, and the bodies stripped of their clothing, with which the Turks covered themselves, and yet no soldier of either the British or Turkish armies

armies was infected with the plague. The disorder ceased between the 17th and 24th of June, at the precise time when its cessation had been anticipated and assured by the inhabitants, except at Aboukir, where it continued to exist some time longer. It was also affirmed to us by the French officers, that although the plague had raged in Cairo that year with very great violence, and carried off some of the French army, yet notwithstanding a constant communication was held between the garrison stationed in the citadel and the inhabitants of the town, the soldiers in the citadel were not affected, in any one instance, with the disorder. Many thousands of the inhabitants of Lower Egypt had died that year of the plague. The Indian army, passing through Upper Egypt, had traversed a country in which about sixty thousand inhabitants were said to have perished; whole villages having been destroyed; but yet the troops of that army brought no infection with them, nor were any precautions adopted to prevent contagion on their junction with the British European army. To these circumstances Sir R. W. was an eye-witness. Wishes also to state, that as they moved through the country, the inhabitants pointed out to them particular villages that were infected with plague, and which plague did not extend out of those particular villages to any of the contiguous villages, although there was no precaution whatever used as to the communication with the inhabitants of the infected villages. Conversing with Dr. Desgenettes, the chief physician of the French army, and M. Assilini, the head surgeon of the French army, they assured Sir R. W., that whenever a battalion infected with the plague had been marched out of the infected place, the soldiers recovered, and never conveyed the infection to other garrisons; and that troops marching into that infected garrison which had been vacated, did not become themselves infected, unless they remained there longer than eight or ten days. And M. Assilini further assured Sir R. that several French officers and soldiers, who had the plague, having removed themselves, or been removed when sick of the plague, into other places, they had almost always recovered. But he said, his great difficulty was in persuading people to make the exertion of movement; for they were generally so enervated that they preferred to remain where they were and meet their fate. Thinks the plague is a fever originating in a particular state of the atmosphere, produced from local causes and confined to their influence. Should suppose it must arise from a putrid state of the atmosphere; because those villages which were infected, were stated by the inhabitants to be generally those where the mud had been left longest, and the moisture only, after considerable stagnation, had been absorbed. The Nile annually overflows all the inhabited land with a body of water four or five feet in depth, and charged with a very rich mud, which it deposits. After several months, when the Nile falls, the water, in a comparative pure state, is carried off the land by canals, which at a prescribed season are opened for that purpose. As the sun gains power, the moisture of the mud which has been left is absorbed, until the mud becomes quite dry and brittle; the absorption is so great that the ground is over all its surface broken by large fissures, sometimes three or four feet in depth, and which render the passage for horses extremely dangerous.

Charles Dalston Nevinson, M.D.—Is of opinion that the plague of 1665 did not originate in this country, and that no degree of cleanliness would prevent its being extended to the individuals of the country, if they were sufficiently exposed to the contagion. Considers all regulations which prevent the communication of persons infected with the plague with others, as useful, of whatever kind, whether performing quarantine on ship board; in floating lazarettos, or in lazarettos on shore. The plague not appearing in the lazarettos is no more a proof that it is not contagious, than that typhus is not contagious because it does not extend in the hospitals in this metropolis, where every precaution is used by ventilation and cleanliness to prevent its propagation. In St. George's hospital he never knew an instance of its communication for nearly twenty-six years, and has certainly seen some of the worst states of typhus in that hospital.

Richard Powell, M.D.—Thinks the plague is contagious. Deduces this opinion from the recorded facts upon the subject, which establish it to his mind as strongly as any fact with which we are acquainted.

Edward Ash, M.D.—Has travelled a good deal, and endeavoured to collect all the information he could upon the subject of the plague in travelling on the continent. Is of opinion, that the plague is decidedly contagious; that it is propagated from body to body; and from infected articles of merchandize, goods or clothes, to human bodies. This opinion is formed upon several facts. One was the immunity from plague which was preserved in the Foundling House at Moscow, which was free from the disease while the population of the town was perishing around, by drawing a cordon of troops around the building, which was completely insulated, and making the most strict quarantine regulations; by means of which none of the inmates of the hospital perished. Another principal fact was, the effect of shutting up or wholly insulating the Frank quarter, during the prevalence of the plague in Constantinople, Aleppo, Smyrna, and all the other towns which are exposed to ravages of the disease; a measure the success of which in preventing the spread of plague, renders it impossible to suppose that the plague should be communicated

nicated by the air only, and renders it highly probable that contact is necessary to spread the disease.

James Frank, M.D.—Has been in countries where the plague existed. Thinks it is highly contagious. Was upon the expedition with Sir Ralph Abercrombie in the year 1800, and had the first establishment of the plague hospital at Aboukir. The army landed in the month of March, and was perfectly free from the disease till about the middle of May; when a person from the commissariat depôt at Aboukir was reported to be ill, and it was said that he had the plague; he was removed from thence to the hospital, which might be the distance of about a mile, and confined in a tent by himself; he had been ill about four-and-twenty hours before at the depôt, and it was supposed that he was intoxicated, and that his disease arose from excess; he died. On the second or third day, two more persons, from the same depôt, in a day or two afterwards, were reported to be sick in the same manner, and they were sent to the same place. It was doubtful whether it was the disease; and then it became a question how it had got to the depôt. There were two reports; one, that it was imported by a Greek boat from Cyprus with Cyprus wine; and the other, that it was brought from Rhamaneh by the Arabs, where it was known the plague had been raging. It was afterwards said that a man had died on board this Greek boat. The impression upon Dr. F.'s mind is, that it was brought by this Greek boat; the disease had been at Cyprus and upon the coast of Syria; and Brigadier General Koëhler, and the detachment of artillery under his command, had fallen victims to the disease, and died at Jaffa three months previous. As before stated, he had the superintendance of the plague hospital at Aboukir, and consequently the arrangement for the accommodation of the plague patients, though not the immediate charge of the sick; for it had spread from the attendants, who had caught the disease from those persons above mentioned who had died, to the sick of the hospital. His whole arrangements were made, upon the principle of its being contagious, to prevent its spreading to the army, which was in the lines before Alexandria, at a distance of ten or twelve miles from the depôt. The hospital at that time consisted of the wounded men after the actions of the 8th, the 13th, and the 21st of March. There were very few sick, who were placed in some rude huts, which the French had built; for there was no house whatever at Aboukir, the village having been destroyed; and among those sick the plague patients were sent, it having appeared that the disease had been communicated to them by the attendants upon the first plague patients; and as it was impossible to say who had got the plague or who had not, Dr. F. recommended that the whole should be guarded by sentinels, and that the whole should be

 throw

thrown into quarantine: this was effected. The disease increased in the huts at Aboukir; and the medical officers, and the servants attending upon those patients, were almost all seized with the disease, and several of them died; three or four hospital mates died. From these facts deduces his opinion that it is a contagious disease; for had it not been so, there would have been an equal number of patients, or probably more, among the wounded than there were in the huts; the wounded men, who were in tents and in temporary buildings, being separated from the huts at least half a mile, or probably nearer a mile. These precautions having been taken, of throwing the whole of the plague patients, as soon as circumstances would admit, under quarantine, kept, he thinks, the army upon the lines before Alexandria very free from the disease; believes there were very few, if any, cases of plague sent from thence to Aboukir. The plague was almost entirely confined to Aboukir; there could have been but very few, if any, of the army before Alexandria who had the plague, during the whole of the time that the plague hospital was at Aboukir. If a person infected with the plague on board any vessel in the Mediterranean, was coming to Great Britain, should suppose that he would either die or be cured upon the passage, if the voyage was very long; if he lived, he would probably arrive without the seeds of the disease about him. Considers that it would not be possible to introduce a person from the Mediterranean, infected with the plague, into our quarantine establishments, after a long voyage. Thinks it probable that infected goods will retain the infection, unless exposed to the air; and therefore whenever they are exposed to the air, the persons handling them are liable to infection if certain circumstances are favourable to bringing out the contagion. Believes the plague in the year 1665 was the true Levant plague.

Richard Harrison, M.D.—Was at Naples in 1816 and 1817: during part of that time the plague raged severely at Noja, a town situated about 150 miles from that place. Saw, also, at Naples, in the spring and summer of 1817, several persons labouring under a contagious disease, which corresponded with the descriptions generally given of the plague; but will not assert it to have been that disorder. Supposes the cause of the plague to be contagion. Considers the quarantine establishments as very useful; but should conceive, from what he has seen in the quarantine establishments here, at Venice, Naples, Malta, and other ports of the Mediterranean, that considerable modifications might take place.

John Mitchell, M.D.—Being asked to what cause or causes he would attribute the freedom of England from the plague,—answers,

swers, "The diseases of a place are in their nature, even although they go under the same name, as subject to variations, as are the manners, customs, and circumstances of its inhabitants. At one time in this country we had well-marked agueish and asthenic or inflammatory complaints; now we have few instances, at least in London, of pure agues, and our inflammatory complaints degenerate into asthenic congestions or defluxions. We have had a change from nervous to bilious ailments, and this not founded on the caprice of medical systems, but in the nature of the complaints themselves. In Sydenham's time, whose works are full of the descriptions of the epidemic fevers of London, dependent on particular constitutions of the atmosphere in various years, it was computed that 66,000 out of the 100,000 died in London of fevers. This large proportion of fevers is now supplanted by other diseases; and even our fevers are not of the same complexion they were in those days, for we are strangers to the symptoms in them denoting their former pestilential or malignant quality. But certainly, if any causes could have contributed to the immunity we enjoy from the plague and bad fevers, they are to be found in the greater cleanliness and less crowded state of the inhabitants, with the widening of the streets, and the better and more general construction of common sewers and drains; to which may be added the profusion of water now distributed through the metropolis. There is a passage in Assalini, en eminent surgeon, who accompanied the French in their expedition to Egypt, and who has published the result of his investigations on the plague, very much in point. After stating that the present visitations of the plague in Egypt were unknown in the days of its ancient grandeur; and that the ruins of entire cities destroyed and overwhelmed, with the majestic remains of monuments, in part submerged and surrounded by water, afforded sufficient evidence of the revolutions and changes which the whole surface of Lower Egypt had undergone; he proceeds thus: 'At this day, the lakes, the marshes, and the filthiness which one finds in the cities of Lower Egypt, are the principal causes of the frequent diseases to which they are subject, and which can never be eradicated until we have found means to purify the atmosphere of their environs. This important advantage may be obtained, by draining off the waters of the lakes and filling them up; by keeping the cities clean, paving them; and giving a free exit to the rain water, which stagnating in different parts of these cities, becomes corrupted, and, conjoined with filths, infects the atmosphere. By similar operations, several cities and provinces in Europe, America, and the Indies, have been rendered healthy. I have no doubt that the salubrity which we enjoy at this day in France and Italy, is the result of the amelioration of agriculture and the perfection of the arts.'—As Dr. Mead (who has written on the plague, although he never saw it, has mainly contributed, by his authority, to establish the quarantine regulations of this country,) speaks in a particular manner of Grand Cairo, stating it to be quite a seminary for the plague; begs leave to solicit attention to the state of that city, as illustrative of the causes of plague. The streets are narrow and winding, amid a multitude of houses which crowd one another; and it is so exceedingly populous, (several families residing in one house, and a number of people in each room,) that Savary informs us, two hundred persons live within a compass that would accommodate but thirty in Paris. A great canal, besides, passes through it, into which all manner of filth and carrion is thrown, causing an intolerable stench. Notwithstanding all this, however, it is remarkable that the plague, as well in this city as in others of the same part of the globe, has its chief seasons; for it breaks out or becomes epidemic only when the hot sultry winds from the south, blowing across the sandy plains and deserts of Arabia and Africa, begin to set in. The blasts of these winds are most pernicious to animal life; according as they are described by travellers, under the names of the Simoon, the Samiel, the Kampsin, and the Scirocco. It would seem that they contained a large portion of hydrogen or inflammable gas, and that in their full force (for the greater levity of this gas, it is probable, causes them to rush in currents,) they extinguish every principle of irritability in the living fibre: persons killed by them, speedily become black and spotted, from extravasated blood. But to return: it is a fact, that so soon as these winds cease to prevail, (and it may here be mentioned that the same winds have been with justice considered, by the most early writers, the chief causes of epidemic diseases, particularly by Hippocrates and Pliny; who say, that the pestilence travels with them from the southern to the western parts of the world,) the disease abates. When, again, the winds from the north-eastern quarters, called by Hippocrates the Etesian, or annual gales, set in, the season of health returns. Conformable to these facts, Assalini states, 'I constantly observed, that whenever the winds from the south and south-west prevailed, the number of sick and of deaths was always increased. The contrary happened in fine weather, and when the wind came from the north.' As the rising of the Nile happens about the same time these healthy winds begin to blow, covering the muddy and slimy surface of its banks, and washing away all the filth which had been accumulated in the stagnant waters and canals, with which it communicates, the cessation of the plague in many places of Egypt, is calculated almost to a day. Now, if to the filthy and dirty state of the cities in Egypt and the Levant, the pestilential

lential effects of the regular returns of these enervating winds may be attributed, it is very easy to comprehend how London, as Sydenham gives us to understand, in a like crowded and narrow and dirty state of the streets without drains, such as it was previous to the great fire, should now and then have been subject, in particular constitutions of the atmosphere, to visitations of the plague. Is inclined to think the plague is of an analogous nature to fever, and dependent on the same general causes, only that the causes are influenced by the climate and atmosphere of the place, and by the manners and circumstances of its inhabitants. It is the conclusion of a very sensible writer on the plague, who appears to have well studied his subject, that the miasmata, which in Germany and England produced tertians, in Hungary pestilential fevers, in Italy remittents, in Syria and Egypt seem to occasion the plague. Does not believe the plague is a contagious disease, nor fevers in general.

Being asked to furnish the Committee with some of the early accounts as to the plague—answers, "The first I shall beg leave to mention is given by Alexander Benedictus, who tells us, that in consequence of shaking a feather-bed, which had been thrown aside in the corner of a house seven years before, a plague was raised at Wrateslaw, which carried off 5,900 people in twelve weeks. The same author gives an instance of the effects of the pestilential contagion, which had been shut up in a rag for fourteen years. Hieronymus Fracastorius and Forestus say, that about 1511, when the Germans were at Verona, twenty-five soldiers died, one after another, from putting on an old leathern coat; and that, ere the cause was discovered, 10,000 persons perished. The coat was then, very prudently, burnt. Another author, Victor Trincavellius, relates, that at Justinoples, in Italy, some cords which had been made use of in burying the dead of a former plague, twenty years before, infected the person who found them behind a box, and caused the death of 10,000 people. Our countryman, Dr. Mead, relates, on the authority of Sir Theodore Mayerne, that some clothes, fouled with blood and matter from plague sores, being lodged between matting and the walls of a house in Paris, gave the plague, several years after, to a workman who took them out, which presently spread through the city. The same author discovered that the plague of London, 1665, was carried to Poole, in Dorsetshire, in a pedlar's pack, and to Eham, in the Peak of Derbyshire, by a tailor's box; but he leaves us in the dark as to the manner by which it was conveyed to other places in England, over a great part of which he says it spread. Ingram, who writes on the plague, gives an historical account of it, in respect to foreign countries, from 1346 to 1665. I would only, however, beg leave to give the history of of its invasions, for the first three years of this period; when, observing a direction from about south-east to north-west, it travelled through China, India, Syria, Turkey, Greece, Egypt, Africa, Sicily, Italy, Pisa, Genoa, Savoy, Provence, Catalonia, Castile, Germany, Hungary, Flanders, Denmark and England. The bare recital of this carries with it the impossibility of accounting for the introduction of this scourge into so many places of the habitable globe, on any other principles than those of atmospheric influence. Even the notion of flights of birds transporting the contagion in their plumage, would obtain less credit, than a similar one, that its introduction into the houses of the Franks during their seclusion, may be caused by some importunate cat escaping from the cage where she had been confined, bringing it back in her tail.

Dr. Russell states, that it first appeared in England in the seaport towns of Dorsetshire; thence passed into Devonshire and Somersetshire as far as Bristol; and though the Gloucestershire people cut off all communication with that city, yet at length it reached Gloucester, Oxford and London. This was the great plague which happened in Edward the Third's time; and we have it from history, that whilst Edward, and his rival Philip of France, were thinning the inhabitants of either country by their sanguinary conflicts, pestilence carried off one-fourth of the inhabitants of the western world. London, as might be expected in those days, suffered the full force of its raging violence, for in one year there were above 50,000 buried in Charter-house churchyard. There is not a shadow of proof of the *importation* of the plague into London in 1665, as is said, from Holland, where it is stated to have been brought by a bale of cotton from Turkey. It seems a bare assertion of Dr. Hodges. This same Dr. Hodges tells us, that the first instances of it occurred in Westminster, where about the end of December 1664 two or three people died suddenly in one family. Other accounts state its first appearance to have been in like crowded and unhealthy places, viz. St. Giles and Clare-market; all of which, I may observe, are places remote from the Custom-house or quays, near which, on the supposition of its importation, it should rather have broken out. If, however, it did appear in December 1664, it must have slept a good deal; for by the bills of mortality only four died of it, from its very first appearance till the second week of May 1665. It was in June it began to spread, sometimes being in one part of the town, and sometimes in another. It reached its height in September, and in December it very suddenly subsided. As to that of Marseilles again, the king's physicians, sent expressly to investigate the cause of the plague there, broadly deny that it came by Chataud's vessel. But the accounts themselves of this

vessel

vessel bringing it, carry on their face strong marks of improbability. In the first place, there was a clean bill of health from the place the ship took in her cargo. She left Sidon, on the coast of Syria, the 31st January 1720, and did not arrive at Marseilles till the 25th of May, making a long voyage of four months. In the second place, admitting the plague was on board of this ship, how happened it, that after three of her crew died at Leghorn, none of the rest should have fallen sick till two days after her arrival at Marseilles, when one of the sailors died? And it is here proper to observe, that although she put into Leghorn, where these men died, she did not carry the disease there; on the contrary, the physician and surgeon of the lazaretto there, after inspection, granted a certificate that these men died of a malignant fever, the consequence of bad provisions, a circumstance very likely to occur from the length and badness of the voyage. In the third place, how happened it, that a cabin boy, who must all along have been with the ship, did not die till some time after a quarantine officer, who had been put on board on her arrival at Marseilles, died? The quarantine officer died on the 12th of June, and the cabin boy on the 23d. But besides this ship of Chataud's, the contagionists of those days were obliged to have recourse to other ships for bringing the contagion; viz. a Captain Gabriel's arriving on the 13th of June, and a Captain Ailland's arriving on the 23d from Sidon. Both these ships, however, having brought foul bills, were placed under strict quarantine. Last of all, they are obliged to give countenance to an idea, that the crews of all the different vessels smuggled small parcels of their goods, in order to account for the disease appearing in different quarters of the city, particularly in the Rue de l'Escale, a filthy part of Marseilles, like our St. Giles. There chances, however, to be a gratuitous admission on the part of the medical attendants of the lazaretto, that a bad fever had been previously prevailing; and Monsieur Didier, the physician, gives some cases which occurred even before the arrival of Chataud's vessel, bearing every resemblance to the plague, inasmuch as parotids and carbuncles were amongst the symptoms.

Asked whether authors on the plague specify any period after exposure or contact, that the disease seizes—anwers, "We have some wonderful accounts of the subtlety and instantaneous effects of the plague contagion. Thus Boccacio, in his description of the plague at Florence in 1348, relates that he saw, with his own eyes, two hogs instantly fall into convulsions, and die in less than two hours, after snuffling about with their snouts, and gnawing some pieces of bread that had been thrown out of the house of a poor man dead of the plague. Forestus tells us of a young man seized with the disease, only by thrusting his hand into an

old

old trunk, wherein was a spider's web, which in an instant raised a plague sore. There is a like story in Van Swieten, of an apothecary who was seized with a blister and carbuncle on the leg, only from kicking up some straw in which his servant, ill of the plague, had lain eight months before. The carbuncle took a long time to heal, but he received no other injury with respect to his health. Another person was seized with the plague, only from holding a bit of thread. A woman of Zealand removed into Almeria in Germany, having exposed some clothes to the sun, some children playing on them, received the infection, and all died. A man dropped down dead of the plague, by standing on a Turkey carpet. A lady, by smelling at a Turkey handkerchief, died of the plague on the spot. It is needless to observe, that such stories (and a long string more of them might be added) are by no means consonant with the general laws of the animal œconomy. But they receive a direct contradiction from Russell himself. Amongst the many thousands he saw ill of plague, he says he never met with an instance where the person was sensible of the stroke of contagion at the time. He thinks he has seen examples after some hours, as well as after two, three or more days. He does not think he met with any longer than ten. As there are like traditions about people struck down and dying in opening bales of goods in the lazarettos, I would beg leave to give Assalini's report thereon. 'It has often been said, that in breaking open a letter, or on opening a bale of cotton, containing the germ of the plague, men have been struck down and killed by the pestilential vapours. I have never been able to meet with a single eye-witness of this fact, notwithstanding the inquiries which I have made in the lazarettos of Marseilles, of Toulon, of Genoa, Spezia, Livournia, Malta, and in the Levant; all agree in repeating that they have heard of such an occurrence, but that they have never seen it happen. Among those whom I have interrogated about this fact, I may name Citizen Martin, captain of the lazaretto of Marseilles, who for thirty years past has held that situation. This brave and respectable man told me, that during that time he had seen opened and emptied some millions of bales of cotton, silk, fur, feathers, and other goods, coming from several places where the plague raged, without having ever seen a single accident of the kind.'

Charles MacLean, M.D. again examined.—To the reasons which he has before assigned, for considering epidemic and pestilential diseases as never depending upon contagion; begs to add the following: 1. Generally, because the laws of epidemic and those of contagious diseases are not only different, but incompatible; and because pestilences observe exclusively the laws of epidemics, of which they are but the higher degrees. 2. Because

cause no adequate proof has ever, in any single instance, been adduced of the existence of contagion in pestilence. From its first promulgation to the present day, the doctrine has been nothing more than a series of gratuitous assumptions. 3. Because had pestilential diseases been contagious, consequences must have followed, which have not taken place. Being capable of affecting the same persons repeatedly, they would never cease, where no precautions are employed (and in such case no precautions could avail,) until communities were extinguished: Turkey would long ago have been a desert. 4. Because phænomena now take place, which, if pestilence were contagious, could not happen. Instead of the laws of epidemic, they would observe only those of contagious diseases. 5. Because a superabundance of irrefragable proof has been adduced, showing that pestilence never arises from contagion; and because the assumption resorted to, in order to elude this proof, that 'to the effect of contagion, a particular state of the atmosphere is necessary to produce the disease,' is only, in other words, an acknowledgement that a particular state of the atmosphere is its real cause. 6. Because, for centuries before any intercourse, direct or indirect, was established between this country and the Levant, or rather as far back as history extends, pestilence was at least as frequent in England, as in the 16th and 17th centuries, when our commercial intercourse with Turkey was considerable. 7. Because, when the free states of Italy traded both with the Levant, and with the north of Europe; when they were the carriers, not only of the merchandise, but of the troops of the principal powers of Christendom engaged in the crusades; and when they possessed Smyrna, Cyprus, Candia, Scio, Cephalonia, Caffa, and even Pera, a suburb of Constantinople, no apprehension was then entertained, under a constant intercourse, of pestilence being propagated by infection, nor any precautions adopted by any nation for the prevention of such a calamity. 8. Because, during the century and a half which has elapsed since 1665, and in which there has been no plague in England, our commerce and intercourse with the Levant have been more extensive, and more rapid than at any former period. 9. Because there is no reason to believe, that in modern times, pestilences have undergone any revolution, in respect either to their nature or to other causes, further than may depend upon the advancement or retrogradation of countries respectively, in cultivation, civilization, and the arts of life; or upon an alteration in the seasons. 10. Because, as contagion, where it does exist, is sufficiently palpable, (it did not require the evidence of inoculation to show that small-pox depends always upon that source, and never upon any other,) if it were the cause of pestilence, its existence could not, for thousands of years, have remained

remained concealed. It must have been discovered, and demonstrated to the satisfaction of the world, by the ancient physicians; and could not now have been a subject of controversy among their successors. 11. Because no person has at any period of history been known to arrive in England, from the Levant, labouring under pestilence. 12. Because no person employed in purifying goods in the lazarettos of England or of Malta, has ever been known to be affected with pestilence, which could not have happened if contagion had existed in the goods; and because such goods could not be uniformly exempt from contagion in particular countries, if that were the cause of plague. If in other countries, expurgators of goods in lazarettos have been known to be affected, it must have been from other causes. 13. Because, after three hundred thousand deaths from plague have happened in one season, in Grand Cairo, two hundred thousand in Constantinople, and one hundred thousand in Smyrna, as we are told has repeatedly occurred in those places, and the clothes of the dead have been worn by their surviving relatives, or sold in the bazaars, and worn by the purchasers, the disease, instead of spreading wider and wider, as would inevitably have happened, if contagion were its cause, (since in that case it could not fail to be carried in the clothes,) has, on the contrary, regularly declined and ceased at the usual periods. 14. Because, in those countries in which the plague is supposed to be introduced by means of contagion, conveyed by travellers or goods, as Egypt, Asia Minor and Syria, it never occurs epidemically, but at particular seasons; although in the other seasons, travellers and goods from places in which the disease prevails, continue equally to arrive. And because in other countries, as Persia, which maintain a similar uninterrupted intercourse with places liable to frequent attacks of plague, that disease never occurs.

Documents in the Appendix.

(Copy.)

Sir, The Hague, April 3, 1819.

In compliance with Lord Castlereagh's desire, communicated to me in your letter of the 16th ult. I have made inquiry respecting the quarantine regulations established in the United Netherlands, and find that the following is the system now practised in this regard:

No vessels whatever of any nation arriving in these ports are subjected to quarantine, excepting such alone as come from the coast of Barbary. These latter are immediately visited, and carefully inspected by a medical person, who reports thereon to the Marine Department at the Hague, which department determines, from the nature of the report, to what extent quarantine shall be

enforced

enforced in the particular case. The vessel in the interim remains under the surveillance of a guard-ship.

I have obtained from the minister of marine printed copies of the several laws and ordinances of the kingdom, relating to this subject; but as these have been delivered to me in the Dutch language (they being not extant in any other), I have taken measures to get translations thereof, which I shall transmit to the Foreign Office. This, however, cannot be immediately effected, as they are rather voluminous.

The minister of marine has likewise informed me, that a commission has been appointed by the king of the Netherlands, to inquire into the subject of the quarantine laws, to report thereon, and to suggest any further regulations that may be considered necessary. I have, &c.

J. Planta, Esq. &c. &c. (Signed) J. GAMBIER.

(Copy.)

Sir,—The Commissioners having received an order from the Select Committee of the Honourable the House of Commons, respecting the contagion of the plague, dated the 11th instant, directing that there be laid before that Committee,

" An Account of all cases of absolute Plague in any Lazaretto in this Kingdom, from 1619 to the present time;"

I have it in command to transmit to you an Abstract of the Reports received from the collectors and comptrollers of this revenue at the quarantine ports on this subject; and to signify the request of this Board, that the Lords Commissioners of His Majesty's Treasury will, agreeably to their Lordships' standing order, be pleased to be the means of the same being presented to the before-mentioned Committee.

I am, sir, your most obedient
and very humble servant,
D. CURLING,
in the Secretary's absence.

Custom-House, London,
March 29, 1819.

S. R. Lushington, Esq. &c. &c.

Enclosure.

Abstract of the Reports received from the Collectors and Comptrollers at Rochester, Portsmouth, Falmouth, Milford, Bristol, Liverpool and Hull, in return to the Board's Order of Inquiry, on an Order of the Select Committee of the Honourable House of Commons respecting the Contagion of the Plague.

ROCHESTER.—The books and records at this port do not go further back than the year 1716. A proclamation relative to quarantine appears to have been issued on the 25th August 1720, and the first regular quarantine establishment appointed at that time,

time, although in a letter in 1722 allusion is made to the former quarantine in 1709, when sheds were erected for airing goods at Hoo Fort. In the year 1721, permission was granted to air goods on the decks of the importing ships, or in hired craft; and this practice seems to have continued until the year 1755, when the first floating lazaretto was established at Standgate Creek.

There is not any record of a case of absolute plague in any lazaretto at this port having occurred, from the earliest period that can be traced to the present time; but the following cases of strong suspicion, as to the cargoes of the ships being contagious, appear to have occurred:—

In 1721, the ships "Turkey Merchant," and "Bristol," with their cargoes, were taken from Standgate Creek out to sea, and burnt, in pursuance of an order in council, dated the 28th July 1721.

In 1792, a chest of goods burnt, imported in the "St. George," from Zante.

In 1800, the ships "Aurora," "Mentor," and "Lark," from Mogadore, were destroyed, with their cargoes, pursuant to an order in council of the 7th January 1800 (grounded upon a representation of the Committee, consisting of His Majesty's physician and others), great suspicion being entertained of the same being infected with the plague. The master of the "Lark" died at Mogadore, where the disease was raging at the time the vessels sailed; and it was reported that nearly all the persons who assisted in loading the ships, also died of the plague.

In August 1814, a large quantity of hare skins, imported in the "Lucy," from Smyrna, were burnt by order of Dr. Pym, upon a report made to the Lords of the Privy Council, from the Consul at Smyrna, that the persons who were employed in removing and packing the said skins, had died of the plague.

Portsmouth.—It cannot be ascertained, that any case of absolute plague has ever occurred at this port, on board any lazaretto. No regular lazaretto was appointed until the year 1805, previous to which time it was the usage to hire vessels for the airing of goods.

Falmouth.—The officers at this port are not aware that any case of what is usually called plague, has occurred; but have stated, that a disease, highly contagious, has frequently occurred there, and been arrested by precautionary means.

Milford.—No case of absolute plague has occurred at this port. The first lazaretto established here, was in the year 1806; previous to which it was the practice to hire vessels to air goods on board, subject to quarantine.

Bristol.—No instance is on record of any case of absolute plague having occurred at this port, from 1619 to the present time. Lazarettos were first established, pursuant to the act of the

the 26th Geo. II. cap. 6; prior to the passing of whch act, no information can be obtained as to what regulations were adopted for the due performance of quarantine.

Liverpool.—The officers at this port have not any knowledge of the plague having had existence in any lazaretto, or other vessel there. The first regular lazaretto was appointed in the year 1815; and previous to that time, and for about forty years before, it was the practice to hire vessels to air enumerated goods on board, and prior to that period, such goods were aired on board the importing vessels.

Hull.—The officers at this port cannot find recorded in their books, a case of absolute plague in any lazaretto, during the last 200 years.

Hired lazarettos were first employed at this port in the year 1774; before which time it was the usage to employ labourers on board the vessels placed under quarantine, in airing their cargoes.

IV. *On the Third Edition of Professor* Jameson's *System of Mineralogy. By* P. J. Brown, *Esq.*

To Mr. Tilloch.

Sir,—Having derived much pleasure from the study of mineralogy, I looked forward with some anxiety for the promised third edition of Professor Jameson's System; indulging the hope that five years experience since the publication of the second, would have weaned him, in some degree, from his implicit attachment to the external system of Werner; and that reflection, aided by the extensive opportunities of study which he cannot fail to possess, would have produced a work worthy of the science and of himself.

On receiving the third edition my expectations were high, when I saw it stated in the title page that the minerals were arranged according to the "Natural History method."—As nothing can be less natural than attending altogether to the external appearances of minerals, without any reference to the nature of their constituent parts, (considering how many adventitious circumstances may and do give rise to considerable variations in their exterior aspects,) I could not but suppose that the "natural history method" consisted in assigning to each individual a place in that situation where its relation to the neighbouring species entitled it to a claim*.

* This principle is the foundation of the natural methods in zoology and botany; and Sir James Smith most sensibly observes, that it is by the test of Jussieu's arrangement that the validity of any genus in the latter science must be proved. It is true that for the purposes of general study, more particularly with those who have not opportunity of entering very deeply

Let us now ask, What is the most evidently natural mode of arranging the subjects of the mineral kingdom? Is it in forming orders from the various combinations of the most obviously distinct substances? or, Does it consist in collecting from various quarters, those minerals which possess an agreement in some external characters, however they may differ from each other in more material points; separating them without remorse from species which nature has established as their nearest kindred? According to the first method, what can be more natural than forming an order from each particular metal, commencing with it in its pure state, and following it through its several combinations with oxygen, sulphur, and the different acids, in every one of which the metal will still form the most characteristic ingredient? —What can be more unnatural than the second plan of huddling together a most heterogeneous collection of species, merely because they have the same colour, the same kind of lustre, or are nearly of the same weight; without ever inquiring of what they consist?

Since the earliest attempts to reduce the mineral kingdom into order, it has almost invariably been the practice to divide it into four principal sections, or classes; viz. the earthy, metallic, saline, and inflammable bodies: the two latter being very few, it follows that nearly the whole science is divided between the two former; which might be expected to have some obvious and considerable distinctive characters. How are they separated by Professor Jameson?

Class 1st. Earthy minerals, tasteless; specific gravity above 1·8.

Class 3d. Metalliferous minerals, tasteless; sp. grav. above 1·8.

What a wonderfully scientific division!

Following the external system, Jameson arranges under his class of earthy minerals, above 50 kinds which had hitherto been considered as metallic ores; and of which above 40 do not appear to have one particle of any kind of earth in their composition!—Can this be scientific? and of the few which yielded a portion of earth, on analysis, several did not contain 4 per cent. which most probably was accidentally derived from the matrix.

We will state as an example the copper ores as arranged in the second edition of the Professor's system; and show how they are distributed in the third, in order to judge whether the science advances or retrogrades under his direction: premising that the coppers are selected as being one of the most numerous, and not by any means in consequence of their having been more particularly disjointed.

into the investigation of botany, the sexual system of Linnæus affords facilities which could not be derived from any other extant; but it is invariably distinguished by the appellation of the "artificial system," in consequence of its occasioning the separation of many genera naturally very nearly allied.

Sp.

2d Edition.	Composition.					
Sp. Native copper	Pure copper.				The only species preserved under the head of copper.	
	Copper.	*Iron.*	*Sulphur.*			
Copper glance	78·05	2·25	18·50	Silex 0·75	Transferred to glance.	These are collections from all the various metallic minerals; glance and pyrites being sulphurets—the ores oxides.
Variegated copper	69·5	7·5	19·	Oxygen 4		
Copper pyrites	30·	53·	12·		Ditto to pyrites.	
White copper	40· with iron, sulphur, and arsenic.					
Gray do.	19·2	51·	14·1	Arsenic 15·7		
Black do.	37·75	3·25	28·	Antimony 22.		
Copper black	"said to be an oxide of copper."				Ditto to ores.	
Red copper	88·5	Oxygen 11·5				
Tile ore	No analysis.					
		Carb. acid.		*Water.*		
Azure copper	56·	24·	14·	6·	Ditto to malachite.	Forming part of the Class of *Earthy* Minerals.
Velvet do.	No analysis.					
Malachite	58·	18·	12·5	11·5		
Brown copper	(oxide 60·75)	16·7		Silex 2·1; Oxide iron 19·5		
Copper green	40·	7·	10·	Silex 26·; Water 17·		
Ironshot do. do.	No analysis.					
	Oxide of copper.					
Emerald copper	28·57	Carb. of lime	42·85	Silex 28·57		
Muriate of do.	73·	Muriatic acid	10·1	Water 16·9		
Phosphate of do.	68·13	Phosphoric acid	30·95			
Copper mica	39·	Arsenic acid	43·	Water 17·	Ditto to mica.	
Lenticular ore	49·	Ditto	14·	Ditto 35·	Ditto to malachite.	
Oliven ore	60·	Ditto	39·7			
Martial arseniate of copper	No analysis.					

Now,

Now, in the name of common sense, what business can these latter have amongst the *earths?* What business can the sulphate, molybdate, chromate, phosphate, carbonate, muriate, and arseniate of lead; together with the carbonates of zinc, manganese, and iron, have amongst the *earths* jumbled into an order with the combinations of barytes and strontian, and the tungstate of lime? while other combinations of the same metals are transferred to the various divisions of ores, pyrites, glance, blendes, &c.—We used formerly to hear a great deal about the dismemberment of Turkey: but I trust no plan of dismemberment was ever actually carried into execution with such a vengeance as this.

However inconsistent the book may be with nature and reason, we might at least expect it to be consistent with itself. Is it so?—The essential characters of the order "Spar," and without which I should presume nothing could be a spar, are "no metallic or adamantine lustre—streak white—hardness 3·5 (*i. e.* half-way between calcareous spar and fluor) to 7 (*i. e.* equal to quartz)—specific gravity 2· to 3·7; if 2·4 and less, it is not amorphous." Independently of the inimitable improvement of introducing *negative* characters into natural history, are the above requisites complied with? Have we not in the order "Spar," bronzite, schiller spar, hypersthene and anthophyllite with a metallic-pearly lustre—common hornblende, hornblende slate, schiller spar and hypersthene with greenish-gray streaks—rock cork, amianthus, common asbestus, and rock wood, very soft—rock cork with a specific gravity of 0·679 to 0·991, and mealy zeolite "sometimes so light as nearly to swim on water?" Is not the latter, with its low specific gravity, "massive, reniform, coralloidal, sometimes forming a crust over the other subspecies of zeolite, and sometimes disposed in delicate fibrous concretions?" And are these amongst the scientific forms which exempt a mineral from being considered amorphous? Have we not, thrust into the middle of the order "Spar," a collection of clays, &c. not possessing the essential characters? with an admission that they have no real business there; but that "on account of their affinity with some members of the felspar genus," they are as well there as any where else?

It is observed that heavy spar is known "from all other earthy minerals by its great specific gravity." So it is in every former rational system: yet in the face of this observation the Professor includes in the very order of which heavy spar forms a part, *eleven* of *his* earthy minerals, whose specific gravities far exceed that of the substance in question, many of them being *above half as high again;* and in the other new earthy orders will be found about as many more.—Aye, but we could not separate these

these from their dearest friends on such trifling grounds. Why not, most worthy sir? You have felt no such compunction on other occasions: and give me leave to add, that if you establish characters to which you do not adhere, they are good for nothing*. We are told, in the preface, that the arrangement "is totally independent of any aid from chemistry;" and again, that "the chemical characters and composition of simple minerals are not employed in arranging and determining the species." Yet, at the close of many of the articles we have, under the head of "Observations," some of the most valuable information contained in the work; consisting in a great measure of such discriminating characters as will enable us to distinguish the preceding species from those to which it bears the most perfect resemblance: these differences are in many cases external, but very frequently depend on the effect of the blow-pipe—sometimes on the action of acids: thus, this perfectly independent system, although it will not suffer chemical phænomena to make part of the essential characters of a mineral, resorts indirectly to chemistry for light, where its own vague and indeterminate language would only lead you into a labyrinth, and leave you in the dark. One great objection to the external system is, that, by allowing the characters of each individual to occupy nearly or quite a page, and by introducing shadowy distinctions, which, so far from being understood from mere description, can scarcely be comprehended when pointed out by an instructor, it banishes all hope of being able to carry any useful degree of information in the memory; and leaves the determination of species to instinct, or tact. On reference to the book, the mind becomes bewildered with the indefinite definitions.—Look at fibrous brown zinc blende: there is not a character but what is included in the more diffuse account of fibrous brown iron ore. How shall we discriminate? The lustre of the latter is "intermediate between pearly and resinous." But we are told that the former differs from it "principally by its resinous lustre, *and its accompanying minerals.*" Antimony ochre also "nearly resembles bismuth glance in its external characters, but is readily distinguished from it *by its accompanying minerals.*" This is most sublimely scientific; to determine a doubtful point, not by the examination of the mineral itself, but by

* No one could regret more than Linnæus that the essential characters in his sexual system made it necessary for him to place the *Labiatæ* with two stamens in the class *Diandria* and the *Papilionaceæ* with distinct stamens in *Decandria*: but he saw too well the puerility of forming characters to be adhered to only so long as was convenient: had he thought as loosely of essential characters as Professor Jameson, the plants referred to would have been placed without hesitation in *Didynamia* and *Diadelphia*. There is a very wide difference between a man of real science and a mere system builder.

observing what are its associates! I presume, when the Professor adopted this mode of instruction, he bore in mind the old adage: "Tell me your company, and I'll tell you what you are."

Should this "natural history method" gain a footing in other branches of science, what a scene of confusion would ensue! We must no longer seek for the zebra in the genus *Equus*, but expect to find him associated with a striped tom-cat. What a glorious field for ingenuity would be afforded by the particoloured tribe of parrots! The derangement of the single genus *Psittacus* would open the gates of the temple of immortality. Pursuing the plan of beginning at the wrong end, I shall not despair of seeing some aspiring naturalist reject the teeth, and form an arrangement of the *Mammalia* according to their tails. We should then have animals with bushy tails—with tufted tails—with sleek tails—with curly tails—with stumpy tails—with no tails. How admirable! to clap a Chinese pig and a Dutch pug cheek by jowl, just like sulphate of lead and carbonate of barytes. Poor fallen man! he being the most tail-less of all animals, must then be contented with the lowest place in the new system of external zoology, and become the connecting link between the higher and lower orders of animated beings. Imagine the Lord of the Creation extending his arms, not for the godlike purpose of succouring the distressed, but to connect the tail of a tadpole with the tentaculum of a polype.

On the whole, I must consider the publication of Professor Jameson's third edition, as having thrown a greater impediment in the way of the progress of mineralogy *as a science*, than would have arisen from the absolute loss of every thing which had been written on the subject. We should then have been left to find the way by our own ingenuity, without the risk of being misled through the guidance of a defective hand-post. And I feel no hesitation in expressing my full persuasion that a novice would become a more really scientific mineralogist, by a little attention to Aikin's Manual, which he may carry in his pocket, than he would by a laborious study of the three large volumes of which I have been speaking.

Every publication of this kind makes me more sensible of the value of Mr. Chenevix's excellent reflections on some mineralogical systems, which appeared in two of your former volumes. It would be of much service if those reflections were now published in a separate form, and the perusal strongly recommended to the public. The purchasers of Professor Jameson's third edition should be more particularly urged to give them a serious consideration; as they would then, on imbibing the poison, be at least in possession of the antidote.

Having

Having expressed my opinion of the misnomer in calling the Professor's a "natural history method," I would suggest the propriety, as it is so wholly dependent on externals, of having it known by the denomination of the *superficial* system.

I remain, sir,

Your most obedient servant,

Old Brompton, Jan. 7, 1820. P. J. Brown.

IV. *Remarks on an Article entitled "A few Facts relating to Gas Illumination," published in No.* 14 *of The Quarterly Journal. By* George Lowe, *Esq.*

To Mr. Tilloch.

Sir, — The occasion of my troubling you with the following remarks has arisen from the perusal of a most extraordinary fact-perverting paper, which found a place amongst the highly interesting pages of the Quarterly Journal for July last. The paper I allude to is styled "A few Facts relating to Gas Illumination." If by this title the writer meant it to be understood, that the facts were *very few* in number, which he intended his paper should contain; then it will on all hands be allowed to be well chosen. But if, seriously, the writer of it believed, and intended to make the scientific world believe, that the assertions it sets out with are "facts that are true," it appears to me, that charity to the writer, the cause of truth, the cause of gas illumination, and the credit of the pages of a scientific work, alike demand that a ray of light should be thrown upon the subject, by the reflections of some other pen.

To do this fully and fairly, we must take each objectionable sentence as it rises, and make such answers to it as the experience and practice of this part of the country will bear us testimony.

No one will more readily agree with the sentiments of the writer, in his opening paragraph, than myself;—that the producing of gas fit for illumination from coal, has justly been ranked amongst "the greatest benefits which the science and enterprise of this country have conferred on mankind." It is from a like sense of its importance, that I trust you will consider as praiseworthy the humblest endeavours of any individual who shall lend assistance to this national manufacture, which is yet in its infancy; by either removing, refuting, or even palliating, the grounds of complaint, which, if too highly coloured, might deter numbers of our cleverest men from entering the path of its experimental in-

 vestigation

vestigation and consequent improvement. But alas! how crest-fallen must the science and enterprise of this country appear in the eyes of other nations, upon reading the succeeding paragraph; which, if altogether true, we might fairly call the death-warrant of coal gas! for it says that "important as this discovery was, many defects and inconveniencies have arisen on its adoption: coals contain a large proportion of sulphur, which is volatilized with the gas, and it has hitherto been found impossible to purify it sufficiently for lighting close rooms. The suffocating smell and the property it has of tarnishing every thing metallic, exclude its use from dwelling-houses, on account of the injury it would do to our health, our furniture, books, pictures, plate, paint, &c." Bravo! bravo! Why, Mr. Editor, after this, what must our foreign relations think of our granting Acts of Parliament to almost all our large towns, to allow corporate bodies to vend this *aëriform pandemonium*, to the detriment of not only our health, but our unoffending furniture! What can they think, I say, but that through the medium of this said coal gas we have discovered a method of reducing our overgrown population, infinitely superior to any of the bright thoughts of Malthus or his followers! But to be serious;—for a charge like this, against not only the "science and enterprise," but the philanthropy and genius of this country, demands our most serious attention;—and the question still presents itself, "Is it true?"

Fortunately, Mr. Editor, I have not far to wander for a confutation the most satisfactory. At this moment, my nose and eyes have as conclusive an evidence before them as any Argand lamp can produce, or man the most fastidious could desire, that it is by no means "impossible to purify coal gas sufficiently for lighting close rooms!" Not to insult the understandings of your readers by any endeavours to persuade them that either our health or our furniture are as yet uninjured; I will briefly state, that since the commencement of this year we have been enjoying the many advantages resulting from coal gas illumination; not only in our brewery, offices, and stables; but in our *dwelling-house*, consisting of entrance-hall, library, sitting-rooms, dining-room, laboratory, &c.: even our *bed-rooms* partake of the comfort it affords. The light by which I am now writing is furnished from a chandelier suspended from the centre of the ceiling by four chains, two of which partly screen the two tubes by which the gas is conveyed, the one leading to a ring of 16 jets which rise between heraldic leaves not unlike a coronet; and the other to an Argand of the largest size in the centre, which, for the sake of our eyes, is guarded by a sphere of ground glass. As to any the slightest degree of "suffocating smell" arising from

from the gas, How can there be? For when it tests $\frac{1}{10,000}$ part of sulphuretted hydrogen, the lime for purifying is changed. That this test is very possible as well as very easy, let me refer your readers to the evidence borne by Dr. Henry in his late valuable "Experiments on the Gas from Coal." When speaking of the method of purifying by lime, adopted by his friend Mr. Lee: "After the second purification," says he, "the gas produces no change whatever in the test, which preserves its perfect whiteness, thereby demonstrating the complete removal of sulphuretted hydrogen. In this state of purity its odour (even prior to combustion) also is so much diminished as scarcely to be at all offensive. Besides, can we for a moment believe that Mr. Lee would be at the trouble of conveying it in portable gasometers for "supplying his house, two miles from the manufactory," if it *was* so fraught with this *health-and-furniture-spoiling* property? Again: the ceilings of our rooms, too, are just as white as before the introduction of the gas; and the slight, though equally diffused, heat arising from it, is considered an acquisition, rather than a source of accusation. This at once furnishes us with a ready answer to the latter clause of the paragraph we have been examining; where it is stated that free "hydrogen is also formed and mixed with it in a large proportion, which on being ignited occasions great heat without adding to the light: these effects render coal gas unpleasant in sitting-rooms, and have nearly confined its use to open shops and street lamps." Have *nearly confined!* In the first part of the paragraph we are given to understand that it is altogether so confined; and that its innumerable and insuperably bad qualities "exclude its use from dwelling-houses!!" How much more edifying it would have been, had the writer favoured us with more experiments and fewer assertions by ways of *proving* to us, that a gas (oil gas for instance) consuming twice as much oxygen during combustion, as another (coal gas) should yet give off *less heat!* His observations on coal gas illumination must surely have been confined to the streets of London, where (with all due deference to the metropolis) it is more disgraced than in any place where I have yet seen it; not by the manufacturers of it, but by its wasteful and unscientific consumers. The writer, if he is not *bodily* blind, may convince himself that in many large towns in the country it is not "excluded from dwelling-houses."

The first sentence of the next paragraph, respecting the stopping up and destroying of the smaller pipes, by the action of the sulphuretted hydrogen, is already answered, by showing that it is very possible as well as very easy to remove the cause. But the latter clause so obtrudes itself upon the eye, and so manifestly betrays the object of the whole paper, and the secret spring from which its objectionable passages have arisen; that we have only to quote the author's own words, to inform us, that, being himself either an interested or an enthusiastic admirer of oil gas, he is determined to add fictitious brilliancy to its brightness from the shorn beams of poor coal gas. "The employing," he says, "of coal instead of oil for the purpose of illumination, has an injurious effect on one of the most important branches of trade a maritime country can possess; and in proportion as coal gas is used, our fisheries are injured." We may truly call this a far-fetched objection to bear down the merits of coal gas. However, let us examine the validity of the objection as it is intended to apply to us as a *maritime nation.* What reduction in the price of either oil or candles has taken place, in consequence of the present introduction of coal gas? Scarcely any, notwithstanding the almost unexampled imports of foreign tallow: and we have certainly yet to learn, that the perilous Greenland fisheries are to be compared, as a nursery for our fleets, with our Newcastle coal-trade, our coasting trade, and the Newfoundland fisheries. The fact is, that where the Greenland fisheries rear one sailor, the Newcastle coal trade, the coasting trade, and the Newfoundland fisheries rear fifty.

Let us now proceed to his statement of "the œconomy of light from various sources."—This however we shall find is rather difficult to understand, owing to the extraordinary discrepancies that it presents, and to the prime cost of oil not being mentioned even. We are merely told that one gallon of whale oil "will yield 90 cubic feet of gas." Mr. De Ville, we are told, is inclined to average it at 80 cubic feet. But for even reckoning we will suppose that a gallon (which with us, from a very large dealer, costs three shillings,) produces 100 cubic feet. *Three shillings* then is the price of *one hundred cubic feet of oil gas,* exclusive of the fire under the retort.

Now, what is the cost price with us of its equivalent in coal gas? One ton of excellent soft coal, which answers to Dr. Thomson's cherry coal, costs, when laid down in our yard, nine shillings; but the small coal or cobbles answer very well, and cost only five shillings a ton*. This I am aware is very cheap in comparison to what it is in some counties: we will therefore begin with the double of it, and call the ton of coals ten shillings, or three-pence for half a hundred weight, which quantity will, in four hours time, readily furnish (if judiciously carbonized and

* Of this description is the refuse small coal which is usually left in the coal-pit, and which, it is evident, will be to the mutual advantage of the colliery and gas manufactories to bring into use.

the

the tar converted into gas) 200 cubic feet of excellent coal gas, and perfectly equivalent, if purified by dry lime, on the principle of the Exeter patent*, to 100 cubic feet of oil gas. By this method, which is very cleanly and simple in its application, "all the costly and offensive operation of purifying is turned into profit†!" But we have yet another product from this half-hundred weight of coal, (which, though "costly," we consider not offensive,) that of one bushel of excellent coke, which we can readily sell for fourpence, *i. e.* for a penny more than the original cost of the coal! This source of profit is not peculiar to ourselves; several large gas concerns are now obtaining it. The thing is now put in a tangible shape; and from the data given, any one may form a ratio of the comparative values of coal gas and oil gas, let their prime cost differ in whatever degree.

As to the next sentence, it contains an assertion so unqualified, and yet so circumstantially told, that a person unacquainted with the subject would immediately be led to believe it. But that we may not lose one word of this precious sentence, we will refer to the author's own words. They are these: "The oil gas has a material advantage over coal gas from its peculiar richness in olefiant gas, which renders so small a volume necessary, that one cubic foot of oil gas will be found to go as far as four of coal gas. This circumstance is of great importance, as it reduces in the same proportion the size of the gasometers which are necessary to contain it; this is not only a great saving of expense in the construction, but is a material convenience where room is limited."

* This method of purifying is very superior to any method I have yet seen. More lime is certainly required. But after it has served this purpose it is in a useable state to lay upon land; and being saturated with the very principles of manure, (ammonia, sulphuretted hydrogen and carbon,) it is evidently worth more to the agriculturist, than when in its simple state of quick-lime.

† It seems as if the legislature were determined to make all public gas establishments adopt this method of purifying. The clause now generally introduced into the Act of Parliament, preventing the carrying of any sough or drain or washings into the brook or stream which carries off the filth and nuisances of the whole town, through which it runs, is no less fatal to the method of purifying by *lime mud*, than it is severe upon the establishment, and prejudicial to the public.—Mark the inconsistency. By a prior clause they (the Gas Establishment) are allowed to make as many sewers, tanks, and drains as they please; and yet are not allowed to let these drains run into the general drain or brook of the place! What must be the consequence? And indeed, what is now actually the case? That the washing water must find it escape by filtration to the surrounding neighbourhood, to the irreparable injury of the wells and cellars in that neighbourhood! Would it not be acting more liberally and fairly, to allow the gas manufacturers the same privileges as all other manufacturers are allowed; and merely subject them to a penalty on being convicted before two justices of the peace for suffering an unabated nuisance?

One cubic foot of oil gas go as far as four of coal gas!! In what a most extraordinary point of view must the writer of the above have been contemplating his favourite oil gas! However, we must look to him for the PROOF, as well as to explain the following discrepance, That " Mr. De Ville of the Strand, who has made many important experiments upon oil gas illumination, compares oil gas to coal gas as 9 to 5—not as 4 to 1, as just stated; and that, as a proof, a 10-holed Argand burner will consume $2\frac{1}{2}$ cubic feet of oil gas per hour: yet at page 314 we are told that $1\frac{1}{2}$ is sufficient! Now an 18-holed Argand of coal gas is never allowed more than five, and often only four, cubic feet per hour, which is very near Mr. De Ville's proportion! To satisfy ourselves that this last statement is much nearer the truth, and that the random proportion of 4 to 1 is a something given upon hearsay, and not the result of either theory or practice, we have only to consider the two gases theoretically.

Query? What would be the proportion of light given off during the combustion of 100 measures of oil gas and 100 of coal gas, supposing the former to be all olefiant gas, and the latter all light carburetted hydrogen? Professor Brande says that " the fitness of gas for illumination will be directly as its specific gravity." Dr. Henry, in his late experiments to ascertain the degrees of combustibility of the gases, recommends " the finding experimentally the proportion of oxygen gas required for their saturation, and of carbonic acid formed."

The following table will therefore show at one view, that by either of these methods *olefiant gas* can but possess *double* the illuminating power of either light carburetted hydrogen or of coal gas.

TABLE*.

100 Measures of	Atomic Constitution	Specific Gravity	Requires of Oxygen	Produces of Carbonic Acid
Olefiant gas . . .	1 carb + 1 hyd.	0·974	300 meas.	200 measures.
Carburetted hydrogen	1 do. + 2 do.	0·555	200 do.	100 do.
Cannel coal gas	. . . .	. .	212 do.	111 do.
Clifton do. do.	. . .	. .	151 do.	$79\frac{1}{2}$ do.

Let

* The above computations are taken from Dr. Henry's late experiments, the coal gas being in each instance the average of the first seven hours' product. It would have been desirable had Dr. H. given more particulars as to the species and character of the Clifton coal, whether hard or soft; for with us we have but little hard coal that is at all suited to the purpose of illumination. He merely calls it " common coal of fair average quality." But however fair the average may be with respect to Manchester, it appears to

Let us now, from the supposed constitution of oil gas and coal gas, take a view of their actual composition; and we shall find that, instead of their relative values being as 4 to 1, in reality they will seldom be even as 2 to 1 *. And for this evident reason, that oil gas is seldom or ever (as we have been presuming) free from light carburetted hydrogen, and likewise that the gas from coal is never free from olefiant gas. In proportion, therefore, as these two gases are made, by a good or bad method, to approximate, in the same proportion will their relative values approximate. It is therefore evident, that those individuals or towns that shall be led by this mis-statement to erect gas-holders, or to lay pipes for oil gas in the reduced ratio to those of coal gas of 1 to 4, will to their inevitable disappointment soon discover the error; and it will be found that the proportion stated as Mr. De Ville's, of 5 to 9, is the nearest the truth.

With regard to the composition of oil gas, Dr. Henry states in his paper lately read before the Manchester Society, that in his experiments upon the decomposition of oil, he always obtained a mixture of the two gases; but that M. Berthollet had succeeded in obtaining from oil [query, animal or vegetable] pure olefiant gas. From this it is evident, that on the same fortuitous circumstances of temperature, pressure, and extent of heated surfaces, as mainly depends the generating of olefiant gas from oil as from coal. If further proof were wanting as to the scientific principle, as well as to the benefit resulting from the passing of crude gas (either

to be far short of being so in comparison with ours. For he says that "3500 cubic feet of gas was collected from 1120 pounds of cannel coal; and 3000 cubic feet from the same quantity of the Clifton coal." Either of these products is far, very far short of Brande's experiments (or of the every day's practice about us). In some experiments at the Royal Institution he obtained 26 cubic feet from four pounds of coal. This shows how much depends on the method of carbonization, and leads one to suspect, that the gas on which Dr. H. made his experiments was not generated on the now generally adopted principle, of applying the coal in thin strata, in either semicircular or elliptical retorts, (to say nothing of the still further improvement of the heated medium before condensation,) and that the retorts be charged every four or five hours, as recommended by Mr. Peckston in his excellent book upon Gas Lighting; and not every twelve hours, as seems to have been the case in Dr. Henry's experiments. We, in our small establishment, greatly prefer four hours charges; finding thereby we have more light and less tar from a given weight of coal. Though our coal is rich in bitumen, the tar produced seldom exceeds one-fourth of the condensible fluids.

* See Brande's Manual of Chemistry, page 156. We are there informed (speaking of oil gas) that "from two to three cubic feet may be regarded as equivalent to five or six of coal gas." This was published in April; yet in July we are gravely informed that one of oil gas is equivalent to four of coal gas!!

oil

oil or coal) through the *heated medium before condensation* *, certainly we have that proof now before us in the essay in question, which admits the very existence of oil gas to depend upon it.

The following paragraph your readers will no doubt think with me is very objectionable, in as much as it does not agree with the venerable proverb of " Honour to whom honour, merit to whom merit is due." It states that " about two years since it occurred to Messrs. John and Philip Taylor, that it might be practicable to construct an apparatus capable of converting oil into gas." " In theory the project appeared easy," &c. I am sure the ingenious Messrs. Taylors, who move in the highest circle of civil-engineering, would have been more ingenuous and civil, than to have thus passed by the prior labours of Dr. Henry, who, not only *two* years ago, but, *fourteen* years ago, laid before the public his "Experiments on the Gases obtained by the destructive Distillation of Wood, Peat, Pit-coal, Oil, Wax, &c. with a view to the Theory of their Combustion, when employed as Sources of artificial Light." The first memoir appeared in Nicholson's Journal for June 1805. The second in the Transactions of the Royal Society for 1808 ! ! In reply to this I shall only quote Nelson's motto, " Palmam qui meruit ferat;" and say, that though fifty

* I certainly feel no small degree of pleasure to think that the pages of The Philosophical Magazine for April and November 1818 should bear the prior record of this my favourite system, which for two years I have been making almost numberless experiments upon ; and indeed, the more I see of its action, both on the large and small scale, the more I am convinced that it must prevail and become general. Those who still doubt that by it a much greater per centage of olefiant gas as well as carburetted hydrogen is produced from a given quantity of coal, will do well to visit the gas establishments at either Bath or Cheltenham, the brilliancy and purity of the gas at which places are becoming quite proverbial. Mr. Manby of the Horseley Iron Works was, I believe, the engineer employed in each of the above-mentioned towns, and with whom the idea of the heated medium occurred at much about the same time as with Mr. Parker of Liverpool and myself. Mr. M. informs me that at Bath, eight retorts *with the heated medium* elicit more and better gas, than twelve retorts did prior to its adoption, the retorts in each instance being charged alike! I mention this, in order to show that it must have been from the want of heated surfaces, that Dr. Thomson in his truly valuable " Experiments to determine the composition of the different species of pit-coal," did not succeed in obtaining good gas from *coal tar*. He says that the gas obtained " yielded too little light to make it worth while to prosecute such experiments further." Whereas in my own apparatus we daily decompose the tar, and produce a light from it equal in brilliancy to any oil gas ! To the truth of this many scientific friends can bear witness, the retort being so constructed as to serve for the decomposition of either coal, coal-tar, oil, tallow, or kitchen grease ; on the former of which numbers of analytical experiments enable me to state, that there are yet many curious phænomena to be developed, besides those which I published in No. 252 of The Philosophical Magazine.

patents

patents should be taken out for oil gas apparatus, yet is Dr. Henry the prior inventor both of the theory and practice*.

This is much of a piece with the preceding attempt to make the world believe that there is no such thing as a coal gas apparatus upon a small scale for private individuals, and that all public companies are monopolists. He says, in fact, that coal gas is "confined to public companies or large establishments, thereby materially limiting its utility, and producing an injurious monopoly." What! if, like the town of Derby, the company, who are holders of single shares of 50*l.* restrict themselves by their Act of Parliament to 10 per cent. profit! Let them make use of oil instead of coal, and I am sure that they need not pray Parliament for any such restriction.

One other statement allow me to reply to, and then I will release your readers from these futile objections against coal gas. We are told that "the light of an Argand burner of coal gas, compared with one of spermaceti oil, may be estimated as $2\frac{1}{2}$ to 1; and of oil gas to coal gas as 9 to 5."

Or if we state it thus:

Sperm. oil	: Coal gas	: : 1	: $2\frac{1}{2}$	
Coal gas	: Oil gas	: : 1	: $1\frac{4}{5}$	Therefore,
Sperm. oil	: Oil gas	: : 1	: $2\frac{1}{2} \times 1\frac{4}{5}$, or as	
		1	: $4\frac{1}{2}$	

Hence, oil gas is $4\frac{1}{2}$ times brighter than spermaceti oil when burning in an Argand lamp! "Credat Judæus!" If it were so, I should as soon think of blinding myself by using, as a source of light, the combustion of iron wire in oxygen, or that of charcoal by the galvanic battery.

In conclusion:—Mr. Editor, allow me to assure you, that in thus standing up as the champion of coal gas illumination, I do it not from any other motive than that of promoting the cause of Truth. Patiently have I waited months in the hope of seeing her cause maintained by some of your correspondents more practised with "the grey goose-wing" than myself. Neither am I actuated by any feelings of prejudice against oil gas, for I admire its brilliancy where I cannot see its œconomy. It is the want of this spirit of liberality which I condemn in the essay before us, as much as the distorted aspect in which it endeavours to present coal gas. That the writer may have often met with bad coal gas I do not doubt: but to say that it has hitherto been found "impossible to purify it sufficiently for lighting dwelling-houses," is much the same as to say that it is high time to abolish all high

* Having lately seen a description of a patent portable gas lamp, I conceive it due to the memory of the late Dr. Bilsborrow of this town, to state that not fewer than twelve years ago he showed me some chemical experiments by the light of one which he made himself.

pressure

pressure steam-engines, because an ill contrived one, or one ill managed, has blown up. To argue against the use of an object from its abuse, is the logic of past times, and like the statement of "facts that are not true."

Believe me, sir,
Your attentive reader,

Derby Brewery, Dec. 12, 1819. GEO. LOWE.

V. *On the Comet of July—September* 1819. *By* A CORRESPONDENT.

To Mr. Tilloch.

SIR, — THERE are, I am inclined to think, among your readers, a considerable number of persons, not professed astronomers, to whom the appearance of a comet is an object of considerable interest: and I must confess my surprise that no one among them has favoured you with his observations on the beautiful one by which we were lately visited.

This circumstance must plead my excuse for troubling you with the following particulars: and I shall be most highly gratified if my humble attempts should incite some one possessed of superior abilities and means to enter further into the subject.

It was formerly a practice with the editors of scientific miscellanies, to publish representations of the kind now submitted to your notice; and as the path of the late comet is remarkable for its deviation from a great circle, I have thought it a favourable opportunity for recurring to this custom.—(See Plate I.)

The accompanying sketch was originally made from the joint observations of myself and a friend; and on comparing it with those from Greenwich, and with nearly fifty* made at the Parisian Observatory, I have the satisfaction to find that it does not require any sensible correction.

The region traversed by the comet is perhaps as barren as could have occurred, there being not a single star greater than the fifth magnitude, if we except two of the fourth, in the fore foot of *Ursa Major*, one of which is seen to the left of the plate. The stars are laid down in it from Bode's large catalogue, brought

* Extending from 3d July to the end of August, and recently published in *Conn. des Tems* for 1822. Among my memoranda, I find an extract from the Public Ledger of July 28th, containing an observation said to have been made at the Observatory Gosport, July 21, at $11^h\ 59^m 15^s$, on the meridian; R.A. $8^h\ 0^m\ 45^s{\cdot}34$ ($=120°\ 11'\ 20''$). Decl. $51°\ 8'\ 30''$, corrected for refraction, &c. a determination differing *most unaccountably* from all others both in right ascension and declination.

up

up to August 1, 1819. And as in the course of observation I was obliged to resort to Lalande's *Histoire Céleste*, I have inserted in the plate twelve additional stars from that work, (all of them near the comet's path,) and have distinguished them by Roman letters; viz.

Letter.	Magnitude.	Histoire Cél. Fr.		Approx. Posit. Aug. 1, 1819.	
		Page.	Transit.	Rt. Ascen.	Declination.
			h m s	° ′	° ′
C	5	383	7 2 8·5	106 11·8	49 46·4
D	7·8	——	7 9 43·5	108 5·1	48 53·3
	7	377	7 10 20·5		
E	7	——	7 13 22	108 50	48 16·7
F	7	564	7 24 24·4	111 24·2	50 55·8
G	7·8	——	7 31 14·5	113 6·8	51 27·0
H	8	——	7 54 18·5	118 52·6	51 49·8
B	7·8	——	8 17 33·7	124 40	51 14
I	9	383	8 33 24·5	129 0·2	50 5·7
K	9	——	8 35 23	129 29·7	50 0·4
L	8·9	——	8 37 16	129 58·2	50 36·9
M	8·9	——	8 47 5	132 14·0	50 26·5
N	8	——	8 47 49·3	132 35·0	50 39·1

Flamstead has three imperfect observations of the first star C, which is No. 396 of Miss Herschel's catalogue; and I have reason to believe it is the same with Hevelii δ *Lyncis;* as the longitude and latitude of the latter, given in the *Tables de Berlin*, correspond to it, while in Wollaston's catalogue the declination is six degrees greater, and perhaps may have arisen from a typographical error in the original.

The account given of the comet when first seen at Paris, is, that the nucleus appeared very luminous, pretty well defined, and of a shape inclining to oval. The tail might be about six or seven degrees in length.

The smaller compartment of the plate exhibits a projection of its orbit on the plane of the ecliptic, according to the subjoined elements, corrected by Bouvard, which represent the observed places of the comet to within 50″. *Perihelion passage* June 28, 2225, mean time at Paris*, *i. e.* June 27, at $17^h\ 11^m$ at Greenwich; *perihelion distance* 0·341008; *longitude* 287° 5′ 54″; *longitude* of ascending *node* 273° 42′ 52″; *inclination* of orbit 80° 44′ 44″; motion *direct.*

* The French astronomical time is reckoned from midnight, the English from noon; there is consequently a difference between them of $12^h\ 9^m\ 21^s$.

The

The figure may be shortly explained, thus; NC the orbit of the comet projected on the plane of the ecliptic, with dots representing its place at intervals of 5, 10, 20, 30, &c. days from the perihelion; N n, the line of the nodes; P p, that of the apsis; S, the sun; the orbit of the earth shown by a portion of a circle, with dots corresponding to the same epochs as those on the comet's orbit; the dotted lines joining the two orbits point out clearly the progressive increase of the comet's geocentric distance and longitude. The dotted line N*P′C′* shows the parabolic curve on its own plane, whence the perpendicular heights of the comet above the plane of the ecliptic may be found, by diminishing the ordinates of the curve in the proportion of radius to sin. inclin. *i. e.* 77 to 76 nearly.

The subjoined table gives the distances, &c. from the time of passing the node, to 80 days from perihelion, or upwards of 141° of anomaly.

1819. Mean time.	Days from Perihelion.	Comet's Anomaly.	Logarithm. rad. vector.	Distance in millions of miles*. From Sun.	From Earth.
d h m		° ′			
June 25, 18 52½	1·92965	346 37·0	0·00594	32½	63
27, 17 11	0	0 0·0	0·00000	32	64½
July 2, 17 11	5	33 3·1	0·03664	35	73½
7, 17 11	10	57 59·0	0·11629	42	90
17, 17 11	20	86 36·7	0·27609	60½	118
27, 17 11	30	101 27·1	0·39715	80	140½
Aug. 6, 17 11	40	110 32·5	0·48870	98½	158
16, 17 11	50	116 46·5	0·56105	116½	171
26, 17 11	60	121 22·7	0·62042	133½	183
Sept. 5, 17 11	70	124 57·5	0·67057	150	189½
15, 17 11	80	127 50·6	0·71390	165½	195

With respect to the length of the tail, it is impossible to give any thing like an accurate statement, as different observers vary greatly in their estimations; but if we assume the angular measure already given, to be correct, the length thence deduced will be about 8½ millions of miles.

I am, &c.

Lewes, Jan, 1, 1820. *ΑΣΤΡΟΦΙΛΟΣ.*

* The mean distance of Earth from Sun being assumed 93,768,000 miles.

VI. *Parti-*

VI. *Particulars of the Sword of Meteoric Iron presented by Mr.* Sowerby *to the Emperor* Alexander *of Russia.*

In June 1814 Mr. Sowerby had a sword-blade hammered at a low red heat, out of a slice of the nicoliferous iron from the South of Africa, and supposed to be of the same meteoric origin as those stony masses* which have so frequently been seen to fall through the atmosphere. This blade was welded into a steel haft, and mounted in steel; it was intended to present it to the Emperor of Russia while His Imperial Majesty was in England; but various circumstances united to frustrate that design, and it was forwarded to the Minister of the Interior at St. Petersburgh. Here a fresh delay was experienced, in consequence of the disturbed state of Europe which kept the Emperor from that city. At length, however, it has been received; and a letter, part of which is copied below, was sent to Mr. Sowerby by the Minister of the Interior:

"Sir, ... Several circumstances have hindered for some time the presentation of your letter and sword to the Emperor."

"Now, I have the honour to inform you, sir, that His Imperial Majesty has been pleased to read your letter, and to accept your sword with particular benevolence; and, as a mark of his satisfaction, to make you a present of a ring adorned with diamonds and a large precious stone† in the middle of them."

"As Dr. Creighton, Physician to His Imperial Majesty, is going now to England, and is so good as to take upon him the delivery of this my letter and of the said ring to you, I send it herewith by him. I feel at this moment a double satisfaction, both for your zeal towards my gracious Sovereign, and for the reward you have so worthily merited by it," &c.

(Signed) "Kosodawlew.

"St. Petersburgh, May $\frac{16}{28}$, 1819."

The following is a copy of the letter above referred to.

"*To His Imperial Majesty Alexander Emperor of all the Russias.*

"May it please Your Imperial Majesty,

"Part of a mass of iron of the same nature and cœlestial ori-

* One of the first of these that excited the attention of philosophers fell in Yorkshire, and is now preserved with many others in Mr. Sowerby's museum of the natural productions of Great Britain. The fullest account of this stone is published in the second volume of British Mineralogy, with a figure. It was the sight of this stone that changed Mr. Cavendish's opinion, who had previously considered that the story of falling stones was ill founded.

† An emerald.

 gin

gin as that so celebrated which Professor Pallas discovered some years ago in Siberia, and which is now placed in Your Majesty's museum, being in my possession, I have presumed it would be agreeable to Your Majesty to accept a sword made from it, as a mark from an individual, of that gratitude every Englishman is so anxious to express, and of respect for the familiar way in which Your Majesty has been pleased to visit my country.

" May it please Your Majesty,

" The meteoric iron of which the blade has been hammered, was found about 200 miles within the Cape of Good Hope by Captain Barrow. It has been examined by my countryman Smithson Tennant, Esq. who established its nature by discovering about 10 per cent. of nickel in it. It is the only sword ever made of that rare and extraordinary material. That Your Majesty may be graciously pleased to honour an humble individual by receiving it, is the ambitious hope of

Your Majesty's most obedient
and ever grateful servant,

" July 3, 1814. " Jas. Sowerby."

Besides this letter, a paper accompanied the sword, of which a copy is subjoined.

" The descent of solid stones or masses of ductile iron from the sky, or from meteors, is so inconsistent with the familiar operations of nature, as to excite at first disbelief: but when the fact is established by incontrovertible evidence, wonder and reverence succeed. The ore of the mine or the diamond of the Indies are indebted for their value to their usefulness or rarity; their origin is too familiar to excite any extraordinary emotion. We are little surprised at meeting with hail or rain; but meteors strike the beholder with amazement, and stones or metal falling from them complete his astonishment.

" This is the only blade ever formed of unadulterated meteoric iron; a material that derives its value from its quality, which is superior to other iron, from its scarcity, and, above all, its extraordinary cœlestial origin, with the terrific phænomena attending its fall: a value no other substance possesses, and which renders it worthy to become a sacred pledge of national gratitude towards, and esteem for, the heroic " giver of blessings," who, in alliance with the magnanimous and lawful sovereigns of Europe, has generously fought for " peace alone," and whose arms the Almighty has favoured against the scourge of the earth, to chase away

away the horrors of ambitious warfare; of which gratitude Mr. Sowerby wishes to express his part as an Englishman.

"The immense mass of iron which lately blazed with such fury in America, the one discovered by Professor Pallas in Siberia, another which fell in Normandy, are all of them (at least such specimens as have been seen here) so full of earthy matter, pyrites, or flaws, as to render them totally unfit for hammering into any instrument, even of small dimensions. The only attempt of the kind on record was made in 1620, when Jehangire, Emperor of the Moguls, had four blades formed from the iron of lightning, as it was called; but the workmen found it necessary to add one-fourth of common iron to make it suit their purpose.

"That meteoric stones were revered by the ancients, we have many authorities for believing; and it is understood that the Psalmist alluded to them, when he said, 'The Highest gave his voice, hailstones and coals of fire.' And it is also understood that the first wonder of the world, the Temple of the Ephesians, dedicated to Diana, was built in reverence to one of them. And in the Acts the town clerk appears to mean a similar thing when he observes, 'What man is there that knoweth not how that the city of the Ephesians is a worshipper of that which fell down from Jupiter?' A stone which fell at Ensisheim was preserved in the church in the Emperor Maximilian's time. In short, every part of the globe has been visited by such phænomena at some period, and particularly Russia and Europe lately, where hundreds have witnessed their fiery descent.

"In 1805, Mr. Sowerby received part of the large mass of iron found upon the surface of the ground about 200 miles within the Cape of Good Hope by Captain Barrow. Another piece sent to Holland, is all that was brought away. The remainder was removed as far as the Table Mountain; but its situation is now unknown. It has been examined by Smithson Tennant, Esq. who found in it about 10 per cent. of nickel, which adds to its toughness, gives it a silvery lustre, and proves its analogy to the small grains of iron dispersed through the stones and the masses of iron which have been showered down from meteors in Russia, Great Britain, Benares, Normandy, &c. which is distinguished from all other iron of this globe, by its containing a portion of nickel alloyed with it.

"The blade has been hammered at a red heat, without admixture, out of a single piece of this iron, an inch thick, ground and polished. Its spring was given it by hammering when cold. The haft was lengthened by welding on a small piece of steel. It was found to work very pleasantly, the whole operation taking about ten hours. The mounting and engraving occupied the two following days. Thus no sword was ever completed from the crude material in so short a space of time.

"Mr. Sowerby under all these circumstances considers it likely to be revered by posterity, and hopes it will attract attention, and continually be a memorial of the grand example of the merciful Emperor."

The length of this sword is two feet: it is slightly curved, pointed, and sharpened at both edges to eight inches from the point: its width is 1 inch and 3-8ths. The part that is blunt at the back is nearly filled on one side with engraving: beneath the imperial crown is a wreath of laurel and palm inclosing the word "Mercy:" under this is the Russian spread eagle, and then the following inscription: "This iron having fallen from the heavens, was, upon his visit to England, presented to His Majesty Alexander Emperor of all the Russias, who has successfully joined in battle to spread the blessings of peace through Europe, by James Sowerby, F.L.S. G.S. Honorary Member of the Physical Society of Göttingen, &c. June 1814." On the other side, "Pure meteoric iron found near the Cape of Good Hope."

The surface is not quite free from blemish, in consequence of the spreading of some minute flaws in the material; but they are only superficial, and will serve to distinguish this blade from any imitation that might be made of it. It possesses an excellent spring, much hardness, considering it is not steel, that is, equal to an old Highland broad sword, and a whiter colour with a more silver-like lustre than other iron.

It is highly probable that the foliated structure of most of the other meteoric irons* will render them unfit for hammering out to such a length as is necessary for a sword blade, if they will bear hammering at all. Thus the blade above described is not likely soon to have a rival: it was fitted up in a black scabbard mounted with steel,—a material by no means generally used in Russia.

* An account of several of these will soon appear in No. 27 of Sowerby's Exotic Mineralogy, with figures showing this structure, from which, however, the Siberian is exempt.

VII. *Me-*

VII. *Memoir upon the Conversion of ligneous Bodies into Gum, Sugar, and a particular Acid, by means of Sulphuric Acid; and on the Conversion of the same ligneous Substance into Ulmin by Potash.* *By M.* HENRY BRACONNOT*.

[Read at the Royal Academical Society of Sciences of Nancy the 4th of November 1819.]

THE ancient chemists contented themselves with repeating, that the action of sulphuric acid concentrated on vegetable substances does no more than carbonize them. Among the moderns, M. Berthollet was of opinion that the hydrogen of the vegetable substance united itself to the oxygen of the sulphuric acid, and that, in thus forming of it water and sulphurous acid, the carbonous principle was separated and precipitated. Messrs. Fourcroy and Vauquelin endeavoured afterwards to throw some light on this phænomenon. They imagined that it did not disengage itself from the sulphurous acid; and they devised an ingenious theory to this effect, but which seems to me to rest on very slender grounds.

In examining the remarkable changes which organic compounds experience from the action of sulphuric acid, I have been led to results very different from those obtained by these illustrious chemists; and I flatter myself that the important facts which I have to present will contribute to cast a new light on many phænomena of vegetation, and may be usefully applied to the arts.

Action of Sulphuric Acid on Elm Saw-dust.

Twenty grammes (308·9 gr.) of elm saw-dust well dried, were moistened with cold sulphuric acid of commerce of the specific gravity 1·827, and the mixture stirred with a glass tube. It became very hot; and, according to the theory of M. Berthollet, the sulphurous acid gas disengaged itself with impetuosity; the ashes became black, and appeared to be in the state of charcoal; but it was only in appearance. I poured on the whole a quantity of water. I then separated the black powder, which, on being dried and thrown into the fire, burned with flame. It did not sensibly colour cold water; but it communicated to boiling water, and to alkaline solutions it gave a deep brown colour. It was nearly in the same state which the saw dust would have been by exposure for some years to air and humidity. The acid liquor, which was almost as colourless as water after having been saturated with carbonate of lime, yielded, on evaporation, a yellowish gummy matter, with a solution of which the subacetate of lead formed an abundant white magma. This gummy matter, treated

* Translated from the *Annales de Chimie*, xii.

with weak sulphuric acid, furnished acetic acid and a precipitate of sulphate of lime.

I repeated the experiment with 16 grammes (247·1 gr.) of the saw-dust; but instead of pouring the sulphuric acid on the whole of this quantity, I triturated it, in small portions, adding the acid by little and little. In spite of these precautions, sulphurous acid was still disengaged; but I obtained a very thick tenacious mucilage, which I dissolved in a certain quantity of water and filtered through a cloth; there remained a blackish insoluble matter of the weight of five grammes (77·2 gr.) similar nearly to that in the preceding experiment. The acid liquor saturated with chalk, and then evaporated, yielded 10 grammes (154·5 gr.) of a gummy substance of a reddish-brown colour. As Messrs. Fourcroy and Vauquelin say in their memoir, that vegetable substances are not decomposed by cold sulphuric acid, I thought that this disengagement of sulphurous acid must at least be very rare, and I therefore gave over experimenting with the saw-dust of elm.

In order to arrive at greater certainty in the results, I tried to prepare the ligneous matter: but finding it difficult to bring it to a state of perfect purity, I found it more convenient to have recourse to strips of linen cloth, which may be considered as pure ligneous matter.

Action of Sulphuric Acid on Linen Cloth which had been used.

Twenty-five grammes (386·1 gr.) of this cloth cut into small pieces lost by heat one gramme (15·5 gr.) of water. I put this cloth into a glass mortar, and moistened it with 34 grammes (525 gr.) of sulphuric acid, taking care to agitate continually the mixture with a strong rod of glass, in order that the acid might penetrate as uniformly as possible all the parts of the cloth; and that there should be a long enough interval between each addition of acid, to allow the heat which it developed to be entirely dissipated. Not the slightest portion of sulphurous acid was disengaged. A quarter of an hour after the mixture had been made, I pounded it with a glass pestle: all the tissue of the cloth disappeared, and there remained a mucilaginous mass extremely tenacious, heavy, homogeneous, and faintly coloured, which I allowed to stand for twenty-four hours. I mention these precautions as proper to be followed, in order to obtain the purest products; for, whatever the process adopted may be, no sulphurous acid is disengaged, nor is there the least indication of matter of a carbonous appearance. This mucilaginous mass dissolved entirely in water, with the exception of an amylaceous matter which weighed when dry 2·5 gram. (38·6), and which was nothing but a part of the cloth which had not been sufficiently acted upon by the

the sulphuric acid. The mucilaginous mass thus diluted in water was saturated with chalk, and, being filtered through a linen cloth, became clear, and had only a very slight amber colour. After washing the filter well, and pressing strongly the sulphate of lime, I united the liquors together, and evaporated them to the consistency of a syrup, which was less coloured than that of capillaire. A small quantity of sulphate of lime was separated on cooling. I continued the evaporation carefully until the mass was quite dry, and I obtained a transparent gum, slightly coloured; it weighed 26·2 grammes (404·6 gr.) and was produced from 21·5 grammes (332 gr.) of cloth, deducting the one gramme of water, and the 2·5 grammes (38·6 gr.) of amylaceous ligneous matter. I must not omit to mention that the sulphate of lime resulting from the saturation, although well washed, still retained vegetable matter; for on being exposed to the fire it took a brownish colour, and gave out an odour of sulphurous acid. In order to ascertain whether this remarkable increase of weight might not be owing to a fixation of the elements of water or sulphuric acid, five grammes (77·2 gr.) of this artificial gum were dissolved in water; I added to the solution some oxalic acid, in order to precipitate the lime which it held in combination, and which might also be precipitated from it by sulphuric acid. The precipitate of oxalate of lime, collected and strongly heated, gave 0·28 grammes (4·3 gr.) of lime.

The gummy solution thus precipitated was evaporated to dryness, and the residue treated with boiling nitric acid, and diluted with water; some nitrate of barytes was added, which produced a precipitate, that when heated red weighed 1·6 gramme (24·7 gr.) which contained 0·54 gramme (8·34 gr.) of sulphuric acid. As there was therefore no aëriform fluid disengaged during the action of the sulphuric acid on the cloth, the 26·2 grammes (404·6 gr.) of gummy matter which we obtained may be supposed to be formed thus:

Ligneous matter	21·50 gram.
Elements of sulphuric acid fixed in an unknown manner	2·83
Elements of water fixed in an unknown manner	0·40
Lime combined	1·47
Total ..	26·20 gram.

It will be remarked that the manner in which we have viewed the action of sulphuric acid concentrated on the cloth is very opposite to that of Messrs. Fourcroy and Vauquelin, since these chemists admit that the vegetable matter is decomposed, and di-

 vides

vides itself into charcoal and water, which by uniting itself to the acid produces the heat which is developed ; but this heat appears rather to be owing to the actual fixation of the elements of the sulphuric acid and the water in the undecomposed vegetable substance.

Being desirous of knowing whether the sulphuric acid diluted with half its weight of water, could convert the ligneous matter into gum; I moistened some cloth with this acid thus reduced, but I did not obtain any mucilaginous mass. Exposed to a gentle heat and agitated continually, it resolved into a very homogeneous paste, which mixed with water gave a white substance resembling boiled starch; further diluted it appeared like an emulsion. On holding it up to the light, we remarked that the white matter held in suspension was formed of a multitude of brilliant spangles of extreme tenuity, nearly similar to what may be observed in a solution of soap. This sort of emulsion on being allowed to stand deposited, though very slowly, a substance which we took at first sight for starch, but which has none of its properties; it represented almost the whole of the cloth employed. The liquor separated in this manner gave, after being saturated with chalk, a small quantity of almost colourless gummy matter, which appeared to me to contain but few traces of sulphuric acid.

If the cloth be moistened with nitric acid, it may also be converted into a white starchy matter. No apparent alteration is manifested at an ordinary temperature; but by exposing the mixture to a hot water bath until nitrous gas begins to be formed, it is converted into a white and uniform paste, exactly similar to that obtained by the sulphuric acid. Well washed and dried, this matter had a slight satin appearance, especially when reduced to powder; if moistened, a slight hissing was heard, and it became converted into a paste much divided. In potash it does not undergo any sensible solution. It appears to be the ligneous matter of the cloth very slightly altered, being nearly analogous to what results from the putrefaction of rags for the preparation of paper. It might be curious to inquire, whether during this sort of fermentation gum and a little sugar are not formed, as happens in respect of putrefied starch, according to the recent observation of M. de Saussure.

[To be continued.]

VIII. *Notices*

VIII. *Notices respecting New Books.*

Remarks on the History and Philosophy, but particularly on the medical Efficacy, of Electricity in the Cure of Nervous and Chronic Disorders; and in various local Affections, as Blindness, Deafness, &c. By M. La Beaume, Medical Surgeon-Electrician, F.L.S. &c. 1 vol. 12mo. pp. 373. 9*s*.

This is a second edition of a work, which should be perused by medical practitioners, illustrated with many new and striking cases. The following extract presents a perspicuous view of the theory of Electricity advocated by the author.

"Among the illustrious names who have formed theories on the electric principle—Franklin and Watson, Du Faye and Symner, deserve chiefly to be reckoned. These learned philosophers and mathematicians have entertained different opinions in accounting for the electrical phenomena. Some of these maintained the doctrine of the *unity* of Electricity in *distinct states;* and others, not without reasons to support their hypothesis, contended for *two distinct fluids* in the electrical principle. The belief, however, of modern Electricians in *one fluid, sui generis,* possessing attraction and repulsion, and other qualities on which the system is chiefly founded, may be comprised in the following summary.

"1st.—That *all* substances in nature, whether solids or fluids, animate or inanimate, have *inherent* in them a certain portion of the electric principle, and which in their *proper state* is the *quantum sufficit* of their *natural capacity*—the just proportion of this elementary matter exactly preserving the balance—so that its mutual attraction and repulsion should *maintain* the equilibrium of their being—*preserve* their characteristic differences, and *prevent* disorganization and decay.

"2nd.—A redundancy of this elementary principle compressed into a limited capacity, either by a chemical or mechanical agency, constitutes that state which is denominated *positive* or *plus Electricity*, while a *deficiency* of this *ethereal principle* is designated *negative* or *minus Electricity*. These opposite conditions, each of which is disorder, exist as an accidental effect of some producing cause or causes, and exhibit the different appearances which result from the action of the electric fluid.

"3rd.—The *efforts* of the electrical principle to maintain its natural state in all bodies, not only appear to prove the correctness of this theory, but show that its positive and negative, or *attracting* and *repelling* powers are only *the directions* which it takes to restore the balance of its disturbed repose. Thus a body *positively electrified*, imparts its superabundant Electricity to one

negatively

negatively electrified, or in a *natural state*, to enable it to bear the burden of an accumulated load. In a machine it will be observed, that the rubber and glass disturb the equilibrium of *electrical quiet*. The *chain* and *cushion*, from the *air* and *earth*, supply the loss of the electric fluid, imparted by the cylinder to the positive *conductor*, which rushing thence to surrounding atoms, returns to the source whence it came.

"To illustrate these remarks, observe by way of *comparative* analogy, the operations of a common pump. The water is forced up from the well by the handle and hand, and passing through the spout falls by the power of gravity instantly to the earth. A reservoir may, for a season, contain a part; but when a vessel is over-filled, the superabundant water passes from the inside to the outside, and then returns again to its native depositary. But when a vessel is not over-filled, the water will be *retained* in it for some time, till it is evaporated; and then descends again in fruitful showers on the parched ground. So it is with the Leyden jar, when charged with the electric fluid; but it acts with this difference, that its *outside* is in a *deficient* or *attractive state*, while its *inside* is *overloaded* with a *propelling* fluid, and the whole of its contents are discharged at once.

"*Gravity* can make but humble claims on the electrical principle, and even these are granted rather as a *boon* than a *right*, for the *equilibrium* of nature is its *grand business* and *end*. The Franklinian theory, therefore, has been found so agreeable to the analogy of nature, that it has been generally adopted. Like the Newtonian system, the *simplicity* of the *principle* gives *dignity* to the *doctrine*; while it supplies the *best key* to many dark and otherwise *inexplicable points* in *philosophy*, *physiology*, and *pathology*.

"The theory of *two distinct fluids* originated with Du Faye, an eminent French philosopher contemporary with Franklin, which he deduced from his practical researches into the different properties of vitreous and resinous electrics excited by friction. As he found the electricity developed by the positive and negative electrics was dissimilar in its states, he concluded they were *distinct* fluids, though possessing the *same* nature and *residing together* in all bodies. From their being separated in conductors, as well as from their reciprocal efforts to unite again, it was supposed the truth of this theory was made apparent. This opinion was strongly supported by Symner, who had philosophized so ingeniously on the subject, that, though it was contrary to the known laws of electricity, he gave to the error an air of plausibility.

"This opinion, however, is now as much exploded as the astronomical systems of Ptolemy or Tycho Brahe. The double current of these electricians imposed as much labour on the electric

tric principle, as the immense revolutions of these astronomers did on the sun performing its circuit round the earth, or the primary planets round their satellites. The honour of establishing a *sound theory*, founded on experimental results, is justly due to Drs. Franklin and Watson—names which will live in the records of history, and be handed down with honour to the remotest posterity."

A Treatise on Adulterations of Food and Culinary Poisons, exhibiting the fraudulent Sophistications of Bread, Beer, Wine, Spirituous Liquors, Tea, Coffee, Cream, Confectionary, Vinegar, Mustard, Pepper, Cheese, Olive-oil, Pickles, and other Articles employed in Domestic Economy; and Methods of detecting them. By FREDRICK ACCUM, Operative Chemist, Lecturer on Practical Chemistry, &c. 12mo. pp. 386. 9s.

The title of this very useful little volume fully explains its object. The author, in stating the experiments necessary for the detection of the frauds which he has exposed, has judiciously confined himself to such processes as may be performed by persons unacquainted with chemical science, and has given his instructions in the plainest language, divested of those terms which would be out of place in a work intended for general perusal. We shall take an early opportunity of laying some extracts before our readers.

Reports made upon the Patent Moveable Inodorous Conveniences, by the Royal and Central Agricultural Society of France: with a Supplement by the Comte FRANCOIS DE NEUFCHATEAU; *The Royal Medical Society of Marseilles; The Society for the Encouragement of Natural Industry; The Medical Society of Lyons; The Society of Emulation of Rouen; and The Medical Society of Paris.* Translated from the French. 8vo. pp. 116.

The invention to which this pamphlet alludes is the subject of a patent in France, and patents have also been granted for it in this country. Its title explains in few words the nature of the invention—" moveable conveniences;" but the moveable part may be described rather as *the receptacle* than the whole apparatus. In fact, it consists of two casks so contrived and adapted to the *seat*, that they may be removed from time to time, and others substituted in their place; and all so well fitted that no effluvia can possibly escape. In cities circumstanced like Paris, where the convenience of water-closets, as in use in London, is but little known, this invention offers many inducements for its adoption. The invention however offers other inducements, which, in a national point of view, as promising an incalculable increase in our agricultural products, render it highly deserving of public attention.

tention. We have often been surprised to think what an immense value is every year washed into the Thames from the water-closets, &c. in London, through the common sewers; but the difficulty of obviating this loss, without great sacrifices in cleanliness, comfort and health—blessings above all price in a large community—always struck us so forcibly, that till we perused the pamphlet before us, we never thought of the possibility of preserving, nay, improving these, and at the same time preserving separately, both the *soil* and the *urine* for the purposes of agriculture. The object is so highly valuable as to be worthy of Legislative interference; and we recommend the subject to the most serious consideration of all who take an interest in the welfare of mankind.

In the Press,

A third edition of Dr. Merriman's Synopsis of the various Kinds of difficult Parturition; with Additions, and an Appendix of illustrative Cases, plates, &c.

IX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

A PAPER On the Polarization of Light, by J. F. W. Herschel, Esq. F.R.S. was commenced reading on the 9th, and continued on the 16th and 23d December 1819, when the Society adjourned for the Christmas vacation.

ROYAL ACADEMY OF SCIENCES, PARIS.

This body has proposed the following prize subject:

To furnish a comparative description of the brain in the four classes of vertebral animals; particularly in reptiles and fishes: endeavouring thence to ascertain the analogy of the different parts of this organ, carefully marking the changes of form and proportion which they present, and tracing out, as accurately as possible, the roots of the cerebral nerves. Observations on a certain number of genera will be sufficient; but it is required that the principal preparations be represented by drawings, so detailed that they may thence be reproduced, and their accuracy determined. The prize is a gold medal of the value of 3000 francs, to be adjudged in the Public Sitting of May 1821. The memoirs, written in French or in Latin, to be transmitted to the Secretary of the Institute by the 1st of January of that year.

SOCIETY OF PRACTICAL MEDICINE, PARIS.

This Society offers a gold medal, of the value of 200 francs, to be adjudged in the last sitting of this year, for the best answer to the following question:

"Are the affections which may be traced in the abdominal viscera, after putrid or adynamic and ataxic fevers, the effects, the cause, or a complicated effect of these fevers?"

The memoirs, in French or in Latin, to be delivered (free of charge) to M. Girandy the Secretary before the 1st of October 1820.

ROYAL ACADEMY OF SCIENCES, NAPLES.

This Academy has proposed the following as a subject for a prize to be awarded on the 30th of next June.

"Required a description and drawing of an instrument which shall, 1st, unite in itself the properties of the largest and most perfect meridian circles and meridian telescopes that have yet been made; 2. the adjustment of which shall not depend on a spirit level; 3. which may traverse and turn with facility, so as to admit of observation in two contrary positions; and, 4. which can be made by any good workman possessing the means afforded by the present state of the arts." The memoir may be written either in Latin or Italian, without a name, but having a motto, and sent to the Secretary, under cover to the Secretary of State for Foreign Affairs, before the end of February.—Prize, a gold medal of the value of 600 ducats.

ROYAL SOCIETY OF SCIENCES, GOTTINGEN.

A prize of 50 ducats value is offered by this Society, to be awarded in November 1820, for

"A critical Synopsis of the most ancient Monuments of all kinds that have been found in America, to be placed in comparison with those of Asia, Egypt," &c. The memoir is to be written in Latin.

X. *Intelligence and Miscellaneous Articles.*

LETTER FROM THE ARCTIC LAND EXPEDITION *.

"Aug. 27, 1819.—At sea.

"After passing the southern part of Greenland, named Cape Farewell, we met with much ice; but as it did not lie thick, little difficulty was experienced in forcing a way through it, nor did it prove so great an impediment as the contrary winds which still

* From Blackwood's Edinburgh Magazine.

continued

continued to thwart us. Near the Greenland coast the streams or fields of ice consisted of a collection of loose and comparatively flat pieces, more or less densely compacted together, according to the state of the weather; but on approaching the shores of Labrador, we fell in with many icebergs, or large floating fields of ice. The variety of forms assumed by these masses afforded us amusement, but occasionally we saw some of such an enormous size, that every other feeling gave place to astonishment. One of these larger bergs we estimated to be 200 feet high above the water, and above half a mile in length. Its surface was broken by mountains of no mean size, with deep valleys between. Enormous as these dimensions must appear, you will be more surprised when I inform you, that the part of an iceberg which projects above water, amounts only to a ninth part of the whole mass, that being the proportion of ice which floats above salt water. Arthur's Seat clothed in snow would have formed only one pinnacle to this berg. When these bodies became familiar to us from their frequency, we derived much pleasure from the various shades and gradations of colour they exhibited. The more compact parts were generally of a bright verdigris blue; towards the base a fine sea green prevailed; here and there a tint of red was seen, and the summits alone were snow-white. As the part of the ice which is covered by the sea decays more rapidly than that which is in the air, it often happens that one of these islands becomes top-heavy and tumbles over. We never saw one in the act of making this revolution, but most of them bore evident marks of having been overturned twice or thrice; the old water lines, intersecting each other in various directions, being still deeply engraved on their surfaces.

"We first beheld the land (Resolution island) during a fog, which soon became so thick that we could not see the length of the ship. In consequence of this, we got involved in a field of ice: then, to add to our distress, it fell calm; and although we could perceive that we were carried along by a violent current, yet the fog deprived us of ascertaining its direction, and the depth of water was too great to admit of our anchoring. After remaining in this situation for two or three hours, receiving occasionally some heavy blows from the ice, an alarm was given that we were close on the rocks. We all ran upon deck, and beheld a tremendous cliff frowning directly over the mast heads of the ship. It was perfectly perpendicular, covered in many places by sheets of ice; and its summit was so high, and shrouded in so thick a fog, that it could not be traced from the deck. We had scarcely time to make any useful exertions; for in a few minutes the ship fell broadside against the cliff, along the face of which

she

she was violently hurried by the current, towards a ridge of broken rocks, which in a short time would have torn the stoutest vessel to pieces. The heavy swell which prevailed caused the ship in her passage to beat against various rocky ledges which projected under water. One of the blows she thus sustained, drove the rudder out of its place; but it fortunately hung suspended by tackling which had been employed to secure it on coming amongst the ice. At this instant, when all human exertions seemed perfectly fruitless, the current eddied off shore, the land-breeze sprang up, a boat that we had put overboard succeeded in taking us in tow, and—what appeared almost miraculous—one of the last thumps the ship received caused the rudder to fall back into its place. By this combination of favourable circumstances, we succeeded in getting round the point we so much dreaded; and, setting all sail, we steered from the land. Upon the first alarm of danger, the women and children, of whom we had a large number on board, going to Lord Selkirk's colony, rushed upon the deck much terrified. The officers, hower, succeeded in calming their fears, and prevailed on them to go below out of the way of the sailors: but scarcely had this been effected, when the current carried us against a large iceberg which had grounded upon a ridge of sunken rocks that lay at some distance from the shore. The crash of the masts and yards, together with the grinding of the ship's side against the ice, terrified them more than ever; but we speedily got clear of the second danger without receiving further damage. Our troubles, however, were not at an end; the ship had received so much damage whilst on the rocks, that, on examination, a great deal of water was found in the hold. All hands were instantly set to the pumps; but, to our mortification, we found that the water rushed in faster than we could, with every exertion, discharge it. Affairs now wore a gloomy aspect; the water in the hold increased to upwards of five feet, and the men were getting tired at the pumps, when fortunately the weather cleared up a little, and we saw the *Eddystone*, one of the vessels that accompanied us, at no great distance: we bore down, and informed them of our situation. Every assistance in their power was promptly supplied; they sent 20 men and two carpenters. The services of the latter were invaluable, as our own carpenter had died in the earlier part of the voyage. With this fresh accession of strength, we kept the leak from gaining upon us; and after some time the carpenters succeeded in discovering and patching up the broken parts so as sensibly to diminish the influx of water. Their operations were however slow, and it was not till the evening of the second day that we succeeded in getting all the water out of the ship. During the whole of of this time, not only the officers and men worked hard, but even many of the women, recovering their spirits, proved eminently useful at the pumps. As the water decreased, the carpenters were enabled the more readily to repair the damage that had been sustained: and they ultimately succeeded so well, that one pump proved sufficient to discharge the water as fast as it leaked in. In this state we have continued ever since.

"In these straits the Hudson's-bay vessels are generally visited by a tribe of Esquimaux, who frequent the shores during summer, and come off to the ships for the purpose of bartering their whole wealth, which consists in whale- and sea-blubber, for iron, which has become an article of the first consequence to them. Accordingly, one day when we were above 20 miles from the shore, these poor creatures ventured off in their skin canoes, pulling with the utmost anxiety to reach the vessels. It sometimes happens, when the ships have a fair wind, that they run past the Esquimaux haunts without stopping: in the present instance, however, we were detained by light contrary winds, which enabled them to overtake us; and when they did so, they expressed so much joy and exultation, that it was easy to conceive how great their disappointment must have been when they missed us. In a short time we were surrounded by 30 or 40 canoes, each carrying one man, with his small cargo of merchandize, which, to their great satisfaction, they speedily exchanged for pieces of iron, hoops, knives, saws, hatchets, and harpoons, and tin-pots. The wind continuing contrary during the remainder of the day, we stood in towards the land, and gave the women of the tribe an opportunity to come off, which they did in five large canoes framed like the large one of skins, but open, and each capable of carrying from 20 to 30 people. The oars were pulled by women, but there was an old man in each boat to direct them. As they brought off a great many children, I suppose we saw the whole tribe, amounting to nearly 200 souls.

"The features of the Esquimaux are not the most regular in the world: but it was pleasing to see their flat, fat, greasy faces. When they had disposed of their articles of trade, we presented the women and children with a few needles, beads, and other trinkets, and sent them away highly delighted. Since that time we have been contending against contrary winds; but by perseverance have succeeded in getting within a few days' sail of York factory, at which place I shall conclude and despatch.

"August 31—York Factory.—We have landed here in safety; find the country more pleasant than we expected, and have been told that the difficulties of travelling in this country have been much exaggerated. "J. R."

FOSSIL

FOSSIL ANIMAL REMAINS.

In making some further excavations lately in caverns in the vicinity of Breuge, in the department of Lot, the workmen laid open a depository of bones, some of horses; some of the rhinoceros, of the same species of which fossil fragments have been found in this country, in Germany, and in Siberia; and others belonging to a species of stag, now a non-descript, with horns pretty much resembling those of a young rein-deer. They were collected, and presented to the Academy of Sciences, by M. Cuvier, and are now in the King's cabinet.

MINERAL ANIMAL MATTER.—ZOOGENE.

Sig. Carlo di Gimbernat has discovered a peculiar substance in the thermal waters of Baden and of Ischia, of which he gives the following description in the *Giornale di Fisica*: —"This substance covers, like an integument, many rocks in the valleys of Senagalla and Negroponte at the foot of the celebrated Epomeo, beneath which mountain the poets confine Typhon. It is remarkable that in this very place should be found a substance very similar to skin and human flesh. One portion of this mountain that was found covered with this substance, measured 45 feet in length by 24 in height. It yielded, by distillation, an empyreumatic oil; and, by boiling, a gelatine, which would have sized paper. I obtained the same results at Baden. It may therefore be considered as confirmed that an animal principal is present in these thermal springs, which being evaporated becomes condensed in their neighbourhood. To this principle I have given the name of "Zoogene."

The Editors of the *Giorn. Fis.* state that they have seen the substance obtained by M. Gimbernat, and that externally it has the appearance of real flesh covered with skin.

M. FAUJAS SAINT FOND'S MUSEUM OF GEOLOGY AND NATURAL HISTORY.

M. Barthelemy Faujas Saint Fond, Professor of Geology, and one of the Administrators of the Royal Museum of Natural History, died last summer at Paris. This gentleman was well known by his various interesting memoirs on geology and other branches of science, and by his travels through Scotland. His passionate attachment to science induced him to expend an ample fortune in forming a geological museum, and by his numerous travels and unceasing efforts he succeeded in obtaining a most interesting collection, which was sold by public auction at his house adjoining the Jardin des Plantes, in the last month (December). M. Faujas is well known to have devoted a considerable share of attention to the study of volcanoes and volcanic products, and his collection was particularly rich in specimens from the ancient volcanoes of France, and other parts of Europe. These were purchased by M. Beudant for the private collection of the King of France. M. Faujas was also one of the first geologists who saw the high importance of the study of fossils to elucidate the former revolutions of the globe; and his collection was celebrated for the numerous and well characterized fossil remains which it contained, particularly of the class of vertebrated animals.

M. Faujas possessed the most perfect tooth of the gigantic Tapir that has yet been discovered. Of these very rare teeth not more than three or four have at present been found in any part of the world. The one belonging to M. Faujas was the hindermost grinder,and is the tooth drawn and described in Cuvier's *Recherches sur les Ossemens Fossiles,* Supplement to the article *Tapir.* Pl. II. fig. 7. This tooth was purchased by Mr. Bakewell, who was then at Paris, together with a tooth of the Mastodon from the South of France, and another species from the Cordilleras, and teeth of the fossil Hippopotamus and fossil Rhinoceros. Several magnificent specimens of fossil fish were in this collection, particularly one of nearly three feet in length from Montebolia in the Veronnais, the *Blochius longirostris* (Volta). Among various selections of fossil shells was one series from Plaisântin, particularly rich in species of which the greater part have their analogies living in the present ocean, as was proved by M. Menard de Lagroil, in a memoir published in the *Annales du Museum d'Histoire Naturelle.* The simple minerals in this museum were not very numerous, but there were some extremely rare and valuable specimens.

BORACIC ACID.

In our last, we noticed the boracic acid found in solution in certain lakes in Tuscany. M. Lucas, in a letter to M. Arago, mentions that this acid is found also in the crater of Vulcano. It is met with on the most heated parts of the surface, where vapours are continually rising, in a white light state, though sometimes soiled and sometimes mixed with sulphur. The incrustations are generally about three-fourths of an inch thick, and sometimes of more than a foot in extent. It is met with in scales, and sometimes fibrous. The nature of these incrustations was ascertained by D. Gioacchino Arrosto of Messina.

PHOSPHORIC ACID IN VEGETABLES.

Mr. Barry, in making experiments on pharmaceutical extracts obtained by carrying on the evaporation *in vacuo,* "became acquainted with the singular fact, that phosphoric acid in a soluble state

state was found in all the extracts. On further investigation, it was ascertained that this acid, besides that portion of it which exists as phosphate of lime, is contained in a vast variety of vegetables . . . .All those which are cultivated seem to contain phosphoric salt in great abundance."

WAVELLITE.

This substance appears, by an analysis made by M. Berzelius, to contain

Alumine	35·35
Phosphoric acid	33·40
Fluoric acid	2·06
Lime	0·50
Oxides of iron and manganese ..	1·25
Water	26·80
	99·36

NEW VEGETABLE ALKALI.

MM. Pelletier and Caventon have announced the discovery of this alkali in the seeds of the *Veratrum Sabadilla*. In examining among other vegetable substances which have a marked action on the animal system, they found, in the *Ciradille*, a crystallizable alkaline matter extremely acrimonious; but they have not yet become perfectly acquainted with its nature.

RECTIFICATION OF ALCOHOL.

A correspondent of the *Giornale de Fisica* reports an experiment which may be applied with advantage to this purpose. It is a well known fact, that water passes with facility through bladder, while alcohol is almost perfectly retained by it. If a bottle of wine be closed by a piece of bladder, instead of a cork, a portion of the water will be found to have evaporated and passed off through the membrane, and the wine left will be found proportionally stronger. If a bladder half filled with alcohol of the specific gravity of 867, and having its orifice closed, be exposed to the sun, the air, or the heat of a stove, in a short time the alcohol will be found rectified to 817 spec. gr. and in this manner all the water may be evaporated. If the same bladder with its contents be then exposed to a humid atmosphere, (as in a damp cellar,) it will imbibe water, and return to 867 spec. grav. which water may again be separated by hanging it in a dry place. In one word, the bladder is a filter, which suffers water to pass through it, but not alcohol.

NEW PIGMENTS.

MM. Colin and Taillefort have observed, that if any of the blue or green carbonates of copper are heated to the temperature

 of

of 212° Fahr. they lose their water and become *brown.* In this anhydrous state they form a body of such good colour as to promise to be useful as a pigment.

But painters have been more at a loss for a durable purple than for browns, and this desideratum seems at length to be supplied. His Excellency the Count le Maistre of St. Petersburgh, in a letter to Dr. Creighton (published by Dr. Thomson) describes a process for obtaining this unchangeable colour. The Count first notices the tendency of the muriate of gold to form purple coloured compounds—with the oxide of tin; with gelatine and starch, when mixed and boiled with them; and with the earths, if precipitated with them and heated: and then gives the following process for preparing the new colour:—One part of dry muriate of alumina, one of sulphate of magnesia, four parts of muriate of barytes, and five of carbonate of soda, are each pulverised separately. They are then mixed in a glass mortar, and a little water is added, merely enough to moisten the mixture. Then a diluted solution of gold is added, by little and little, pounding the matter all the time in the mortar, till the whole has acquired a light sulphur-yellow tint, and the consistence of cream. The pounding is continued a long time, to produce the decomposition of the salts with as little water as possible. When no more effervescence is perceptible, and when the salts cease to crack under the pestle, a sufficient quantity of water is to be added for their complete solution. This tedious process is essential to unite the oxide of gold with the earths; and the whole success of the operation (which is pretty capricious) depends on it. The precipitate is to be left 24 hours in the mortar, stirring it from time to time with a glass rod. It is then to be poured into a saucer, or a similar vessel, and left till the powder subsides. The supernatant liquid is then drawn off with a syphon, and the deposit is dried in the shade, without washing it. The precipitate when dry is yellowish white. The muffle in which it is to be baked, ought to be red hot. The powder is put (of the thickness of one or two lines) upon a silver or porcelain plate; and it must be withdrawn from the fire the instant that it acquires its purple colour. If too long exposed to heat it acquires a violet tinge. This is occasioned by the salts it still contains; for, after it has been washed, it may be kept red hot without losing any of its colour, which indeed acquires greater lustre. These trials were made on a small scale, and, the Count observes, may be improved. The colour appears to want intensity, but the mixture of oils or gums renders it sufficiently deep. For oil-painting it must be carefully rubbed with a mixture of drying oil and varnish. The painting is to be begun by a thin transparent coat: a second is sufficient to give it all the lustre of which it is susceptible. The under coats ought to be prepared with raw terra de Sienna.

NEW

NEW PURPLE OF CASSIUS.

The Count de Maistre having placed a sequin in contact with mercury at one of its sides, and having at the end of 24 hours fused it with an equal weight of tin, obtained an alloy fusible in boiling resin. This alloy, triturated with caustic ammonia in a mortar, yielded a powder of a fine purple colour.

FULMINATING GOLD.

A fulminating gold has been formed by the Count de Maistre, by pouring a small quantity of solution of gold into red (Bordeaux) wine. A sediment fell down, which when dried, and then exposed, in an iron capsule, to the heat of a charcoal fire, exploded.

EARTHQUAKES.

AMULREE, (*Scotland*,) Dec. 4—About half an hour past seven o'clock this evening, there was felt here a smart shock of an earthquake, similar to that felt at Comrie a few days ago: it lasted two or three seconds; its direction was by the Grampian Hills eastward. The houses and furniture shook, and the whole went away with a noise like the slow passing of carts.

MITTENWALD, (*Bavaria*,) Dec. 29.—On the 20th inst. at 55 minutes after seven in the morning, the shock of an earthquake was felt around this place. It lasted about seven or eight seconds, and took a direction from south to north. The motion it produced was quite as strong as that on the 10th of April last year. The wind, from the south, was very still. The thermometer of Reaumur stood, in the shade, four degrees above zero, and the barometer two degrees above changeable weather. On the 21st, the thermometer rose seven, and the barometer six degrees, with a powerful south wind.

INDIA. MIRZAPORE. Letters from Chunar and Mirzapore mention, that about eight o'clock in the evening of the 16th June, 1819, the shock of an earthquake was experienced at these places. The same effect, in a slight degree, was remarked about the same time at Calcutta. At Chunar, the motion was accompanied by a noise in the atmosphere, which resembled that occasioned by the rapid flight of birds.

JIONPOOR.—The shock of the earthquake that was felt in Calcutta, on the evening of Wednesday the 16th June, about half-past eight o'clock, was experienced also at Jionpoor, pretty nearly about the same time, as will appear by the following extract of a letter transmitted to us from that station, which has just reached us:

"A strong shock of an earthquake was felt here on the night of the 16th of June, at a quarter past 8 o'clock; there were three distinct vibrations from west to east, with the usual accompaniments of rattling wall shades, swinging punkahs, and flapping doors. There are different opinions as to its duration, which appeared to me about twenty-five seconds; the intervals were very distinct. It was not accompanied by the rumbling noise I have usually heard on such occasions, and which I have hitherto imagined to be the earth's vibration. Both the noise and motion must be separate effects of some unknown cause. The rains have not yet commenced, and the weather has been unusually hot."

SULTANPORE.—Extract from a letter dated Sultanpore, Oude, 17th June 1819:—"A severe and awful shock of an earthquake was felt at this station last night, at seventeen minutes past eight, which lasted some time, and occasioned very considerable alarm. The bungalows actually rocked, particularly the mess one of the 1st battalion 19th regiment, in which the officers were at dinner at the time, and the huts of the soldiers were a good deal damaged. The heat for the last two or three days has been excessive, and not a drop of rain has yet fallen."

BENEFICIAL USE OF SALT IN FEEDING STOCK*.

In the year 1817, eight tons of salt only were granted under the then existing act, for the use of cattle; five tons of which were imported for the use of the Schoose farm. In the last year, ten tons were obtained for the like purpose; but from the facilities afforded of transferring it, above thirty persons were supplied from the stock; and all of those with whom Mr. Curwen has had the pleasure of conversing, concur in the benefit which has been derived by the addition of salt to the food of their cattle and horses.

The practice of daily giving salt to cattle, in the proportion of four ounces per head, commenced at the Schoose farm, in October 1817, with the feeding on steamed food, consisting of chaff, i. e. cut straw, or the husks of grain, and continued during the succeeding seven months; after which, and during the five summer months, while the cattle were fed on clover and grass, the daily quantity was reduced to three ounces per head, mixed with a little bran. Calves and one and two years old stock were given from two to three ounces each every day. The least thriving of the calves were observed to be the fondest of salt.

In a very early stage it was discovered, that salt thus given, removed the unpleasant flavour from the milk, while milch cows fed not only on the bulb but the tops of turnips. What may be the operation of salt, Mr. Curwen does not presume to decide: the fact, however, valuable as it is, is completely established; and the tops of turnips, amounting in weight to a sixth part of

* From the Communications to the Board of Agriculture.

the

the crop, may now, with the addition of salt, be safely given to milch cows, though previously to the use of salt, these were never so applied.

A general opinion prevails that inflammatory disorders have been less frequent since salt has been used. It appears to promote digestion, increase the quantity of milk, and dispose the animal to fatten: it also makes an evident improvement in the handling and the appearance of the skin.

Though the allowance of salt to cattle has hitherto been restricted to four ounces daily, yet little doubt is entertained that it might beneficially be increased: but experiments have not been made to ascertain the extent to which it might advantageously be given.

The allowances to horses last year was four ounces each per day; this year it has been increased to six, during the autumn and winter months, mixed with steamed potatoes; in the summer with their corn: their general health and spirits are considered to be improved, the gloss on their coats remarkably fine; fewer instances of grease have occurred, and the general opinion is, that with moderate care, the use of salt would diminish the liability to the disease, if it should not prove an actual preventive to the complaint. An experiment was made with two horses, which had been many years in the coal works, and, from the grease, were rendered nearly unserviceable. During the first month, they had each eight ounces of salt per day; the second ten; and now they have twelve ounces. The first perceptible effect was to prevent the very offensive smell of their dung—their coats improved—the discharge from their legs decreased, and has ceased—the swelling, in some degree, has subsided—the pain appears to be less violent, the stiffness of the limbs is not so great, and they are able to perform moderate work. They have been observed to drink somewhat more water. Whether a complete cure can be effected in so advanced a stage of the disorder, is yet doubtful, though the beneficial effects of the salt seem to be confirmed by the experiment.

The importance of salt in preserving the health and soundness of sheep is decisive. Two ounces per head has been given weekly to the flock. To those feeding on turnips or rape, three ounces. One hundred sheep have been slaughtered in the course of the last six months, without the least appearance of unsoundness being perceptible:—in former years, and before salt was made use of, the ewes were scarcely ever untainted.

Since it has been discovered that rock salt is capable of being ground, much labour and expense have been saved.

As all the drainage from the sheds and yards is collected and thrown over the middings, the dung of the ensuing year will have the addition of four tons of salt, or the greater part of it, the effect of which may possibly be perceptible in the succeeding crops. The greatest benefit to agriculture, from a general and liberal use of salt, may reasonably be anticipated,—besides opening a new and extensive source of trade, and giving employment to thousands.

Whilst four ounces per day to any animal were considered as ample, the general consumption at this rate would reach, it was supposed, 300,000 tons annually: subsequent experience has confirmed this estimate, which possibly may be greatly increased, should an augmentation of the daily allowance of stock be found a beneficial measure.

16, Chapel Place, Jan. 23. 1819. J. C. CURWEN.

The duty on rock salt is 5*l.* the ton; the cost of the salt 10*s.* It may be obtained by application in writing to the excise officer of the place, who then grants a certificate. The salt may be transferred from any stock, on application to the excise officer, who grants a certificate, by which any quantity, not less than eight stone, may be had. Messrs. Horne and Stackhouse, of Liverpool, have offered their services in furnishing salt at 6*l.* per ton, on receipt of the exciseman's certificate, the purchaser paying the freight.

MILDEW.

The following is a communication from Sir J. Sinclair:—"The blight or mildew in wheat is by far the greatest calamity to which, in an agricultural point of view, this country is liable. As it originates from corruption and the growth of the fungus tribe, it seems to me most probable that the use of saline manures would be found the most efficacious preventive. Many circumstances, already communicated to the public, tend to justify that idea; and it is now in a great measure confirmed by an experiment that has just been reported to me by Mr. A. Robinson, at Almond Myrehead, about 16 miles from Edinburgh. On the 1st of November 1818, Mr. Robinson sowed 28 lbs. of marine salt on three falls of sandy land, mixed with the seed wheat—this at the rate of 26 bushels per Scotch, or about 20 bushels per English acre. The crop was reaped on the 27th of August 1819, and the part salted produced at the rate of about three bushels per acre more than the rest of the field. The whole crop was much injured by the rust, otherwise called blight or mildew, *excepting the part that was salted;* which though not altogether free from it, yet the injury was very inconsiderable, and perhaps would have been totally avoided, had a greater quantity of it been made use of. Mr. Robinson thinks that it will be better first to sow the wheat separately, the salt to be sowed and harrowed-in afterwards; for he found that the wheat did not spring up so well, in consequence of its being sown in immediate contact with the salt. Crushed rock

rock salt will answer as well as marine salt, and the quantity should be varied from 20 to 30 bushels per English acre. It would be extremely desirable that the result of any further experiment tried should be communicated to the public, that the question may, if possible, be put to rest."

FREEZING SWEET WORT.

To Mr. Tilloch.

Sir,—From repeated observation, I have found that it requires a much greater degree of cold to freeze sweet wort, which of course has not been fermented, than it does to freeze water. Can any of your ingenious correspondents explain on philosophical principles, why frost so operates as to produce the effect above mentioned?

Boston, Jan. 1820. S. V.

ACORNS A SUBSTITUTE FOR COFFEE.

Dr. Maex, a German physician of some eminence, ascribes great medical virtues to an infusion of acorns used in the same manner as coffee. In 1793 he published some experiments on this subject, and gave the following directions for preparing and using the acorns:

Take sound and ripe acorns, peel off the shell or husk, divide the kernels, dry them gradually, and then roast them in a close vessel or roaster, keeping them constantly stirring; in doing which especial care must be taken that they be not burnt or over-roasted, either of which would be hurtful. The Doctor recommends half an ounce of these roasted acorns, ground and prepared like coffee, to be taken morning and evening, either alone or mixed with coffee and sweetened with sugar, either with or without milk.

The author says that acorns have been always esteemed a wholesome nutriment for men, and that by their medical qualities they have been found to cure slimy obstructions in the *viscera*, and to remove nervous complaints.

QUESTIONS BY A CORRESPONDENT.

What effect will *compressed* air have on the lives of animals? Injurious of course, but with what symptoms attended, as well as what affection it induces?

What is that by which the ratio of sidereal to solar time can be determined, if we have only the data of either's revolutions?

General Axiom.

To a spectator on a body which turns round its axis whilst revolving in an orbit, things without the orbit will *appear* to make as many circuits around that body as it *really performs* revolutions; but the number of circuits which things within the orbit will appear to make, will be either *one more* or ONE LESS than the

the number of revolutions really performed by the body, according as *the body revolves in the orbit reversely to the direction of the circum-polar motion*, or MAKES BOTH REVOLUTIONS TO THE SAME HAND. The above is abstract from any motion of the observer.

DRY ROT.

This destructive enemy of buildings, which generally commences its ravages in the cellars, may be prevented, or its progress checked, by white-washing them yearly, mixing with the wash as much copperas as will give it a clear yellow hue.

HYDROPHOBIA.

Sig. A. M. Salvatori of Petersburgh, in a letter to Professor Marrichini of Rome, gives the following remedy for this dreadful malady:

"The inhabitants of Gadici, but when or how I know not, have made the important discovery, that near the ligament of the tongue of the man or animal bitten by a rabid animal, and becoming rabid, pustules of a whitish hue make their appearance, which open spontaneously about the 13th day after the bite; and at this time, they say, the first symptoms of true hydrophobia make their appearance. Their method of cure consists in opening these pustules with a suitable instrument, and making the patient spit out the ichor and fluid which run from them, often washing the mouth with salt water. This operation should be performed the ninth day after the bite. The remedy is so effectual, that with these people this hitherto incurable disease has lost its terrors." *Bibl. Ital.* xiv. 428.

EXPLOSION OF A STEAM-BOILER.

On the 15th of December a dreadful accident occurred at Little Harwood, near Blackburn, by which two men were killed on the spot, two others were so shockingly bruised that they have since died of their wounds, and a fifth lies in a most deplorable state with little chance of recovery; and another man and a boy are also much bruised. The accident was occasioned by the explosion of a steam-boiler at the mouth of a pit which Colonel Hargreaves and two other gentlemen are now sinking for coals. The boiler weighs about four tons, and was carried, it is supposed, about fifty yards high, and fell sixty-five yards from the place where it was working. It was seen in its transit by many persons at a considerable distance. One piece of iron of a ton weight was thrown fifty yards, and several stones and slates were found at a hundred yards from the place they had occupied. The building which contained the engine was left with scarcely one stone upon another. One of the unfortunate sufferers was carried by the violence of the explosion to a distance of eighty yards, and lived several hours after he was taken up.

SIR WILLIAM CONGREVE'S CONICAL BALLS.

To Mr. Tilloch.

SIR,—In some of your late numbers, it appears, a patent has been taken by Sir William Congreve for a conical ball. Below are extracts so far back as 1808, on the same subject, which I beg you will insert in the Philosophical Magazine:—The Essayist has been dead several years. Your obedient,

Glasgow, 17th Jan. 1820. JAMES BOAZ.

Surgeons'-hall, Glasgow, 21 Nov. 1808.

Mr. Robertson read an essay upon the different forms of musket and other balls, exhibited paterns of various shapes; amongst these, he most approved of the long egg-shaped-ball, having one end thick, the other tapered small to a point: he maintained that this, containing as much matter as a spherical ball, will receive much less resistance in passing through the air; consequently, will not only go further with the same charge of powder, but hit an object with greater force and precision, as its deviation will be less, especially, by making spiral grooves in it, to give it a whirling motion in its progress through the air.—Mr. Robertson is to go more fully into this subject at next meeting.

28 Nov. 1808.

Mr. Robertson read further an essay upon projectiles, and the resistance balls receive in passing through the atmosphere, compared with what bodies of different kinds receive in passing through water. He exhibited some drawings and a diagram illustrative of his subject.

5 Dec. 1808.

Mr. Robertson brought to the Society some further models of balls of an oblong shape, hexagonally fluted in such a manner, that in their passage through the air they revolve on their own axis, while their heavy end goes foremost, being exactly upon the principle of an arrow shot from a bow. Mr. R. is to read an essay upon those exhibited this evening at some future period.

JAMES BOAZ, Secretary.

[Extracted from the Minute Book of the Glasgow Philosophical Society, by JAMES BOAZ, Secretary. Glasgow, 17th Jan. 1820.]

ENORMOUS BIRD.

Mr. Henderson has discovered, in New Siberia, the claws of a bird measuring each a yard in length; and the Yaknts assured him, that they had frequently in their hunting excursions met with skeletons and feathers of this bird; the quills of which were large enough to admit a man's arm. Captain Cook mentions having seen an immense bird's nest in New Holland, on a low sandy island, in Endeavour river, with trees upon it, and an incredible number of sea-fowls: he found an eagle's nest with young ones, which he killed; and the nest of some other bird of a most enormous size, built with large sticks on the ground, no less than twenty feet in circumference and 2 feet 8 inches high.

SENECA OIL.

Mr. George Gibson, near George town, on the Conemough river, Westmoreland county, Pennsylvania, in boring for salt water, at the depth of 270 feet, obtained *seneca oil*, which is said to be very pure. According to appearances, one barrel per day may easily be procured.

ERUPTION OF MOUNT VESUVIUS.

Naples, 4th Jan.

"On the 1st of January snow fell here, accompanied by much thunder. About the middle of the night the inhabitants were awakened by a violent subterraneous noise, and soon afterwards one of the most dreadful eruptions of Vesuvius commenced that has been witnessed for 20 years. The inhabitants of Torre del Greco, of l'Aumenziata, and even of Portici, experienced the greatest disquietude, apprehending the fate of Herculaneum and Pompeii. The lava, however, fortunately divided itself into five torrents, and flowed to the foot of the mountain for the space of a league. The crater is much enlarged, a part of its brink having fallen in. On the 7th the lava still continued to flow."

APOPLEXY.

The following facts, says *The Journal des Debats*, cannot fail to be interesting to humanity:—A lady about 40 years of age, who lived at the corner of one of the streets in Paris, was struck early in the month with apoplexy. Mr. Lavalette, the physician, was called in, and he restored her instantly to life by bleeding in a jugular vein, and stimulating the blood to flow abundantly, by the application of a common needle to the exterior part of the vein and the adjoining nerves. It is thus shown, that the mechanical stimulant is able to revive nervous sensibility, and by creating a kind of peristaltic motion, to deduce from the arteries a great quantity of blood.—There is also another instance of the efficacy of this application in the case of a Mr. Chatelin, a linen merchant, who has also been restored to existence under similar circumstances. It is trusted that this discovery will be generally beneficial.

STATISTICS.

During the year 1818, there have been in Paris 6,616 marriages, 22,421 deaths, and 23,067 births. Of the number of births 8089 children were illegitimate: of these 2004 have been acknowledged, and 6094 abandoned: of the deaths, 993 have been occasioned by the small-pox.

LECTURES.

We have every wish to notice among our miscellanies, intended Courses of lectures relating to any of the departments of philosophy,

sophy, medicine, the sciences and the arts: but by a new construction put by the Stamp Office on the laws respecting the duty chargeable on advertisements, we cannot do it without subjecting ourselves to an expense of which some who send these notices seem not to be aware.

RED RAIN.

On the 2d of November, in the afternoon, a red or reddish-coloured rain fell at Blankenburg and Dixmude, in Flanders. In the following night, the same happened at Scheveningen. Such phænomena have been frequently observed, and to them may be attributed the chronicled reports of showers of *blood* affirmed to have fallen in different countries.

LIST OF PATENTS FOR NEW INVENTIONS.

To Francis Fox the younger, of Derby, M.D., for his new or improved method of facilitating and ensuring the discharge of fire-arms and artillery of every description.—15th Jan. 1820.

To John Leberecht Steinhauser, of Moffatt Terrace, City-Road, Middlesex, artist; for improvement in portable lanthorns or lamps applicable to various purposes.—15th Jan.

To John Oldham, of South Cumberland Street, Dublin, Esq. for certain further improvements on his former patent bearing date the 10th day of October 1817, for an improvement or improvements in the mode of propelling ships and vessels on seas, rivers, and canals, by the agency of steam.—15th Jan.

To Joseph Main, of Bagnio-Court, Newgate street, London, for an improved method of preparing and spinning wool, cotton, silk, flax, fur, and all other fibrous substances.—15th Jan.

To James Thom, of Wells-street, St. Mary-le-bone, piano forte maker, and William Allen, of Castle-street, same parish, piano-forte maker; for a certain improvement in piano fortes.—15th Jan.

SINGULAR PHENOMENON.

During the night of Tuesday, 16th Nov. there fell, in the township of Broughton, North Am. on the south shore, so great a quantity of a black powder, as completely to cover the snow which was then on the ground.

Hartwell, Tunbridge Wells, Jan. 17th 1820.

Dear Sir,—I submit for the Philosophical Magazine, the following short journal of the thermometer and barometer during the late extraordinary cold weather.

1819. Dec. 31. Thermometer at midnight +10° of Fahrenheit.
Lowest during the night +2° ditto.

The barometer pretty stationary at 29 40 till night, when it fell considerably.

1820

1820 Jan. 13. At day break Thermometer $+2°$
Barometer 30·10.

Jan. 14. This extraordinary day and night deserve to be minutely described. The day became quite clear, and the thermometer fell between noon and 10 P.M. from $+10°$ to $-2°$. At midnight it was $-3°$. At 2 A.M. Jan. 15 it was $-5°$. And between that time and sunrise it fell to $-10°$, the lowest by fourteen degrees that I ever remember it. A correspondent represents it to have been still lower on the high Kentish hills. N.B. I use the plus sign + for *above zero*, and the minus sign — for *below zero*. The barometer continued falling through the night, and the following day was warmer. This severe frost killed great numbers of small birds, which I found frozen to death in the garden. Early on the Saturday morning (the 15th) many labouring people were forced to leave their work, and several accidents are reported to have happened to wayfaring people during the night. Near the city of Canterbury, I find the cold was equally severe, but in other places it was seven or eight degrees less rigorous. If, however, in this warm part of the island, the mercury descended to 10 degrees below zero, what must have been the cold on the Scottish hills! This leads to a question of the local influence of extremes of heat and cold. Perhaps some of your correspondents will favour the public with registers from different parts of North Britain. Yours, &c.

T. Forster.

To Mr. Tilloch.

Croom's Hill, Greenwich, Dec. 31, 1819.

Sir,—The register which I now send you of the quantity of rain fallen, and the quantity of evaporation from the surface of water during the past year, was kept (as heretofore) at Croydon until the 12th of September last. The rain-gauge and evaporator were then removed to Croom's Hill, and placed at the same height from the ground, and in every other respect the same as described and published in your Magazine. It will be recollected that the rims of both are placed exactly at four feet from the surface of the ground—and that the divided tube for measuring the quantity is to the same scale as that used by the Royal Society.—I subjoin the register of the last three years, as it may be pleasing to your readers at one view to see the different results.

	Inches of Rain.	*Evaporation.*
1817	25,349	22,227
1818	24,252	27,064
1819	27,339	21,369

I remain, sir, yours &c.

Henry Lawson.

Month.	Rain.	Evaporation.	Month.	Rain.	Evaporation.
From			1819.		
27 Dec. 1818,			11 to 18 July.	0·147	0·624
to 3 Jan. 1819,	0·028	0·038	18 to 25	1·275	0·809
3 to 10	0·998	0·115	25 to 1 Aug.	0·002	0·947
10 to 17	0·469	0·175	1 to 8	0·059	0·846
17 to 24	0·417	0·170	8 to 15	0·040	0·800
24 to 31	0·709	0·104	15 to 22	0·004	0·830
31 to 7 Feb.	0·591	0·172	22 to 29	0·004	0·834
7 to 14	0·562	0·191	29 to 5 Sept.	0·718	0·687
14 to 21	0·780	0·235	5 to 12	0·015	0·564
21 to 28	0·901	0·111	12 to 19	0·515	0·540
28 to 7 Mar.	0·799	0·200	19 to 26	0·345	0·303
7 to 14	0·004	0·182	26 to 3	1·223	0·336
14 to 21	0·142	0·333	3 to 10	0·079	0·321
21 to 28	0·453	0·333	10 to 17	0·009	0·249
28 to 4 April.	0·169	0·508	17 to 24	0·998	0·043
4 to 11	0·452	0·473	24 to 31	1·347	0·125
11 to 18	1·337	0·484	31 to 7 Nov.	0·196	0·168
18 to 25	0·707	0·245	7 to 14	0·426	0·104
25 to 2 May.	0·007	0·817	14 to 21	1·426	0·116
2 to 9	1·058	0·758	21 to 28	0·220	0·032
9 to 16	0·084	0·692	28 to 5 Dec.	0·883	0·064
16 to 23	1·330	0·543	5 to 12	0·164	0·066
23 to 30	0·778	0·565	12 to 19	0·820	0·047
30 to 6 June.	0·240	0·738	19 to 26	0·890	0·143
6 to 13	0·326	0·881			
13 to 20	1·110	0·702		27·339	21·362
20 to 27	0·159	0·682			
27 to 4 July.	0·215	0·688			
4 to 11	0·710	0·636			

P. S.—A table for ascertaining the relative quantity of rain caught at different heights, is much wanted and would prove of great use to the meteorologist. The publication of accurate observations on the quantity of rain which has fallen in various places at no great distance from each other, will prove serviceable to engineers as well as philosophers; and it will probably be found that rain for many months will be comparatively local; for I had occasion to observe that in Northamptonshire and Leicestershire, the quantity was not by some inches so great as in the neighbourhood of London, within the last six months; so much so as to cause an evident defalcation in the supply to mills and inland navigations in those counties.

☞ The greater part of my readers must have heard of the afflicting loss sustained by my much respected friend Mr. Cary of the Strand, to whom I have been indebted for the monthly meteorological table ever since the commencement of The Philosophical Magazine. I am sorry to have to announce such a reason for its absence in the present number. A. T.

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1819.	Age of the Moon.	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Dec. 15	28	35·	29·30	Cloudy
16	29	35·	29·85	Fine
17	new	36·	29·44	Rain and stormy
18	1	51·	29·53	Cloudy
19	2	51·	29·60	Ditto—rain A.M.
20	3	54·	29·30	Ditto
21	4	41·	29·76	Ditto
22	5	42·	29·50	Fine—rain at night
23	6	38·	29·14	Cloudy
24	7	33·5	29·20	Fine
25	8	32·5	29·35	Ditto
26	9	28·	29·50	Ditto
27	10	33·5	29·50	Ditto
28	11	33·	29·50	Ditto
29	12	27·	29·60	Ditto
30	13	28·	29·40	Ditto
31	full	33·5	29·60	Cloudy—snow A.M.
1820 Jan. 1	15	10 A.M. 8 1 P.M. 18·0	29·50	Very fine
2	16	33·	29·52	Cloudy
3	17	24·	29·66	Very fine
4	18	27·	30·63	Cloudy
5	19	28·	30·10	Ditto
6	20	36·	30·05	Ditto
7	21	31·	30·35	Ditto
8	22	29·	30·58	Ditto—snow A.M.
9	23	28·5	30·76	Snow
10	24	31·5	30·40	Cloudy
11	25	33·5	29·70	Snow
12	26	28·5	30·15	Cloudy
13	27	29·	30·15	Ditto
14	28	24·	30·20	Fine

N.B. The 1st of January was by far the coldest day experienced in this place since the 9th of February 1816, on which day the thermometer indicated precisely the same degree of severe frost.

XI. *A Letter from Dr.* Hutton *to the Marquis* De Laplace, *on several Astronomical and Philosophical Subjects.*

Dr. Hutton having of late years perceived some illiberal attempts made, in his own country, unjustly to deprive him of the honour of his calculations to determine the density and mass of the earth; and even a seeming attempt of the same kind elsewhere; he deemed it necessary to address the following letter to M. De Laplace, on observing a similar recent appearance in the *Connaissance des Tems* of the years 1821 and 1822.

"Bedford-Row, London, April 9, 1819.

"My Lord Marquis,—I have for many years most anxiously wished to have the honour of paying my respects to you in person; but as there is no prospect of my being able to make a journey to France, being now in my 82d year, it only remains to address you in writing, and to request that you will accept the homage of a sincere lover of those sciences which you have contributed so essentially to advance, with the highest honour to yourself, as well as advantage to the scientific world.

"An opportunity has just offered for conveying this letter to Paris, and I avail myself of the same to request your acceptance of a copy of my "Tracts on Philosophical and Mathematical Subjects," which I beg to present to you as a small but sincere token of the high respect which I entertain, in common with the rest of the world, for your genius and talents.

"In these Tracts, I wish particularly to call your attention to the leading article in the second volume, "On the Density of the Earth;" and to express my wish that you, who have already effected such profound investigations of its figure, and the doctrine of its tides, would pursue the inquiry respecting its density, so as to correct the errors, or confirm the truth, of my labours. Indeed it seems to me surprising that similar experiments and operations have not been hitherto made in France, where science in every other department has been pursued with the most laudable zeal and the most distinguished success.

"As you must naturally feel an interest in whatever relates to this important undertaking, I beg here to state certain circumstances which led to it, with a general account of the manner in which the operations were conducted; and shall conclude by pointing out a slight mistake in the *Connaissance des Tems* for 1821, page 330, line 2, where my labours, in the solution of this important problem, are ascribed to Dr. Maskelyne, a credit which he himself never claimed, but, on the contrary, always corrected the mistake whenever he heard it mentioned.

"Some time before the year 1769, when a transit of the planet Venus was expected, the Royal Society contemplated the propriety of sending some skilful astronomers to distant countries, to observe that transit. But as the expense of such a mission was above their finances, they represented their difficulty to the King; when His Majesty was graciously pleased to grant a specific sum to defray the said expense. In consequence, two of my astronomical friends, MM. Wales and Bayly, were in due time sent out to Hudson's Bay, where they passed a summer and a severe winter; and after returning to England, they gave an account of their mission. This was communicated to Government by the Royal Society, together with a statement of the expenses incurred; which being less than the sum that had been granted, they begged to know the King's pleasure touching the surplus; when His Majesty was graciously pleased to reply, that the Royal Society might employ it on any experiment, or to any purpose they might think fit.

"After duly contemplating this subject, the Royal Society came to the resolution of employing that surplus in determining, by experiment, the mutual and universal attraction of matter, by observing the deviation of a plumb line from the perpendicular, by the attraction of some mountain; in forming which resolution Dr. Maskelyne took a leading part. In consequence, a correspondence was held with many persons in all parts of Great Britain, to discover a hill or mountain proper for the experiment; which was at length found in the mount Shichallien in the centre of Scotland. The next consideration was, what person was to go to make the experiment? It was much wished that Dr. Maskelyne would go on that mission; but he excused himself, on account that he could not be spared from his duty as Astronomer Royal. After some delay, it was agreed to send a Mr. Burrow, a person who had formerly been Dr. Maskelyne's assistant at the Royal Observatory; who was instructed, first to make an accurate survey of the mountain in all its dimensions, in every direction horizontally, and a great number of vertical sections from all sides; so that from these a perfect model might be made of it, or its magnitude be accurately computed; and then he was to observe the deviation of the *plumb line.* After a year or two had been spent in those measurements, the Royal Society fearing the work might not be well performed, urged Dr. M. to go down himself and direct the operations; which, after obtaining the King's leave, he did accordingly, and brought back Mr. Burrow's survey and measurements, with his own observations of the deviation

tion of the plummet made on both sides of the hill. These operations employed the years 1774, 1775 and 1776; and the method of performing them was stated by Dr. M. in the Philosophical Transactions for the year 1775.

"Thus then the problem was performed, so far as showed experimentally that there *is a mutual attraction in all matter;* and that the sum of the two attractions on both sides of the hill in particular, amounted to 11·6 seconds. The Society were very well pleased with the operations. They also conferred on what further use might be made of these measurements; and it was perceived that by their means, after due calculations made, the mean density of the whole earth might be determined: for there were now obtained these data; viz. the mass and attraction of the hill, with the magnitude and attraction of the earth, to determine its density and mass. This was of course a grand and most important object. But the question was, *Who* was to perform the immense calculation that was requisite? who *would do it?* and indeed who *could* do it? There might be one or two persons who were thought capable of undertaking the task; but those and all the other members of the Society stood aghast at the prospect of the number and magnitude of the calculations, and shrank with dread from the mighty labour. I was then looked to, and asked by the President and Council to undertake the task; to which I was also strongly solicited by Dr. Maskelyne. Being then a *young member of the Society*, and anxious to distinguish myself, I undertook the operation and became responsible for the result; which, after the daily labour of nearly two years, I produced to the Society, with a minute detail of the particulars of the calculations, accompanied with the appropriate drawings. The Society were so well pleased with this work that they awarded me a liberal *premium*, and printed my memoir in the Philosophical Transactions for the year 1778, exactly in the form in which I had delivered it.

"It is to be observed, that in this operation, being the first of the kind that was ever made, all the modes of calculation were to be devised or invented; and that, without several plans which I contrived, it might be doubted whether the labour could ever have been accurately accomplished. To the ingenuity of these contrivances the most honourable testimony has lately been borne by one of the best mathematicians in this kingdom, Professor Playfair. The result of that calculation was, that the mean density of the earth is to that of the hill, in the ratio of 9 to 5. What was the density of the hill, was then not known, and therefore the density of the earth, as compared to water, could not then be determined. But to show the mode of making the comparison, I assumed for the sake of example, the smallest density of stone, $2\frac{1}{2}$, though this was obviously below the real density of the rock of the hill; then $\frac{9}{5} \times \frac{5}{2} = \frac{9}{2} = 4\frac{1}{2}$, would be less than the density of the earth, as the numbers to be adopted till we could know the real density of the hill.

"Many years after this, a lithological survey was made of the same hill by Professor Playfair, who found it to consist of rocks of a great variety of densities from 2·5 to 3·2, the medium being about $2\frac{3}{4}$ or 2·8. Assuming the former number then $2\frac{3}{4}$, as the density of the hill; then $\frac{9}{5} \times 2\frac{3}{4} = \frac{99}{20} = 4\frac{19}{20}$, or nearly 5, results for the mean density of the earth. And this result, instead of the former, I substituted in its proper place in my abridged edition of the Philosophical Transactions, from the beginning, to the end of 1800, in eighteen large quarto volumes, where it may be seen. If we assume 2·8, instead of the $2\frac{3}{4}$ above used, then $\frac{9}{5} \times 2{\cdot}8 = 5{\cdot}04$ comes out for the density of the earth, a little above 5, as the former was a little below it. I fixed therefore on the number 5, as my result for the mean density of the earth, determined by my calculation. The same conclusion is also given in my Tracts in the first memoir of the second volume, where the whole process is stated at large, as may be seen in the set of those Tracts now presented along with this letter; and as they were also formerly presented to the Royal Institute.

"And now, after all these evidences of my being the first, and only person, who had calculated the mean density of the earth from the original measurements, and have been considered and acknowledged as such for almost half a century, the world must be astonished to perceive the honour of that determination in danger of being transferred to another person who never expected nor desired it, unless the accidental mistake or misnomer in the *Connaissance des Tems* be acknowledged and corrected. I hope and trust therefore, from the known honour and liberality of your character, that the mistake will be corrected in the next Number in the *Connaissance des Tems* for 1822*.

"I hope you will pardon my troubling you with this long letter, and so many minute particulars. But old men, who can enjoy but little of the present, and expect still less from the future, live chiefly in retrospect. In my humble walk in life, my chief

* That Number of the *Connaissance des Tems* has been lately published, but without any direct notice of the mistake above alluded to; and even in a new memoir, similar to the former, not only is the mention of the very name and experiment omitted, but special commendation is bestowed on another far inferior experiment by Mr. Cavendish, performed by an apparatus, which was prepared by the inventor of that experiment, the Rev. Mr. Michell.—Feb. 1820.

gratification

gratification is the reflection of having been a zealous promoter of mathematical science for upwards of sixty years, as a teacher, chiefly at the Royal Military Academy at Woolwich; as a laborious contributor to the Philosophical Transactions, and an Editor of that work; as the conductor of certain scientific Journals which have met with great encouragement; and also as the author of several successful publications on mathematical subjects.

"If I feel this satisfaction in a retrospect of laborious application in the zealous pursuit of those elementary sciences, which lead to the knowledge of the Principia and other works of our immortal Newton; how much more gratifying must your reflections be, in having so ably illustrated as well as honoured our great author, and carried his discoveries higher than perhaps he himself could have ever contemplated!

"That you may long enjoy this gratification, and continue to enlarge the boundaries of science, is the sincere wish of

"Your most obedient and faithful servant,

"CHARLES HUTTON."

To the Marquis de Laplace.

XII. *Memoir relative to the Annular Eclipse of the Sun, which will happen on September* 7, 1820. *By* FRANCIS BAILY.

THE solar eclipse which will happen on Thursday, September 7, 1820, will be the greatest of all those which have happened in this part of Europe ever since the year 1764; and indeed of all those which will again happen here before the year 1847. Like the two eclipses above alluded to, it will be *annular:* that is, the disc of the moon will not wholly cover the disc of the sun; but, in certain parts of the earth, the sun will show the appearance of an *annulus*, or ring, round the body of the moon; the position and magnitude of which will depend on the situation of the spectator. In no part of England, however, will this annular appearance be observed*: but, on the continent, in any part of that tract of country which extends nearly in a straight line from the north of Westphalia to the south of Italy, the inhabitants will have an opportunity of beholding this singular phænomenon.

Annular eclipses do not appear to have been noticed by the ancients, who probably confounded them with partial ones. Indeed, the only authentic accounts of any well observed annular eclipses in this part of Europe (besides the one in 1764 above mentioned) are those of February 18, 1736-7 and of July 14, 1748;

* The eclipse however will be annular in the Shetland islands: and it will be of considerable magnitude along the whole eastern coast of Great Britain.

1748 *; the former of which was observed by the celebrated Colin Maclaurin at Edinburgh, and the latter by the Earl of Morton and Mr. Short at Aberdour Castle near the same place. Indeed the annular appearance of the eclipse of 1737 was confined principally to Scotland: and the eclipses of 1748 and 1764, although visible to a great part of Europe, were not so generally observed as could be wished on account of the badness of the weather; so that we have not any very considerable degree of information respecting this kind of solar eclipses. Moreover at those periods the lunar tables were so defective that it could not be predicted, with any degree of accuracy, *where* the annular appearance would be visible: so that many valuable observations were probably lost on that account. This difficulty however is in a great measure removed by late improvements not only in the lunar tables, but likewise in the analytical investigations relative to the calculation of eclipses; although the computations are still very laborious and troublesome.

Prior to the total eclipse, which took place in London in the year 1715, Dr. Halley published an account of the path of the moon's shadow across the island of Great Britain; and called on the inhabitants to note down their observations and forward them to him, in order that he might afterwards compare them, and thereby correct the elements made use of in the calculation of eclipses. The good effect of this measure may be seen in the Report which that illustrious astronomer afterwards drew up and sent to the Royal Society, and which is inserted in the Philosophical Transactions, No. 343, vol. xxix. page 245. Mr. Maclaurin, likewise, previous to the annular eclipse in 1737 before mentioned, wrote to several persons in the country, "desiring that they would determine and note down the duration of the annular appearance as exactly as possible; in hopes, by comparing their observations, to have traced more correctly the path of the centre and limits of the phænomenon." And in 1748 Mr. Alexander Munro (Professor of Anatomy of Edinburgh) by Mr. Short's desire wrote to all his friends in different parts of the country, to prepare in the best manner they could for the most exact observation of the annular eclipse which was about to take place in that year. And he regrets that he did not make this application earlier; for he remarks that had "my request of having the duration of the annular appearance measured been made more public before the eclipse (after Dr. Halley's example in 1715) I doubt not but I should have been able to have given a more exact

* See a detailed account of these eclipses, and of the phænomena attending them, in the Philosophical Transactions, vol. xl. page 177, and vol. xlv. page 582.

account

account of the progress of the centre of this phænomenon and of its limits." M. De L'isle also, with a similar view, published a notice to astronomers* in order, as he observes, "exciter les curieux de l'Europe, qui pourront voir l'éclipse annulaire qui doit arriver, d'y apporter toute l'attention possible, et de faire, de bonne heure, toutes les dispositions nécessaires pour la bien observer; afin de nous procurer tous les avantages que l'on en peut retirer pour l'astronomie, la géographie, et la physique."

It is worthy of remark that this eclipse (1748) was the first that the celebrated Lalande (to whom the astronomical world is so much indebted) ever saw. He was then only 16 years of age: and the impression, which it made on him, fixed his future pursuits in life, and induced him to become an astronomer. It indeed excited so much attention in Europe that the King of France (Louis XV) went purposely to Compiegne in order to observe it, attended by the Abbé Nollet, and Messrs. De Thury and De La Condamine; and furnished with every convenient instrument for the purpose. The royal astronomer there made several important observations †. M. Lemonnier likewise undertook the journey from Paris to Edinburgh, furnished with proper instruments, purposely to observe it, during its annular appearance: and he afterwards published some important remarks thereon ‡. M. De L'isle, abovementioned, published also a paper on the subject entitled *Nouvelle théorie des éclipses,* founded entirely on the recent observations that had been made §. M. Pingré afterwards added very considerably to these reflections in his interesting memoir, entitled *Recherches sur la longitude des plusieurs villes* ‖. Méchain likewise made a great many calculations relating to it, from the manuscript collections of M. De L'isle. But it was reserved to Lalande, *fifty years after the event*, to deduce the most important conclusions from this singular phænomenon, in his paper *Sur la grande éclipse annulaire de* 1748 ¶.

Considering therefore the interest which has always been attached to this kind of phænomena, and the important conclusions to be drawn from them, I was somewhat surprised to find that no particular notice has been taken of the ensuing eclipse either

* *Avertissement aux astronomes sur l'éclipse annulaire du soleil que l'on attend le* 25 *Juillet* 1748. It was published, however, only three months prior to the eclipse taking place: so that there was scarcely time for it to get into general circulation.

† The same monarch had also made several observations on the eclipse of 1737 at Versailles, attended by the celebrated Cassini.

‡ See the *Mémoires de l'Acad. Roy. des Sciences* for 1765, page 463.

§ *Ibid* for 1757, page 490. ‖ *Ibid* for 1766, page 17.

¶ See *Mémoires de l'Institut. (Scien. Math. et Phy.)* vol. ii. page 364.

in the *Connaissance des Tems*, or in the Nautical Almanac: but that it is merely announced there in the usual formal manner, without a single remark on the occasion. It is true that M. Bode, in his ephemeris published at Berlin, has given (as usual) a general outline of the eclipse together with a map descriptive of the phases: but he has not called on the inhabitants to look out for this phænomenon, nor drawn their attention to any of the subjects which it is most desirable they should observe. In order to supply this defect, as far as my humble efforts will avail, I have drawn up the following memoir, under the hope that it may induce others, who have more leisure and are at the same time more conversant with the subject than myself, to pursue the inquiry, and suggest further hints to those who may have an opportunity of observing this rare phænomenon.

M. De L'isle, in his *Avertissement* above alluded to, respecting the eclipse of 1748, suggested the advantage and propriety of some scientific person, in the principal states of Europe through which the shadow of the moon's umbra passed, undertaking to announce to the inhabitants the several observations which it would be proper for them to make; and afterwards to collect and arrange such observations for further investigation, if sufficiently convinced of their accuracy. A similar plan might be adopted in the present instance; and, from the more general diffusion of science, would be more likely to be attended with a beneficial effect. Such collections of observations (when made) should be sent by the different collectors to one or more of the principal astronomers of Europe, in order that they might be finally investigated, and the result laid before the public.

With that view I would take this opportunity of requesting those, into whose hands the present memoir may fall, to circulate it as much as possible on the continent; and amongst those persons who, from their connection with any foreign literary journal, may be likely to diffuse the subject of it amongst the inhabitants of that part of Europe and Africa where the annular appearance will be observed. Such of our own countrymen likewise, that may be travelling in any of the provinces on the continent here alluded to, will promote the interest of astronomy if they would carefully note down or collect any of the circumstances hereinafter alluded to, or indeed any other remarkable phænomena that may happen during this eclipse. I shall be happy to receive any observations of this kind that may be forwarded to me, and will preserve the result of them, as above proposed, for a future investigation.

The elements of the present eclipse I have computed from M. Burckhardt's tables of the moon and M. Delambre's tables of of the sun: and they are as follow. The ecliptic conjunction will take place on September 7, 1820, at

$1^h. 51'. 37'',3$ P.M. *apparent* time }
or } at Greenwich:
1. 49. 26, 2 P.M. *mean* time }

And, at that time, we shall have the

	s ° ′ ″
True longitude of the luminaries	5. 14. 47. 40,7
True latitude of the moon (north)	44. 39,4
Moon's horary motion from the sun	27. 1,7
—— horary motion in latitude (decreasing) ..	2. 42,0
—— horizontal parallax	53. 53,0
—— semidiameter	14. 41,0
Sun's semidiameter	15. 54,8
—— horizontal parallax	8,7
—— declination (north)	5. 59. 41,0

From these elements it may be determined that the moon's shadow first touches the earth's disc at $11^h. 23'$ A.M. *apparent* time at Greenwich, in N. lat. 59°. 43′, W. long. 90°. 50′ *; and that it finally leaves it at $4^h. 39'\frac{3}{4}$ P.M. *apparent* time at Greenwich in N. lat. 3°. 21′, E. long. 20°. 25′. Consequently the total duration of the general eclipse to the inhabitants of the earth will be about $5^h. 17'$: but, at no one place in particular will the duration be much more than half that time.

The *central* path of the moon's shadow across the earth's disc, which is the most material circumstance in inquiries of this nature (since it serves to point out those parts of the world where the eclipse will be seen annular) may be determined with considerable accuracy from the principles laid down by M. Delambre in his *Traité d'Astronomie* (vol. ii. page 384): and, agreeably to the formulæ which he has there given, I have carefully computed the following table, which shows the several points (expressed by positions of latitude and longitude) through which the centre of the moon's shadow will pass in its progress across the earth's disc, at the several times therein mentioned. The first column denotes the *apparent* time at Greenwich, at the moment when the centre of the moon's shadow passes the given points laid down in the second and third columns: and the last column shows the corresponding apparent time *at those places*.

* All the Longitudes in this memoir are reckoned from the meridian of Greenwich.

Apparent

Apparent time at Greenwich. P.M.	Latitude North.	Longitude from Greenwich.	*Apparent* time at the place.
h. ′ ″	° ′ ″	° ′ ″	h. ′ ″
12. 54. 39	81. 39. 29	W. 149. 32. 55	2. 56. 27 A.M.
55. 0	83. 39. 34	129. 44. 37	4. 16. 2
1. 0. 0	82. 24. 34	42. 38. 12	10. 9. 27
8. 16	76. 6. 21	17. 3. 15	12. 0. 0
10. 0	75. 23. 11	14. 31. 41	12. 11. 53 P.M.
20. 0	69. 9. 41	5. 52. 11	12. 56. 31
30. 0	64. 13. 27	0. 46. 50	1. 26. 53
40. 0	59. 47. 31	E. 2. 50. 42	1. 51. 23
50. 0	55. 44. 40	5. 48. 32	2. 13. 14
2. 0. 0	51. 56. 26	8. 26. 4	2. 33. 44
10. 0	48. 18. 42	10. 57. 6	2. 53. 48
20. 0	44. 49. 25	13. 32. 32	3. 14. 10
30. 0	41. 25. 32	16. 23. 32	3. 35. 34
40. 0	38. 3. 53	19. 44. 0	3. 58. 56
50. 0	34. 40. 41	23. 59. 43	4. 25. 59
3. 0. 0	31. 7. 19	30. 24. 13	5. 1. 37
8. 11	27. 10. 30	46. 2. 4	6. 12. 19

From this table it will be seen that the *central* eclipse commences in N. lat. 81°. 39′. 29″, W. long. 149°. 32′. 55″, when the sun and moon will rise together (the centre of the moon being directly on the centre of the sun's disc) to the inhabitants of that part of the globe, at 2^h. 56′. 27″ in the morning, corresponding to 12^h. 54′. 39″ (or 0^h. 54′. 39″*) in the afternoon at Greenwich: that the sun will be centrally eclipsed on the meridian (or exactly at noon) in N. lat. 76°. 6′. 21″, W. long. 17° 3′ 15″, when it is 1^h. 8′. 16″ in the afternoon at Greenwich: and that the sun will set centrally eclipsed in N. lat. 27°. 10′. 30″, E. long. 46°. 2′. 4″ at 6^h. 12′. 19″ in the afternoon, corresponding to 3^h. 8′. 11″ at Greenwich.

If the points, mentioned in the second and third columns of the above table, be marked on a good map, and lines be drawn connecting these points, we shall have the path of the centre of the moon's shadow across the globe. Whence it will be seen that the centre of the shadow, having entered the earth's disc

* That is, 54′. 39″ after 12 o'clock *at noon.* The English astronomers begin the day at noon; but the French reckon from midnight, as in the civil mode of reckoning. There cannot however be any ambiguity, in the present case, as to the 12^h.

near

near the north pole*, will proceed between the Shetland islands and the coast of Norway down the north sea, and enter the continent of Europe on the coast of Westphalia, about half way between the Ems and the Weser. It will thence proceed, nearly in a straight line, across Germany and the Tyrol country, and enter the gulf of Venice about midway between Trieste and Venice. Traversing that gulf it will cross the heel of Italy; and, after skirting the coast of the Morea and Candia, will pass directly over Alexandria in Egypt; and finally leave the earth in Arabia near the Persian gulf.

If we set off two other lines on the map, parallel to this central line, one on each side thereof, and each at the distance of about 130 geographical miles from the central line, the intermediate space between these two boundary lines will nearly† represent the path of the moon's *umbra;* and will show all those places where the eclipse will be seen *annular*, or where the whole body of the moon will appear on the face of the sun. Some uncertainty, however, may exist with respect to those towns which are situated near the *borders* of the umbra, such as Rotterdam, Aix la Chapelle, Liege, Treves, Freyburg, Parma, Rome, and other places on the one side; and Magdeburg, Leipsic, Ragusa, Athens, and other places on the other side of the central path: since the eclipse may or may not be annular in the neighbourhood of those towns according to circumstances. Nevertheless, at all those places, and indeed to the whole of Europe and to a great part of Asia and Africa, the eclipse will be visible; differing only in magnitude according to the situation of the spectator. But, in no part will it be annular except at those places which are situated within the limits of the umbra, as above mentioned‡.

Those persons, who happen to be situated on the *western* border of the umbra, will, at the time of the middle of the eclipse, see the *upper* limb of the moon in contact with the *upper* limb of the sun; and consequently the unobscured portion of the sun's disc will be seen round the under part of the moon. On

* It will traverse the supposed *polar basin,* and the north east coast of Greenland, the object of so much laudable curiosity at the present moment: so that if the adventurous navigators to those parts should not have returned before the date of this eclipse, they will probably observe it in those high latitudes.

† It must be evident to those acquainted with the principles of astronomy that the umbra will not be exactly of the *same* width in any two points of its course; but will be constantly varying. It will not, however, undergo any material alteration in its progress across the continent of Europe.

‡ For a general view of the path of the umbra across the continent of Europe, see Plate II.

the

the contrary, those persons who are on the *eastern* confines of the umbra will see the *lower* limb of the moon in contact with the *lower* limb of the sun. Whilst, to those who are stationed directly in the central path, the centres of the sun and the moon will appear *exactly* to coincide; and an uniform luminous ring, equal in breadth to about one twenty-ninth part of the sun's diameter, will surround the body of the moon*.

As there are no two points on the face of the globe where the visible appearances of any solar eclipse are exactly alike, it would be an endless task to compute the phænomena for any considerable number of places: and the usual mode, amongst astronomers, is to give a general outline of the path of the moon's shadow, and to calculate the particular circumstances of the eclipse for the metropolis only, or for some known observatory; which calculation may be easily adapted to other parts of the kingdom. The notices which are given in the various ephemerides, on this point, are merely for the purpose of informing astronomers to *look out* for, and *note down* these phænomena: and the observations, thus made, are afterwards collected and compared together. Under these circumstances the reader must not expect to find the exact time and appearances of this eclipse computed for every place on the continent. It will be sufficient for his purpose if he knows at what time of the day he ought to look out for its commencement; and at what point of the sun's disc he ought to fix his attention in order to observe the first point of contact. The following table will show nearly these several particulars for the different places therein mentioned; and will assist the observer in his computations for any other place within the umbra. These values are deduced merely from a *projection* of the eclipse, and are consequently given as approximations only, and by no means as the *exact* values. For, where it is required to have the time true to the nearest second, the observer must calculate the phases of the eclipse for the precise spot where he happens to be stationed.

The angles from the vertex are all reckoned on the right hand side of the sun; as the moon always makes the first impression on the sun's disc, on that side.

* The sun will be elevated, on that day, above the horizon about 34 degrees, to that part of the continent, over which the centre of the moon's umbra passes: consequently the *increase* of the moon's semidiameter will be about 7¼ seconds.

Place

Place of Observation.	Time of commencement.		Angle from the Vertex.
	Mean time at Greenwich. P.M.	*Mean* time at the place. P.M.	
	h. ′	h. ′	°
Lerwick (Shetland)	12. 9	12. 3	52
Bergen (Norway)	15	37	61
Amsterdam ..	26½	46	58
Aix la Chapelle ..	30	54	61
Hamburg	31	1. 11	65
Leipsic	37	26½	71
Frankfort	34	8½	64
Lintz	44½	41½	76
Munich	42	28	70
Zurich	39	13	65
Venice	48	37½	72
Florence	50	35	72
Rome	56	46	76
Naples	1. 1	58	81
Athens	18	2. 53	97
Alexandria	40	3. 40	109

The whole duration of the eclipse will, at all these places, be rather more than two hours and three quarters. But the *duration of the annulus* will not, in any place, exceed six minutes: and, in some places (at the confines of the umbra) it will be momentary. The nearer the spectator happens to be to the centre of the path of the moon's umbra, the longer will the annular appearance continue.

Having thus given a general outline of this eclipse, I shall proceed to state the principal phænomena which have been observed in former eclipses of this kind *, whereby the reader may be aware of the principal observations to which it will be proper for him to attend, and make his preparations accordingly. Many of these phænomena have given rise to much discussion, and are far from being accurately determined, or reduced to general principles. There is also a degree of doubt respecting the existence of some of them. Those persons therefore who are furnished with convenient instruments, and have a favourable opportunity, should carefully attend to, and note down, not merely the phases of the eclipse but such other appearances as may present them-

* These of 1737 and 1748. There are but few observations of the eclipse of 1764.

selves.

selves. It is only by multiplying observations of this kind that we can ultimately arrive at the truth: and an annular eclipse is so rare an occurrence in this part of the globe, that it is hoped every advantage will be taken of it, to improve and advance the connected sciences of astronomy and geography.

There is one important observation however connected with this eclipse, which it is in the power of almost any competent person to make, without the aid of any particular instruments: I allude to the formation and dissolution of the annulus *. This may be determined very accurately, if not by the naked eye, at least with a telescope of *very small* magnifying power †; furnished with a coloured glass to keep off the rays of the sun, or with a glass smoked in the manner hereafter mentioned ‡. The times of these phases may be determined with sufficient accuracy by means of a clock, or watch that beats seconds; and which should, if possible, be set to mean time on the day of the eclipse. The neglect of this precaution, however, should not prevent the observer from noting down the *duration* of the annular appearance; which will be the same, whether the watch is right or not §. As the method therefore of observing this phænomenon is so simple and easy, it is hoped that no person, to whom the opportunity may occur, will omit to note down the particulars; or fail to communicate the same to some person conversant with the subject of astronomy. It will be of equal importance to know that the existence of the annulus is only *momentary*: or even that it is *nearly*, but *not completely* formed ‖.

Although the possession of proper instruments must give a superior degree of credit to the observations of any person; yet I would

* The annulus is considered as completely *formed* when the whole body of the moon just appears on the disc of the sun, however unequal in breadth the uncovered part of the sun's disc may be. It is considered as *dissolved* the moment the moon again touches the concave circumference of the sun's disc. The *duration* of the annulus will not in any place, as already observed, exceed six minutes, and in some places will be momentary.

† A common opera glass might be made use of, if nothing better should present itself: as no method should remain untried for determining this very important phase. If the observer be near-sighted, and have not the advantage either of telescope or concave glasses, he may view the sun through a small hole made in a card by means of a pin.

‡ Those, who cannot procure either coloured or smoked glass, may view the image of the sun in a bucket of water, or a vessel filled with oil, placed in a situation where it may not be agitated by the wind.

§ Should the observer be in such a situation as not to have the advantage of either a clock, or a watch beating seconds, he might easily make a temporary pendulum, of any convenient length, and notice the number of vibrations which it makes during the existence of the annulus. In such case, the length and substance of the pendulum should be specified.

‖ To those who are not much conversant with practical astronomy, it may perhaps be proper to remark, that the more *numerous* these observations may would not discourage those who have not this advantage, from communicating any circumstances that may occur. For it has been justly observed by M. De L'isle, that although no great dependance can be placed on those observations which are not made with a telescope, &c. yet that such observations as are made with the naked eye ought not to be entirely neglected; since it affords us an opportunity of judging of the accuracy of those observations which were made before telescopes, &c. were invented.

Those persons, however, who have the proper instruments, and every conveniency for observing, will of course note down the usual circumstances in such case: viz.

1. The time of the commencement of the eclipse*;
2. The time of the formation of the annulus;
3. The time of the dissolution of the annulus †;
4. The time of the end of the eclipse.

If there should be any *spots* on the sun, it will be proper (previous to the commencement of the eclipse) to make a diagram of the sun's disc; and to note down the times when the body of the moon comes in contact with the spots, and likewise the times when they again become visible. All these may be determined with sufficient accuracy by the assistance of a telescope magnifying thirty or forty times; together with a well regulated clock or watch that beats seconds; and which, if possible, should as

may be (that is, the greater the number of *places* where they are made), the more important will be the consequences to be derived from them. Consequently *every* observation will be material. Those persons who may observe the eclipse in the country, should state the distance and position of such place from the nearest principal town.

* It is rather difficult to determine the *exact* time of the *commencement* of any solar or lunar eclipse; since the impression on the disc does not become visible till some seconds after the eclipse has begun. The field of the telescope should take in at least one-half of the circumference of the sun's disc (taking that portion of it, which may leave the expected point of contact in the centre), as the eye can much better judge of any impression made upon a large, than a small, portion of a circle. In some cases however a *very powerful* telescope (which takes in only a small portion of the sun's disc) may be attended with advantage, as in the case of the solar eclipse on September 5, 1793, where Sir Wm. Herschel observed that the first impression on the sun's disc was made by the projection of two high mountains of the moon, having the appearance of *horns*; which were distinctly visible on the sun's disc before the body of the moon appeared.—Phil. Trans. 1794, p. 39.

† In order to determine, with greater accuracy, the formation and dissolution of the annulus, the observer should take into his telescope that part only of the disc of the sun which is necessary for the purpose. By adopting this method Mr. Maclaurin, in 1737, was enabled to observe the appearance alluded to in page 98; and which preceded the perfect formation of the annulus about twenty seconds: thereby enabling him to look out for and note down the exact time, with greater precision.

before

before observed be set to mean time on the day of the eclipse*. I must again repeat however, that the neglect of this precaution should not deter the observer from noting down the *duration* of the several phases above mentioned, and particularly the times at which the *annulus* is *formed* and *dissolved:* which may be afterwards compared with more exact observations, and lead to a correction of the true times†.

It is presumed that the observer will also, from time to time, during the progress of the eclipse, observe and note down the distance and inclination of the cusps in the usual manner‡. It may likewise be proper to remark that it will be of considerable importance to ascertain, at the time of the middle of the eclipse, the magnitude of the annulus on the north and on the south side of the moon, in order to determine how far distant, at that time, the centre of the moon is from the centre of the sun§. If at the same moment the observer can determine the diameter of the sun and moon, it will add considerably to the importance of the observation; and tend to determine a much disputed point in practical astronomy‖. These observations however should be made with a good telescope furnished with an accurate micrometer: and, in making a report thereof, the observer should describe the kind of telescope made use of, as well as the method employed in determining the magnitude of the annulus, &c. For the sake of

* In the evening of the same day on which this eclipse takes place there will be an eclipse of the first satellite of Jupiter: the immersion will take place at Greenwich at $8^h.34'.34''$ mean time. Those persons therefore who are furnished with sufficiently powerful telescopes, may (if the weather prove favourable) have an opportunity of ascertaining the correctness of their clocks or watches.

† M. De L'isle states, that if we observe the *situation* of the cusps, or only their distance, at the time of the middle of the eclipse (when the eclipse is not annular, but nearly so), it will serve to determine the apparent route of the penumbra and its limits, as exactly as if we had observed the duration of the annular eclipse.

‡ There are two modes of observing the phases of an eclipse of the sun: the one, by looking directly at the sun, with a telescope furnished with a micrometer: the other, by receiving the image of the sun, through a telescope, on a screen, in a dark chamber, or *camera obscura*. Each has its advantages, and may be practised according to circumstances. See Lalande's *Astronomie*, vol. ii. p. 659. M. De L'isle indeed says that "we may determine, with sufficient exactness, the *situation* of the cusps, without making use either of the dark chamber or the micrometer, by observing the moment of the passage of the cusps and of the limbs of the sun, by means of simple wires placed in the focus of the telescope, in any situation whatever; and leaving the telescope in a fixed position, during the time that the sun employs to traverse the field of it."

§ M. De L'isle doubts whether this part of the observation can be made with sufficient accuracy in a dark chamber; on account of the indistinctness of the image of the moon.

‖ See pages 97 and 98.

greater

greater accuracy, he should also make a diagram of the appearance of the sun and moon, at the time of the middle of the eclipse; placing a mark against that part of the sun's disc which appears the most vertical to him. The point on the circumference of the sun's disc (relative to its vertical or horizontal diameter) where the moon leaves it in order to form the annulus, and again where it touches it at the time of the dissolution of the annulus, should also, if possible, be distinctly noted. M. Le Monnier considers this of considerable importance*.

It was observed, in the annular eclipse of 1737, that the annulus was formed and dissolved very *suddenly*. For when the whole body of the moon had entered on the disc of the sun, the last portion that entered appeared to adhere to the concave circumference of the sun's disc for some seconds; and the moon appeared elongated on that side, till the sun's light suddenly broke round it, when the moon reassumed its regular curvature. In a similar manner, when the disc of the moon approached the concave line of the sun's disc on the other side, they seemed to run together like two contiguous drops of water on a table, when they touch each other.

It was also observed, in the eclipse of 1737, "that, as the annulus was forming, the light appeared to break in several irregular spots near the point of contact: and that the limb of the moon seemed to be indented there." These irregular parts seemed likewise to have a kind of motion; although there was no undulation at the same time in the circumference of the sun. Such appearances of a tremulous motion, in certain periods of solar eclipses, are mentioned by Hevelius and others. It was noticed also in the eclipse of 1748†.

In both these eclipses as well as in that of 1764 it was observed that, when the annulus was formed, the moon appeared much *smaller* on the sun than it really ought to be: and indeed much smaller than the calculations seemed to warrant. But

* In his paper *Sur l'utilité des éclipses de soleil* (wherein he has drawn many important consequences from the eclipse of 1748) he remarks, respecting the method of determining the limits of the umbra, that "la plupart des observateurs, en pareil cas, suivent les routes ordinaires, et n'ont jamais fait assez d'attention au point de la circonférence du limbe du soleil où se forment les ruptures de l'anneau : desormais ces points de la circonférence du disque du soleil seront les plus importans, et nous fourniront les limites que nous voudrons bien assigner."—*Mem. de l'Acad. des Sciences* for 1765, page 463.

† The Rev. Mr. Irwin, who noticed the eclipse of 1748 at Elgin, says that "the formation and breaking of the annulus were sensibly to be observed, and passed *in a moment*; affording a very pleasing sight by the irregular tremulous spots of the sun."—Phil. Trans. vol. xlv. page 595.

whether this phænomenon arises from an apparent enlargement of the sun's disc, or from an apparent diminution of the moon's disc, or from both, does not seem clearly decided. M. Du Séjour has discussed this subject, with his usual ability, in his *Traité analytique des mouvemens apparens des corps célestes*, vol. i. page 405, &c.: but he has not come to any precise determination thereon. The observations have not been made with sufficient accuracy, nor are they sufficiently numerous to enable us to determine so nice an element in the calculation of eclipses. It is hoped therefore that the attention of astronomers will be more drawn towards this subject in the ensuing eclipse *.

In the eclipse of 1737, Maclaurin observes that about 20 seconds "before the annulus was complete, a remarkable point or speck of pale light appeared near the middle of the part of the moon's circumference that was not yet come upon the disc of the sun: and a gleam of light, more faint than that point, seemed to extend from it to each horn."

In the eclipse of 1748, it was noticed that there was, "about the middle of the eclipse, a remarkably large spot of light, of an irregular figure and of a considerable brightness, about 7′ or 8′ within the limb of the moon." Mr. Short states that this eclipse was not quite annular at Aberdour Castle: the cusps seemed to want about one-seventh of the moon's circumference to be joined, yet a brown light was plainly observed both by my Lord Morton and myself to proceed or stretch along the circumference of the moon, from each of the cusps, about one-third of the whole distance of the cusps from each cusp; and there remained about one-third of the whole distance of the cusps not enlightened by this brown light."—"I observed at the extremity of this brown light, which came from the western cusp, a larger quantity of light than in any other place, which at first surprised me; but afterwards I imagined it must have proceeded from some cavity or valley made by two adjoining mountains on the edge or limb of the moon. I had often formerly observed mountains on the circumference of the moon, more or less every where round it, but never saw them so plain as during the time of this eclipse. The mountainous inequalities on the southern limb of the moon were particularly remarkable; in some parts mountains and valleys alternately; others extended a considerable way along the circum-

* See Lalande's *Astronomie*, vol. ii. page 445; Delambre's *Astronomie*, vol. ii. page 423; and also M. Le Monnier's memoir *Sur les éclipses totales du soleil*, in the *Mem. de l'Acad. des Sciences* for 1781, page 243. In this memoir there is a map of the path of the moon's umbra in the total eclipse of May $\frac{11}{22}$, 1724, and which appears to have proceeded over great part of England: nevertheless I cannot find any observation of it in this country.

circum-

circumference and ended almost perpendicularly like a precipice. My Lord Morton was able to see them very easily through his small reflector."

The king of France, who (as already mentioned) went purposely to Compiegne to observe this eclipse, discovered towards the middle of the eclipse (which was not more than 9½ digits) "sur la surface de la lune, comprise entre les cornes du soleil, des rayons de lumière rouges, et un filet de lumière qui sembloit masquer le disque de la lune, et qui s'étendoit à une distance des cornes *."

M. De L'isle, in his publication above alluded to, seems to think that a quick eye, guarded with a sufficiently dark glass, might in solar eclipses discover the body or limb of the moon seven or eight minutes before it touched the sun, and also for the same time after it had left it and was entirely off the sun. He remarks that the observer should defend himself as much as possible from the direct light of the sun, and also from the light of the external air. No person however has hitherto noticed such an appearance: although many observers attended particularly to it, in the eclipse of 1748, in consequence of M. De L'isle's remarks. Should the moon in such case ever be visible, it would enable us to determine with greater accuracy the commencement of any solar eclipse †.

During the progress of the eclipse it would be desirable to ascertain the degree of cold and obscurity caused by the diminution of the sun's rays: for which purpose, preparations should be made beforehand, in order that no time be lost during the period of the eclipse. The variations in the thermometer and barometer may be easily noted down without interrupting the astronomical observations. The rapid change in the temperature of the air may cause a hurricane of wind, (together with rain or snow) as was observed about the middle of the eclipse by Mr. Maclaurin in 1737; and by Le Monnier in 1748. Mr. Short says that (in the eclipse of 1748) "we did not at all perceive or feel any greater degree of cold, during the eclipse, than we felt before it began." But M. Cassini De Thury, who went with the king of France to Compiegne to observe this eclipse, and where it was only 9½ digits, says they experienced a considerable degree of cold at the time of the middle of the eclipse; the ther-

* *Mem. de l'Acad. Roy. des Sciences*, 1748, page 56.

† It must be evident to the practical astronomer, that if the moon were really visible in such cases, she would also be frequently visible at the conjunctions, when no eclipse took place. M. De L'isle's suggestion arose from a remark made by M. Cassini on a luminous ring which was seen to surround Mercury in its passage across the sun's disc in the year 1736, and which continued for 6 or 7 seconds after Mercury was entirely off the sun's disc.—See *Mem. de l'Acad. des Sciences* for 1736, page 373.

mometer however fell only 2½ degrees: and the Abbé Nollet found that his burning-glass was then as powerful as before the eclipse began. M. De L'isle, likewise, who observed this eclipse at the Luxembourg, remarks that the thermometer did not indicate any increase of cold caused by the eclipse, although he and many other persons experienced it soon after the middle of the eclipse *.

In the eclipse of 1737 Maclaurin observed that a burning-glass which kindled tinder and burnt cloth towards the end of the eclipse, had no effect during the existence of the annulus, nor for some time before and after it. He likewise remarked that "during the appearance of the annulus, the direct light of the sun was still very considerable: and that although some places, that were shaded from his light, appeared gloomy, yet that the day light was not greatly obscured." He adds that many persons, about the middle of the annular appearance, although not short-sighted, were unable to discover the moon upon the sun when they looked without a smoked or coloured glass †. Nevertheless Venus and some other stars were visible at the same time: and Venus continued visible even after the annulus was dissolved. Venus was also seen in the eclipse of 1748, but it does not appear that any other star was then visible.

If the diminution of light should be considerable (which there is not much reason however to suspect ‡), Mercury, Venus, and Mars, together with some of the principal fixed stars may be visible to the naked eye. Mercury, if visible, will be seen about 18° to westward of the sun, nearly in conjunction with *Regulus:*

* In the total eclipse of 1724 the thermometer had fallen only two degrees at the time of the middle of the eclipse. This is the more remarkable as the eclipse took place late in the afternoon of the 22d of May, at which time we might presume that the atmosphere would be gradually becoming more cool. The total darkness took place at $6^h. 48'$. P.M.

† M. Le Monnier mentions the same thing of himself in the eclipse of 1748.

‡ In the annular eclipse of 1764 an ignorant country clergyman alarmed the people of France by announcing that there would be *total darkness* during the existence of the annulus: and the Royal Academy of Sciences at Paris thought proper to give this report a formal contradiction. It is well known however that the smallest ray of light from the sun would prevent such a phænomenon; as I have shown more at length in my paper "On the solar eclipse which is said to have been predicted by Thales," inserted in the Phil. Trans. for 1811, part ii. page 220.

I shall here take the opportunity of correcting a typographical error in that paper; where, in page 240, line 22, "three *degrees*" should be "three *minutes*." Since the publication of that paper, I find that the *Bureau des Longitudes* in France have printed a *Supplement* to M. Burgh's Lunar Tables, wherein the mean epoch and mean motion of the Supplement of the Node are considerably altered; so as to bring the latitude of the moon within the limits which I there suggested.

Venus

Venus will be about 41° to westward of the sun*: and Mars about 35° to eastward of the sun, not far from *Spica Virginis.* The observer should also look out for any comet which may be visible during this eclipse; and be prepared to measure its distance from the sun or a principal fixed star.

As many persons may be so situated as not to be able to procure any dark coloured glass, for the purpose of viewing the sun, I shall conclude this memoir by inserting Dr. Maskelyne's method of *smoking* glasses, which he published in the Nautical Almanac for 1769, in his *Instructions relative to the observation of the transit of the planet Venus over the sun's disc* in that year.

"Dark glasses should be used to defend the eye from the intensity of the sun's light. Transparent glasses, smoked over the flame of a candle or lamp, will give a more distinct and agreeable vision of the disc of the sun than any tinged or coloured glasses will do. Provide two pieces of glass of convenient length, not too thick (the common crown glass, used for windows, will do as well as any), wipe them clean and dry. Warm them a little by the fire (if the weather be cold) to prevent their cracking when applied to the flame of the candle: then draw one of them gently, according to its whole length, through the flame; and part of the smoke will adhere to the glass. Repeat the same operation, only leaving a little part at one end now untouched; repeat the operation, leaving a further part at the same end untouched, and so each time leave a further part of the same end untouched, till at last you have tinged the glass with several dyes, increasing gradually in blackness from one end to the other. Smoke the other glass in like manner; and apply the two glasses, one against the other, only separated by a rectangular border, cut of brass or card paper, the smoked faces being opposed to each other, and the deepest tinges of both placed together at the same end. Tie the glasses firmly together with waxen thread, and they are ready for use. The tinge at one end should be the slightest possible, and at the other end so dark that you cannot see the candle through it. By this contrivance, applied between your eye and the sun, you will have the advantage not only of seeing the sun's light white, according to its natural colour, and his image more distinct than through common dark glasses, but also of being able to intercept more or less of his light as you please, and as the clearness or thickness of the air requires it, by bringing a darker or lighter part of this combined

* In the total eclipse of 1715, Venus was seen when only 9 digits were eclipsed: but she was not seen at Compiegne in the eclipse of 1748, although the digits eclipsed were 9¼: in the eclipse of 1724 however she was distinctly visible when only 6 digits were eclipsed. This is not remarkable, as she is, in some situations, visible even at mid-day.

dark glass before your eye: which will be a great convenience at all times, but particularly when the brightness of the sun is liable to sudden changes from flying clouds."

I shall merely add, that it is to be hoped the sovereigns of the different provinces and states, mentioned in this memoir (p. 91), will encourage persons from the neighbouring countries to enter and observe this eclipse: and that the love of science will induce them to prevent such persons from being subject to any tarif, or vexatious delay at the Custom-house, on account of any astronomical or philosophical instruments which they may take with them for the purposes of observation.

XIII. *On certain fraudulent and poisonous Sophistications. By Mr.* Fredrick Accum*.

Counterfeit Pepper.

Black Pepper is the fruit of a shrubby creeping plant, which grows wild in the East Indies, and is cultivated, with much advantage, for the sake of its berries, in Java and Malabar. The berries are gathered before they are ripe, and are dried in the sun. They become black and corrugated on the surface.

That factitious pepper-corns have of late been detected mixed with genuine pepper, is a fact sufficiently known †. Such an adulteration may prove, in many instances of household œconomy, exceedingly vexatious and prejudicial to those who ignorantly make use of the spurious article. I have examined large packages of both black and white pepper, by order of the Excise, and have found them to contain about 16 per cent. of this artificial compound. The spurious pepper is made up of oil cakes (the residue of lintseed, from which the oil has been pressed), common clay, and a portion of Cayenne pepper, formed in a mass, and granulated by being first passed through a sieve, and then rolled in a cask. The mode of detecting the fraud is easy. It is only necessary to throw a sample of the suspected pepper into a bowl of water; the artificial pepper-corns fall to powder, whilst the true pepper remains whole.

Ground pepper is very often sophisticated by adding to a portion of genuine pepper, a quantity of pepper dust, or the sweepings from the pepper warehouses, mixed with a little Cayenne pepper. The sweepings are known, and purchased in the market, under the name of P. D. signifying pepper dust. An in-

* From Treatise on Adulterations of Food and on Culinary Poisons.

† Thomson's Annals of Chemistry, 1816; also Repository of Arts, vol. i. 1816, p. 11.

ferior

ferior sort of this vile refuse, or the sweepings of P. D. is distinguished among venders by the abbreviation of D. P. D. denoting dust (dirt) of pepper dust.

The adulteration of pepper, and the making and selling commodities in imitation of pepper, are prohibited, under a severe penalty. The following are the words of the Act*:

"And whereas commodities made in imitation of pepper have of late been sold and found in the possession of various dealers in pepper, and other persons in Great Britain; be it therefore enacted, that from and after the said 5th day of July 1819, if any commodity or substance shall be prepared by any person in imitation of pepper, shall be mixed with pepper, or sold or delivered as and for, or as a substitute for, pepper, or if any such commodity or substance, alone or mixed, shall be kept for sale, sold, or delivered, or shall be offered or exposed to sale, or shall be in the custody or possession of any dealer or seller of pepper, the same, together with all pepper with which the same shall be mixed, shall be forfeited, with the packages containing the same, and shall and may be seized by any officer of excise; and the person preparing, manufacturing, mixing as aforesaid, selling, exposing to sale, or delivering the same, or having the same in his, her, or their custody or possession, shall forfeit the sum of one hundred pounds."

White Pepper.—The common white pepper is factitious, being prepared from the black pepper in the following manner:—The pepper is first steeped in sea water and urine, and then exposed to the heat of the sun for several days, till the rind or outer bark loosens; it is then taken out of the steep, and, when dry, it is rubbed with the hand till the rind falls off. The white fruit is then dried, and the remains of the rind blown away like chaff. A great deal of the peculiar flavour and pungent hot taste of the pepper is taken off by this process. White pepper is always inferior in flavour and quality to the black pepper.

However, there is a sort of native white pepper, produced on a species of the pepper plant, which is much better than the factitious, and indeed little inferior to the common black pepper.

Poisonous Cayenne Pepper.—Cayenne pepper is an indiscriminate mixture of the powder of the dried pods of many species of capsicum, but especially of the *capsicum frutescens*, or bird pepper, which is the hottest of all.

This annual plant, a native of South America, is cultivated in large quantities in our West-India islands, and even frequently in our gardens, for the beauty of its pods, which are long, pointed, and pendulous, at first of a green colour, and, when ripe, of a

* George III. c. 53. § 21, 1819.

bright orange red. They are filled with a dry loose pulp, and contain many small, flat, kidney-shaped seeds. The taste of capsicum is extremely pungent and acrimonious, setting the mouth, as it were, on fire.

The principle on which its pungency depends, is soluble in water and in alcohol.

It is sometimes adulterated with red lead, to prevent it becoming bleached on exposure to light. This fraud may be readily detected by shaking up part of it in a stopped vial containing water impregnated with sulphuretted hydrogen gas, which will cause it speedily to assume a dark muddy black colour. Or the vegetable matter of the pepper may be destroyed, by throwing a mixture of one part of the suspected pepper and three of nitrate of potash (or two of chlorate of potash) into a red hot crucible, in small quantities at a time. The mass left behind may then be digested in weak nitric acid, and the solution assayed for lead by water impregnated with sulphuretted hydrogen.

Poisonous Pickles.—Vegetable substances, preserved in the state called pickles, by means of the antiseptic power of vinegar, whose sale frequently depends greatly upon a fine lively green colour; and the consumption of which, by sea-faring people in particular, is prodigious, are sometimes intentionally coloured by means of copper. Gerkins, French beans, samphire, the green pods of capsicum, and many other pickled vegetable substances, oftener than is perhaps expected, are met with impregnated with this metal. Numerous fatal consequences are known to have ensued from the use of these stimulants of the palate, to which the fresh and pleasing hue has been imparted according to the deadly *formulæ* laid down in some modern cookery books, such as boiling the pickles with halfpence, or suffering them to stand for a considerable period in brazen vessels.

Dr. Percival* has given an account of "a young lady who amused herself, while her hair was dressing, with eating samphire pickles impregnated with copper. She soon complained of pain in the stomach; and, in five days, vomiting commenced, which was incessant for two days. After this, her stomach became prodigiously distended; and, in nine days after eating the pickle, death relieved her from her suffering."

Among many recipes which modern authors of cookery books have given for imparting a green colour to pickles, the following are particularly deserving of censure; and it is to be hoped that they will be suppressed in future editions of the works.

"*To pickle Gerkins*†.—"Boil the vinegar in a bell-metal or copper pot; pour it boiling hot on your cucumbers."

* Medical Transactions, vol. iv. p. 80.

† The Ladies' Library, vol. ii. p. 203.

"To

"*To make greening**.—"Take a bit of verdigris, the bigness of a hazel-nut, finely powdered; half-a-pint of distilled vinegar, and a bit of alum powder, with a little bay salt. Put all in a bottle, shake it, and let it stand till clear. Put a small teaspoonful into codlings, or whatever you wish to green."

Mrs. E. Raffeld † directs, "to render pickles green, boil them with halfpence, or allow them to stand for twenty-four hours in copper or brass pans."

To detect the presence of copper, it is only necessary to mince the pickles, and to pour liquid ammonia, diluted with an equal bulk of water, over them in a stopped phial: if the pickles contain the minutest quantity of copper, the ammonia assumes a blue colour.

Adulteration of Cream.—Cream is often adulterated with rice powder or arrow-root. The former is frequently employed for that purpose by pastry-cooks, in fabricating creams and custards, for tarts, and other kinds of pastry. The latter is often used in the London dairies. Arrow-root is preferable to rice powder; for, when converted with milk into a thick mucilage by a gentle ebullition, it imparts to cream, previously diluted with milk, a consistence and apparent richness, by no means unpalatable, without materially impairing the taste of the cream.

The arrow-root powder is mixed up with a small quantity of cold skimmed milk into a perfect, smooth, uniform mixture; more milk is then added, and the whole boiled for a few minutes, to effect the solution of the arrow-root: this compound, when perfectly cold, is mixed up with the cream. From 220 to 230 grains (or three large tea-spoonsful) of arrow-root are added to one pint of milk; and one part of this solution is mixed with three of cream. It is scarcely necessary to state, that this sophistication is innocuous.

The fraud may be detected by adding to a tea-spoonful of the sophisticated cream a few drops of a solution of iodine in spirit of wine, which instantly produces with it a dark blue colour. Genuine cream acquires, by the addition of this test, a faint yellow tinge.

Poisonous Confectionary.—In the preparation of sugar plums, comfits, and other kinds of confectionary, especially those sweetmeats of inferior quality frequently exposed to sale in the open streets, for the allurement of children, the grossest abuses are committed. The white comfits, called sugar pease, are chiefly composed of a mixture of sugar, starch, and Cornish clay (a species of very white pipe-clay); and the red sugar drops are usually coloured with the inferior kind of vermilion. This pigment is ge-

* Modern Cookery, or The English Housewife—2d edition, p. 94.

† The English Housekeeper, p. 352, 354.

nerally

nerally adulterated with red lead. Other kinds of sweetmeats are sometimes rendered poisonous by being coloured with preparations of copper. The following account of Mr. Miles* may be advanced in proof of this statement:

"Some time ago, while residing in the house of a confectioner, I noticed the colouring of the green fancy sweetmeats being done by dissolving sap-green in brandy. Now sap-green itself, as prepared from the juice of the buckthorn berries, is no doubt a harmless substance; but the manufacturers of this colour have for many years past produced various tints, some extremely bright, which there can be no doubt are effected by adding preparations of copper.

"The sweetmeats which accompany these lines you will find exhibit vestiges of being contaminated with copper.—The practice of colouring these articles of confectionary should, therefore, be banished; the proprietors of which are not aware of the deleterious quality of the substances employed by them."

The foreign conserves, such as small green limes, citrons, hoptops, plums, angelica roots, &c. imported into this country, and usually sold in round chip boxes, are frequently impregnated with copper.

The adulteration of confitures by means of clay, may be detected by simply dissolving the comfits in a large quantity of boiling water. The clay, after suffering the mixture to stand undisturbed for a few days, will fall to the bottom of the vessel; and on decanting the clear fluid, and suffering the sediment to become dry gradually, it may be obtained in a separate state. If the adulteration has been effected by means of clay, the obtained precipitate, on exposure to a red heat in the bowl of a common tobacco-pipe, acquires a brick hardness.

The presence of copper may be detected by pouring over the comfits liquid ammonia, which speedily acquires a blue colour, if this metal be present. The presence of lead is rendered obvious by water impregnated with sulphuretted hydrogen, acidulated with muriatic acid, which assumes a dark brown or black colour, if lead be present.

Poisonous Catsup.—This article is very often subjected to one of the most reprehensible modes of adulteration ever devised. Quantities are daily to be met with, which, on a chemical examination, are found to abound with copper. Indeed, this condiment is often nothing else than the residue left behind after the process employed for obtaining distilled vinegar, subsequently diluted with a decoction of the outer green husk of the walnut, and seasoned with all-spice, Cayenne pepper, pimento, onions, and common salt.

* Phil. Mag. No. 258, vol. liv. 1819, p. 317.

The

The quantity of copper which we have, more than once, detected in this sauce, used for seasoning, and which, on account of its cheapness, is much resorted to by people in the lower walks of life, has exceeded the proportion of lead to be met with in other articles employed in domestic œconomy.

The following account of Mr. Lewis* on this subject will be sufficient to cause the public to be on their guard:

" Being in the habit of frequently purchasing large quantities of pickles and other culinary sauces, for the use of my establishment, and also for foreign trade, it fell lately to my lot to purchase from a manufacturer of those commodities a quantity of walnut catsup, apparently of an excellent quality; but, to my great surprise, I had reason to believe that the article might be contaminated with some deleterious substance, from circumstances which happened in my business as a tavern keeper, but which are unnecessary to be detailed here; and it was this that induced me to make inquiry concerning the compounding of the suspected articles.

" The catsup being prepared by boiling in a copper, as is usually practised, the outer green shell of walnuts, after having been suffered to turn black on exposure to air, in combination with common salt, with a portion of pimento and pepper-dust, in common vinegar, strengthened with some vinegar extract left behind as residue in the still of vinegar manufacturers; I therefore suspected that the catsup might be impregnated with some copper. To convince myself of this opinion, I boiled down to dryness a quart of it in a stone pipkin, which yielded to me a dark brown mass. I put this mass into a crucible, and kept it in a coal fire, red-hot, till it became reduced to a porous black charcoal: on urging the heat with a pair of bellows, and stirring the mass in the crucible with the stem of a tobacco-pipe, it became, after two hours' exposure to an intense heat, converted into a greyish-white ash; but no metal could be discriminated amongst it. I now poured upon it some aqua fortis, which dissolved nearly the whole of it, with an effervescence; and produced, after having been suffered to stand, to let the insoluble portion subside, a bright grass-green solution, of a strong metallic taste: after immersing in this solution the blade of a knife, it became instantly covered with a bright coat of copper.

" The walnut catsup was therefore evidently strongly impregnated with copper. On informing the manufacturer of this fact, he assured me, that the same method of preparing the liquor was generally pursued, and that he had manufactured the article in a like manner for upwards of twenty years.

* Literary Chronicle, No. 24, p. 379.

" Such

"Such is the statement I wish to communicate; and if you will allow it a place in your Literary Chronicle, it may perhaps tend to put the unwary on their guard against the practice of preparing this sauce by boiling it in a copper, which certainly may contaminate the liquor, and render it poisonous."

Poisonous Custard.—The leaves of the cherry laurel, *prunus lauro-cerasus*, a poisonous plant, have a nutty flavour, resembling that of the kernels of peach-stones, or of bitter almonds, which to most palates is grateful. These leaves have for many years been in use among cooks, to communicate an almond or kernel-like flavour to custards, puddings, creams, *blanc-mange*, and other delicacies of the table.

It has been asserted, that the laurel poison in custards and other articles of cookery, is, on account of its being used in very small quantities, quite harmless. To refute this assertion, numerous instances might be cited; and among them, a recent one, in which four children suffered most severely from partaking of custard flavoured with the leaves of this poisonous plant.

"Several children at a boarding-school, in the vicinity of Richmond, having partaken of some custard flavoured with the leaves of the cherry laurel, as is frequently practised by cooks, four of the poor innocents were taken severely ill in consequence. Two of them, a girl six years of age, and a boy of five years old, fell into a profound sleep, out of which they could not be roused.

"Notwithstanding the various medical exertions used, the boy remained in a stupor ten hours; and the girl nine hours: the other two, one of whom was six years old, a girl, and a girl of seven years, complained of severe pains in the epigastric region. They all recovered, after three days' illness. I am anxious to communicate to you this fact, that it may contribute to put the unwary on their guard, against the deleterious effects of flavouring culinary dishes with that baneful herb, the cherry laurel.

"I am, &c.

"THOMAS LIDIARD *."

What person of sense or prudence, then, would trust to the discretion of an ignorant cook, in mixing so dangerous an ingredient in his puddings and creams? Who but a maniac would choose to season his victuals with poison?

The water distilled from cherry laurel leaves is frequently mixed with brandy and other spirituous liquors, to impart to them the flavour of the cordial called *noyau*.

This fluid, though long in frequent use as a flavouring substance, was not known to be poisonous until the year 1728; when

* Literary Chronicle, No. 22, p. 348.—1819.

the

the sudden death of two women, in Dublin, after drinking some of the common distilled cherry laurel water, demonstrated its deleterious nature.

Poisonous Anchovy Sauce.—Several samples which we have examined of this fish sauce, have been found contaminated with lead.

The mode of preparation of this fish-sauce, consists in rubbing down the broken anchovy in a mortar: and this triturated mass, being of a dark brown colour, receives, without much risk of detection, a certain quantity of Venetian red, added for the purpose of colouring it, which, if genuine, is an innocent colouring substance: but instances have occurred of this pigment having been adulterated with orange lead, which is nothing else than a better kind of minium, or red oxide of lead. The fraud may be detected, as stated p. 107.

The conscientious oilmen, less anxious with respect to colour, substitute for this poison the more harmless pigment, called Armenian bole.

The following recipe for making this fish-sauce is copied from Gray's Supplement to the Pharmacopœias, p. 241.

"Anchovies, 2 lbs. to 4 lbs. and a half; pulp through a fine hair sieve; boil the bones with common salt, 7 oz. in water 6 lbs; strain; add flour 7 oz. and the pulp of the fish; boil; pass the whole through the sieve; colour with Venetian red to your fancy. It should produce I gallon."

Adulteration of Mustard.—Genuine mustard, either in powder, or in the state of a paste ready for use, is perhaps rarely to be met with in the shops. The article sold under the name of *genuine Durham mustard*, is usually a mixture of mustard and common wheaten flour, with a portion of Cayenne pepper, and a large quantity of bay salt, made with water into a paste, ready for use. Some manufacturers adulterate their mustard with radish seed and pease flour.

It has often been stated, that a fine yellow colour is given to mustard by means of turmeric. We doubt the truth of this assertion. The presence of the minutest quantity of turmeric may instantly be detected, by adding to the mustard a few drops of a solution of potash, or any other alcali, which changes the bright yellow colour, to a brown or deep orange tint.

Two ounces and a half of Cayenne pepper, 1½ lb. of bay salt, 8 lbs. of mustard flour, and 1½ lb. of wheaten flour, made into a stiff paste, with the requisite quantity of water, in which the bay salt is previously dissolved, forms the so-called *genuine Durham mustard*, sold in pots. The salt and Cayenne pepper contribute materially to the keeping of ready-made mustard.

There

There is therefore nothing deleterious in the usual practice of adulterating this commodity of the table. The fraud only tends to deteriorate the quality and flavour of the genuine article itself.

XIV. *Defence of M.* DE PRONY *from the Aspersions contained in Memoir of Mr.* WATT. *By Mr.* JOHN FAREY.

To Mr. Tilloch.

SIR, — THE memoirs of the late James Watt, Esq. which you have printed in your last volume, are highly interesting, and the writer has done no more than justice to the inventive genius of that great man in the account of the origin of the improved steam-engine.

This memoir charges M. de Prony with committing a flagrant act of injustice in his History of the improved Steam-engine, in two volumes in quarto. It is stated that M. de Prony there gives the invention of the improved steam-engine to Messrs. Perriers, never once mentioning the name of Mr. Watt.

It would indeed have been a most flagrant act of injustice if Mr. Prony had done so, and in that case I would willingly have left that gentleman to encounter all the odium which the author of the memoir throws upon him; but M. de Prony does state Mr. Watt to be the inventor, and that the improved engine was brought from England by Mr. Perrier; which is the fact. As only a few of your readers can have an opportunity of examining M. de Prony's book, and as all must concur in reprobating his conduct, whilst this allegation stands uncontroverted; it will be only justice in you to print the following extracts from the work in question.

It is entitled *Nouvelle Architecture Hydraulique, par M. de Prony, Ingénieur des Ponts et Chausses,* in two volumes quarto, printed at Paris; the first volume in 1790, and the second in 1796.

In the preface to volume I. p. xii. is a Report on the first volume of this work to the Academy Royal in 1798. The reporter says, "Le dernier chapitre contient l'histoire et l'usage des machines à feu depuis le Marquis de Worcester, jusqu'aux dernières découvertes faites par MM. Watt et Bolton, et dont la machine à double injection vient d'être apportée en France, et présentée à l'Académie par M. de Betancourt."

Mr. Prony begins his account of steam-engines at page 563, vol. I. His history of the successive English inventions by the Marquis of Worcester, Captain Savery, Newcomen, and Cawley, is very

very concise, but correct in every particular; and at page 568 he introduces the improved engine thus: "Un Anglois appellé M. Watt a imaginé vers l'année 1770 une machine qui est exempte d'une grande partie de défauts de celle de Savery, et a entre autres avantages celui d'opérer la condensation hors du cylindre." Then follows the reference to a figure of Mr. Watt's original single engine for pumping.

At page 571 he says, "La machine qu'on vient de décrire a été apportée d'Angleterre en France par MM. Perrier, qui l'ont fait exécuter à Chaillot depuis quelques années; et c'étoit ce que nous connoissions de mieux jusqu'en 1788."

Next he speaks of Mr. Watt's double rotative engine, first in the marginal note: "Comment M. le Chevalier de Betancourt a deviné le principe d'une machine à feu postéri eurement construite par MM. Watt et Bolton." Then in the text, "En 1788, M. le Chevalier Betancourt, qui est chargé par le Cour d'Espagne de faire une collection de recherches et de modèles pour la perfection de l'hydraulique, étant allé à Londres, eut occasion de visiter les machines à feu de MM. Watt et Bolton. Il vit le jeu extérieur des ces machines; mais on lui en cacha le mécanisme intérieur, ainsi qu'on avoit fait à ceux qui avoient eu la même curiosité avant lui."

After mentioning some of M. de Betancourt's observations, he continues:

"Il fit plusieurs autres observations dont nous ne pouvons pas nous occuper à present, mais dont les artistes doivent lui savoir d'autant plus de gré que ces observations sont difficiles à faire, lorsqu'on n'a que peu d'instants pour examiner une machine masquée par les distributions d'un bâtiment qui en isolent les différentes parties, même extérieurs, et empêchent qu'on ne puisse en saisir la corréspondance l'ensemble et l'effet général. M. le Chevalier de Betancourt conclut néanmoins de ces observations, que le piston du cylindre devoit être poussé avec le même effort, soit dans sa déscente, soit dans sa montée; et ce résultat lui fit découvrir le double effet qui constitue essentiellement la nouvelle perfection ajoutée aux machines à feu par MM. Watt et Bolton.

"M. le Chevalier de Betancourt, de retour à Paris, fit exécuter un modèle de machine à feu à double effet, et les expériences faites avec ce modèle ont tout le succès qu'on peut désirer.

"Le mécanisme intérieur, au moyen du quel la double injection s'opère, est entièrement de l'invention de M. le Chevalier de Betancourt; et quoiqu'il ignore si ses procédés sont les mêmes que ceux de MM. Watt et Bolton, vu le secret que lui en ont fait ces derniers, il a tout lieu de croire que les artistes Anglois n'ont pas atteint un plus grand degré de précision et de simplicité.

cité. C'est dans cette confiance que MM. Perrier, excellents juges en pareille matière, se sont déterminés à faire construire une machine à feu à double effet, et conforme au modèle de M. le Chevalier de Betancourt. Cette machine destinée à faire mouvoir des moulins doit être en activité au commencement de l'année 1790."

At page 574 he says:

"Le volant (fly wheel) a été imaginé par un Anglois fabriquant de bierre, qui en a le premier fait usage en 1780."

Although this is not correct, there is no reason to conclude that Mr. Prony intended to detract from the merit of Mr. Watt.

The second volume, which was not printed till 1796, is expressly devoted to the steam-engine. In introducing the double engine at page 35, he refers to his former statement in the first volume of Mr. Betancourt's visit to England, and his model, and then he continues:

"M. Perrier l'aîné nous a assuré que quoiqu'il n'ait construit des machines à double effet qu'après avoir connu le modèle de M. de Betancourt, il avoit eu cependant depuis très long temps l'idée de pareilles machines; que son objet étoit de diminuer la grosseur du cylindre à vapeur, de supprimer les contrepoids, de simplifier tout l'attirail, enfin d'économiser le combustible. On ne sauroit revoquer en doute l'assertion d'un artiste aussi habile que digne de foi, il est d'ailleurs très naturel de penser que ceux qui ont beaucoup réfléchi sur les divers moyens d'employer la vapeur de l'eau comme moteur, aient cherché à transmettre son effort d'une manière telle que l'attirail intermédiaire le diminuoit le moins possible; or les machines de Chaillot."

After describing the parallel motion, Mr. Prony introduces a description of an instrument called the Geometrical Pen, by Suarts, which he calls "un instrument très ingénieux, avec lequel on peut tracer une infinité de courbes par une combinaison de mouvements circulaires, et qui a suggeré à M. Watt l'idée de produire avec de semblables mouvements une ascension rectiligne et verticale. Quand cette machine ne seroit pas par elle-même d'une curiosité piquante—elle devroit sur tout intéresser par le rapport qu'elle a avec l'histoire de l'art des machines à feu George Adams, dans un ouvrage intitulé Geometrical and Graphical Essays, etc. London 1791.

"Adams dit positivement que ce principe a été appliqué par MM. Watt et Bolton au mécanisme des machines à feu." It has lately been happily introduced into the steam-engine by MM. Watt and Bolton. "M. Watt a depuis confirmé verbalement la vérité de cette assertion à quelqu'un de notre connaissance."

37, Howland-street, Jan. 5, 1820.

XV. *On*

XV. *On the Geology of Loch Leven, in continuation of Article in Phil. Mag. of September* 1819. *By Mr.* Gavin Inglis.

To Mr. Tilloch.

Sir, — When in London in May last, I mentioned the grounds I had for believing in the existence of a stratified galvanic connexion and internal communication extending under the districts of Loch Leven in Kinross-shire, Comrie in Perthshire, Inverness-shire, and all those parts of the country that have of late years been so frequently visited with the fearful phænomena of earthquakes. Of this I can no longer entertain a doubt. These convulsive throes of nature are undoubtedly the effects of a stratified galvanic influence, violent in proportion to the resistance opposed to the escape of the elastic matter produced in the decomposition of old, and in the formation of new, combinations resulting from the incessant workings of these stratified galvanic piles, of Nature's construction.

To occupy your valuable pages with speculative theories, would be very foreign to my present purpose; I shall confine myself alone to facts as they occurred, and to observations deduced from an attentive inquiry into the present and former geological state of the lake, undertaken with a view to furnish you with one connected account.

This, however, from want of time, I have never been able to accomplish, and must therefore content myself in the present instance with some extracts from my notes, that may be of some general use to the geologist—to the geology of these districts in particular.

You are already acquainted with the remarks and observations of George Braid, the old residenter on the banks of the Leven, respecting the muddy waters discharged from the lake in August 1816, and his declaring "that he had never seen the waters in such a state before but once, and that was when an earthquake happened at Comrie; and," continued he, "an earthquake has happened somewhere, be where it may." Curiosity, as I stated in that communication, induced me to trace the troubled waters to the lake, and to calculate the time necessary for the waters issuing from their outlet to reach the Bleach-field, and found it to agree as near with the time of the shock at Inverness, as you could well suppose the clocks and watches of the two districts to do. But to give a connected detail of the circumstances that led me to form this conclusion, I must retrograde, and go back to summer 1815, and state, that although there is no record of any shock of an earthquake at the period I allude to, so far as I remember; neither have I heard of any *tremblement de terre* in these

these districts above referred to; yet there was a circumstance connected with the concatenated events that have since been experienced, that I think will go far to prove the previous workings of these hidden piles, and which, though partial in effect, was marked with characteristic evidence of its being a member of the same family.

In summer 1815, there was a discharge (of gas in all probability) from the depths of the lake, in such quantity, and so very deleterious in its effect, that its impregnation of the waters destroyed the fish in such numbers as gave rise to a belief that the waters of the lake had been poisoned. Incredible numbers of its finny inhabitants were destroyed and thrown ashore, chiefly the largest of their kinds; such as frequent the deepest waters. Many a monarch of the flood that had reigned perhaps for ages, the terror of the smaller fry, now lay along the beach a prey to the raven and carrion crow; and so strongly and implicitly was the belief of their having died by poison riveted in the minds of the surrounding population, that no one, however needy, would venture to eat them, notwithstanding the finest fish of their species, pike, trout, perch, and eels, might have been collected in cart loads. Pike, upwards of twenty pounds avordupoise, and trouts, &c. of the largest size, lay in promiscuous confusion along the margin of the lake, all of them (except the eel) presenting one uniform characteristic mark of destruction, the eyes of every species of the scaly tribe having been driven or started with apparent force from their sockets. This undeviating uniformity in every species except the eel, shows that they had all died from one cause, and by a similar effect on the vital organs. The exception of the eel will not at all surprise those acquainted with the anatomical construction of this animal, whose beautiful radiant eye is, as it were, hermetically inclosed within its recess, and protected from all external injury by the same skin that enfolds the body of the fish passing over the eye. The particular part that covers the visual organ is somewhat thinner and perfectly transparent; so that, however the vitals of the eel may have been affected, the eye, securely bound within its recess, would not leave its natural position, without bursting this skin, however similar its death might have been in other respects.

At this time the waters of the lake assumed a dark colour resembl'ng an infusion of peat soil, and partially retained this dusky hue till August 1816, when the quantity of aluminous soil, thrown from the botton of the lake by the shock which so much alarmed the town and neighbourhood of Inverness, had the effect of clarifying the waters, and restoring them to their accustomed purity. With the particulars of this your readers are already acquainted.

What I have just narrated, forms the ground on which I founded my my opinion of the galvanic connexion of these districts, and made me resolve to watch the various states and appearance of the water, and, from the shrewd accuracy of George Braid's former remarks, to consult him on all occasions. What I have now to state, has confirmed me in the full belief of the existence of this extended stratification of galvanizing minerals.

Midsummer 1817, a water-spout fell near the town of Dunfermline, which, with the consequent rains that fell in the surrounding country, did considerable damage. This might or might not be connected with the shock of the earthquake felt about the same time in the north. No rain of any consequence fell here, nor near Loch Leven: notwithstanding, that evening and all the day following a burst of muddy water was driven from the Loch, bearing all the characteristic marks of August 1816. We noted the circumstance, and the result confirmed our conjectures.

Again, in November 1818, the water presented a similar appearance with the two preceding years. We expected accounts of an earthquake, and were confirmed by the arrival of the post from the north.

In October last, when I mentioned that the waters from the lake indicated an internal commotion, but darker in the colour than usual on former occasions, although no *shock* was felt that has been heard of, still I am perfectly satisfied it was the partial working of the subsequent shock at Comrie. I did not consider the circumstance of the eels escaping from the lake as any proof of an internal agitation, as that was the natural season of their emigration from the fresh waters to the sea. But the circumstance of their all at one time starting, or being driven *en masse* from the lake, is an event in their history that the oldest eel-fisher on the Leven does not remember. In three successive nights (for they only run in the dark; as soon as *light* of any kind appears they burrow in the mud, or under stones, or the banks of the river) upwards of twelve thousand eels were taken, many of them from eight to ten pounds apiece, but generally weighing from half a pound to four and five pouuds each. None were got after this, except a few straggling dozens that may have burrowed deeper in the mud than the rest, or lodged in some of the deep pools in the serpentine turnings of the river betwixt the lake and the fishing-ground. This must have been connected with the earthquake that followed in November last. On the afternoon of the 29th of that month I was standing at the top of the drying-loft stair, looking into the river, which was quite clean and clear at the time, when all of a sudden it came down filled with mud. I turned round to see if there was any discharge from Ennerlly Burn, when I observed it coming over the dam-dyke or weir-head in the same state. I immediately went down to examine it more closely, sent for my monitor, and we concluded "that an earthquake had happened *somewhere*, be where it might." The first post from Comrie confirmed the fact; and I am now without the smallest doubt of what I have stated, of the whole of that range of country superstrating a natural arrangement of galvanizing materials, which are incessantly at work, and will continue to keep the good people of that part of the world in remembrance of the instability of all sublunary things, even the earth itself.

By an attentive examination of the metals, and a skilful geological survey of the strata and primitive formations in these affected districts, and in particular a studious application to the current of galvanic, electric, or magnetic tendency, which I consider mere terms or modifications of the same principle, only variously attired, in these peculiar combinations, the poles of this galvanic range or ranges might be discovered, and the dread of impending danger lessened, if not altogether averted, by boring, sinking shafts, or other openings in the stratified drifts, at such places as were most likely to facilitate the escape of the gases produced in this great laboratory of nature. I am drawn to this conclusion by analogy, from an existing fact.

The shock which so much alarmed the country in August 1816 was felt at Leslie, Kirkaldy, and several intervening places some miles to the east and south-east of Loch Leven; yet no shock was felt at Kinross, nor any village or hamlet in the immediate circle round the lake; and notwithstanding the troubling of its waters, no subsequent shock has ever been felt for miles round its bed. The depth of the lake gives vent to, and allows the elastic fluids to escape through the stones, gravel, sand, mud, and water, without affecting the surrounding solid matter. The stratified formation at the bottom of the lake must be the negative termination of one of these galvanic ranges; and perhaps to its regular discharge of hydrogen into the waters of the lake may be attributed their superior purity and softness, when contrasted with the numerous and copious springs of peculiarly hard silicated water that flows from the bottom and sides of the surrounding mountains, and fall into the lake round its margin.

To hydrogen uniting with kindred affinity to the base of this hard silicated water, may be attributed the copious deposit of siliceous dust that covers so large a portion of the bottom of the lake, and constitutes no inconsiderable proportion of the mud brought down in these eruptive discharges.

On viewing the geological map of British strata exhibited in Cary's

Cary's window, London seems to occupy the only spot in Britain, perhaps in the world, that may be deemed secure against the partial workings of these principles; and, although not founded on a rock, is destined, I hope, to flourish till time shall be no more—

> "Until that great and awful day
> That shall the world in ashes lay,
> As David and the Sybils say."

The terrific effect, however, of these momentous operations of the mineral kingdom cannot be contemplated even at a distance without a sensation of the most chilling description, mixed with the awful incertitude of human existence, where immediately exposed to the instantaneous rendings of contending elements; and the revolutions of nature must bear with no trifling degree of dread and alarm on the human mind, and make it shrink with conscious nullity from the workings of superior power. Nature recoils and shudders at destruction.

Much time and labour and efforts of human genius have been more unworthily spent than by the geological philosopher turning the bent of his attention to the wonderful agency of galvanizing nature in all its majesty.—Shakespear says, "All the world's *a stage*."—I say the world's a galvanic pile, of grand dimensions, having the south pole for its positive, and the north pole for its negative: hence the current of magnetic impetus towards the north; thence the electric grandeur of the *aurora borealis:* and from this may be traced the origin of the island of Iceland, which still continues to be the point of deflagration in the grand trough of primitive formation, and whose volcanic wonders are only the super-workings of the major pile.

The various volcanos now extinct or silent, as well as those still in turmoil, scattered over the surface of the globe, are mere frieze-workings of minor arrangements of the more recent formations, brought into action by what constitutes the base of the Neptunian system, which had its origin as in the days of Peleg, when the earth was *torn* asunder; when the continents of the east and the west were thrown from their bases like the explosion of a bomb. The *terra australis*, or positive, falling by its own impetus as well as gravity towards the centre, prevented the recoil of the negative and the collapsation of the longitudinal hemispheres; and into this abyss "the rolling seas together flow, and leave the solid land;" exposing to elementary action the Neptunian deposits of ages, and leaving them to the free agency of their own galvanic powers. Hence all the volcanic modifications on which rests the basis of the Huttonian system, and from whence in support of twofold truth are collected the multiplied and apparently incontrovertible indications and proofs of both theories, which, like all other theories of nature, must, when impartially examined, centre in the great creative, omnipotent Power—the formator, protector, and governor of all. Yours truly,

Jan. 31, 1820. GAVIN INGLIS.

XVI. *Memoir upon the Conversion of ligneous Bodies into Gum, Sugar, and a particular Acid, by means of Sulphuric Acid; and on the Conversion of the same ligneous Substance into Ulmine by Potash. By M.* HENRI BRACONNOT.

[Concluded from p. 56.]

Examination of the artificial Gum produced by the Action of Sulphuric Acid on Linen.

WE have seen that this gum, obtained by saturating sulphuric acid with chalk, contains in combination, lime which may in fact be separated by oxalic acid. This gum may be also purified by adding to a solution of it some of the sub-acetate of lead: this produces a very abundant white precipitate, which being decomposed by an excess of sulphuric acid, the liquid is evaporated to a sufficient degree, the sulphate of lead is separated, and the gum precipitated by alcohol. I prefer however in place of chalk to saturate the sulphuric acid with the oxide of lead heated: there results from it a liquid of a sugary, smart taste, owing to the lead held in solution: the latter is separated by passing through a current of hydrosulphuric gas; and the filtered liquor being evaporated, the artificial gum is obtained as pure as possible. Barytes may also be used in place of the oxide of lead; but as this gum retains barytes in combination, it is necessary to separate it by sulphuric acid.

This gummy matter, such as I have obtained, resembles gum arabic. It is transparent, of a slight yellow colour, inodorous, insipid, though it reddened the tincture of turnsole, and seemed to act in the manner of acids. Its fracture is vitreous. It adhered strongly to the vessels in which it was dried, always when it was prepared with care, and formed on them a very shining kind of varnish. It forms a mucilage less tenacious than that of gum arabic: its cohesive properties are also inferior: but this does not prevent its being of use in many of the arts. Exposed to the fire, it consumed, giving out a penetrating odour of sulphurous acid, owing to the decomposition of the sulphuric acid which it contains in a particular state of combination, such as the usual reagents

reagents cannot indicate. The residum shows after incineration some traces of sulphate of lime.

When this gum is exposed with potash to the fire to decompose it partially, no sulphurous acid is disengaged. If the residuum is dissolved in water, and nitric acid added, a brown flocculent matter is precipitated, which is known more particularly by the name of artificial *ulmine*. If nitrate of barytes is added to the filtered liquor, it produces a precipitate of sulphate of barytes.

The solution of this gum in water is not disturbed either by nitrate of barytes or by acetate of lead; but the sub-acetate of lead produces a very white and abundant magma entirely soluble in weak acetic acid. If the lead which the liquor above this precipitate contains is precipitated by carbonate of ammonia, and then evaporated to dryness, the residuum is a triple combination of gum, acetic acid, and ammonia. The hydrochlorate of the protoxide of tin also precipitates this gum from its dissolvent. Lime water or barytes in excess produces slight flocculent precipitates, which are nothing but combinations of gum with lime or barytes. The red sulphate of iron does not affect in the least the solution of this gum, while it coagulates abundantly that of gum arabic. Treated with sulphuric acid, this gum furnishes a large quantity of oxalic acid in fine crystals, but no mucous acid.

Sugar from Liuen.

The gummy matter the properties of which we have mentioned, being boiled for some time with diluted sulphuric acid, experiences such a change of equilibrium in the nature of its principles, that in separating they produce two very remarkable substances: the one which forms almost the whole of the matter is crystallizable sugar; the other contains the elements of the sulphuric acid which were disseminated in the gummy matter, and constitutes a singular enough acid, which I will designate under the name of the *vegeto-sulphuric acid.* I was led accidentally to this result, by treating a solution of the acid mucilaginous mass, produced by the action of the sulphuric acid on linen, with the oxide of lead, subjected to a long continued heat of 100° centigrade; but after having passed through the liquor a current of sulphurated hydrogen gas to precipitate the lead contained in solution, and after evaporating it, I was agreeably surprised to see that the whole of the gummy matter was entirely converted into an acid sugary mass. I digested this mass with very concentrated alcohol, by which the vegeto-sulphuric acid was dissolved: the sugary matter remained a little coloured, and of a very fresh flavour. Twenty-four grammes (370·6 gr.) of old cloth well

dried,

dried, were reduced into mucilage by 34 grammes (525 gr.) of sulphuric acid, observing the precautions before indicated; the acid mixture, dissolved in a certain quantity of water, precipitated the ligneous matter a little altered; when dried it weighed 3·6 grammes (55·5 gr.) The acid liquor thus diluted with water, was boiled about ten hours; after which it was saturated with carbonate of lime. This liquor did not precipitate the subacetate of lead; it no longer contained any gum; the residue was evaporated and dried as much as possible until it began to exhale an odour of calomel. In this state it weighed 23·3 gram. (359·8 gr.) which were furnished by 20·4 grammes (315 gr.) of cloth, deduction being made of what was not reduced: but I believe that there were some losses; for the sulphate of lime, although well washed, had a slight coloured teint, different from what results from the preparation of artificial gum: however, like the latter, in place of burning in the fire and disengaging sulphuric acid, it became whiter, and did not exhale any very particular odour. I made these 23·3 grammes (359·8 gr.) of sugary matter into the consistence of syrup*; at the end of 24 hours it began to crystallize; and some days after, the whole was solidified into a single mass of crystallized sugar, which was pressed strongly between several folds of old cloth; crystallized a second time this sugar was passably pure; but treated with animal charcoal it became of a shining whiteness. The crystals were in spherical groupes, which appear to be formed by the union of small diverging and unequal plates. They are fusible at the temperature of boiling water. This sugar, of a fresh and agreeable flavour, produced in the mouth a slight sensation of coolness. It dissolves in hot alcohol, and crystallizes by cooling. Dissolved in water and mixed with a little yeast, it fermented; the vinous liquor which resulted furnished alcohol by distillation. Burnt with potash, and its charcoal washed with diluted nitric acid, it yielded a fluid not troubled by nitrate of barytes. It would be useless to insist further on the properties of this sugar; it is evident that it is perfectly identical with the sugar of grapes, or of starch.

The conversion of wood into sugar will no doubt appear remarkable; and when persons not familiarised with chemical speculations

* This syrup was weakly affected by sulphuric acid, while a concentrated solution of gum obtained by saturation with chalk was sensibly precipitated by the same acid which separated it from the sulphate of lime; from which there seems reason to presume that in converting this gum into sugar, by boiling it a long time with weak sulphuric acid, the elements of that acid which it contains do not entirely reunite to form the vegeto-sulphuric acid; but that a part separates from it in the state of free sulphuric acid, and the rest continues mingled with that which serves to effect the saccharification.

culations are told that a pound weight of rags can be converted into more than a pound of sugar, they may regard the statement as a piece of pleasantry, though nothing can be more real.

It appears to me, that from the conversion of wood into gum and sugar, some important consequences may be deduced, which may throw light on many obscure points of vegetation. In fact, since the observation seems to indicate that woods consist of gum or mucilage with less of oxygen and hydrogen than the proportions necessary to make water, we can, by remounting to the origin of the formation of ligneous matter, appreciate the means which nature puts in operation to create it. If we examine it a little before its birth, we see that it presents the form of a mucilage containing some small white grains which appear to be a first germ of wood: this mucosity, on account of the important part which it acts in vegetation, has received the name of the *organizing substance* or *Cambium de Duhamel*. Aided by the vital influence, this substance appears to abandon by little and little a part of the elements of water, to form first the cortical beds, the sap, the parenchyma; and at last the wood, properly so called, which must be extremely variable in the proportion of its principles, according as it is of new or old formation. This manner of viewing the transformation of cambium into wood, appears to be probable enough, when it is considered that we can retrograde the latter to its primitive state of mucilage. It is scarcely necessary to remind the reader, that wood often concretes in great abundance in the heart of mucous and sugary matter, as we see in nuts, in ligneous concretions of pears, &c. It may be further observed, that the death of the vegetable does not put an end to this subtraction of oxygen and hydrogen; it continues to take place in all the different states through which the ligneous matter passes, till it is at length entirely destroyed.

Of the Vegeto-Sulphuric Acid.

We have said that after having saturated a solution of the acid mucilage formed by the action of sulphuric acid upon linen with the oxide of lead, exposed to a long continued heat, there was formed sugar, and an acid of a particular nature, which we separated by rectified alcohol in which it was dissolved. This alcoholic liquor, however, retains also some sugar. I evaporated it to the consistence of a syrup, and agitated it with ether, which took a slight straw colour, and left after its evaporation an almost colourless acid, very sharp, almost caustic, and setting the teeth on edge. This acid is deliquescent, uncrystallizable, and attracts the humidity from the air. It gradually became brown in the air when above mean temperature. Put into a capsule plunged

plunged in a sand-bath, it decomposes and becomes black a little before the water begins to boil. When treated in this state with a little water, flocculi of vegetable matter partly carbonized are separated; and when some nitrate of barytes is poured into the liquor, an abundance of sulphate of barytes is precipitated. When this acid is subjected to a temperature higher than that of boiling water, its decomposition is more rapid, and suffocating vapours of sulphurous acid are emitted. This acid does not produce any change on metallic solutions. Nitrate of barytes and sub-acetate of lead are not in the least affected by it. It makes a strong effervescence with carbonates, and appears to dissolve all the metallic oxides, with which it forms salts, uncrystallizable, deliquescent, insoluble in acetified alcohol. These saline and neutral combinations, subjected to fire, decompose, emit sulphurous acid, and leave sulphates and charcoal. The same acid dissolves iron and zinc, liberating abundance of oxygen gas. It forms with oxide of lead and barytes very soluble salts, which have a gummy appearance. It seems to have a strong enough dissolving faculty; for it even dissolves to a certain degree sulphate of lead.

This acid is composed of sulphur, carbon, hydrogen and oxygen; or of a vegetable matter, and the elements of sulphuric acid, but in proportions with which I am unacquainted.

Action of Sulphuric Acid on Silk.

In treating silk with sulphuric acid, I conceived the hope of returning it to its primitive state of silky liquor, such as is extracted from the bodies of certain caterpillars, and with which, according to Reaumur, the Mexicans prepare their admirable varnish. I hoped by means of a similar liquor artificially prepared with pieces of silk, that it would be easy to fabricate cloth not spun. I have not however obtained this result, though I have not abandoned the hope of attaining it. Be this as it may, the sulphuric acid can convert the silk into two mucilaginous substances very distinct.

If some pieces of white silk are slightly moistened with this acid, and, after a few minutes have been given to allow it to act, the mixture is treated with a quantity of water, a very thick white mucilage is obtained resembling gum tragacanth. When a greater quantity of water is added, the whole of the mucilage is precipitated, and the surnatant liquid, which is as colourless as pure water, retains only a small quantity of silk in solution. This mucilage well washed with water is insipid to the taste. It is not sensibly affected by cold water; but a great quantity of boiling water dissolves it: the liquor being evaporated, there remain some insoluble membranes: an infusion of nut-galls produces a precipitate.

precipitate. This mucilaginous matter differs from that which is found in the caterpillar, inasmuch as it does not dry so rapidly, and is resoftened by water.

If a greater quantity of sulphuric acid is made to act a longer time on the silk, different results from those which I have mentioned are obtained.

Five grammes (77·2 gr.) of white silk cloth torn into small pieces were triturated in a glass mortar, by adding succesively sulphuric acid until the whole was reduced to a homogeneous mucilage; there was a production of heat, but no sulphurous acid was disengaged. Twenty-four hours afterwards the mixture was treated with water, which produced an entire solution without precipitating the least particle of carbon: all that was separated was a yellow flocculent matter, which when dried weighed 0·15 gramme. The acid liquor was saturated with carbonate of lime, and part of it reduced by evaporation, in order to favour the precipitation of the sulphate of lime which it retained. The process was finished by evaporating it to perfect dryness. There remained 4·2 grammes of a residuum reddish and transparent resembling isinglass. This residuum dissolved in a small quantity of water did not congeal by cooling. With potash it did not disengage ammonia. Distilled it yielded carbonate of ammonia, and left after its incineration sulphate of lime. The infusion of nut-galls and the sub-acetate of lead precipitated it abundantly from its solution, but it was only slightly precipitated by the acetate of lead.

When therefore sulphuric acid falls upon a vestment of silk, or linen, or of cotton, it does not burn them, as it is said; but it perforates them, and the portion affected is converted into gummy matter soluble in water.

Action of Sulphuric Acid upon Gum and Sugar.

Gum arabic pulverized has been treated with concentrated sulphuric acid in quantity sufficient to dissolve it; but, far from producing carbon, as Fourcroy pretends, the mixture was scarcely coloured. At the end, however, of twenty-four hours it had contracted a brownish colour: diluted with water, it did not precipitate the least particle of carbon. The liquor saturated with chalk produced a gum, which had exactly the same properties as that which we formed by the action of sulphuric acid upon wood. Exposed to the fire it burned, exhaling an odour of sulphurous acid. The acetate of lead did not trouble its solution, but the sub-acetate of lead produced a very abundant white coagulum.

Cane-sugar acted a little differently with sulphuric acid: it was coloured almost immediately, and became of a blackish maroon, which deepened in course of time; but it did not form sulphurous acid, and the whole matter was entirely dissolved in

water

water without depositing any particle of carbon. The liquor saturated with chalk furnished by evaporation a deep brown residuum of a sharp sugary savour. When heated, it exhaled vapours of sulphurous acid.

Conversion of ligneous Bodies into Ulmine by the Action of Potash.

We have seen that wood appropriates to itself the elements of sulphuric acid and water, in order to pass into the state of gum, and that the latter by a new distribution of its principles can be transformed almost entirely into sugar and a small quantity of a particular acid. We now proceed to show that, by extracting from wood the proportions of oxygen and hydrogen necessary for making water, it may be converted into a substance in which the carbon predominates, and which appears to me to have a great analogy to ulmine. M. Vauquelin is, I believe, the first who has made known natural ulmine as a particular substance *. He found it in combination with potash in the purulent ulcers of old elms, the mass of the wood of which had been in part eaten up and destroyed by suppuration. It was not until seven years after that Klaproth made mention of it: it appears that he had lost sight of the experiments of the French chemist; which may be believed, since he attributes to it properties which do not belong to it. Berzelius, Smithson, and Thomson afterwards directed their attention to it. The first of these chemists thought that it formed a constituent part of the bark of almost all trees; but it appears to present many varieties. I have found it in large enough quantities in the bark of the beech tree, partly combined with potash associated with a gum, a particular red matter, a small quantity of tannin, and a principle the odour of which is exactly similar to that of vanilla.

It is by studying the action of potash upon wood, that I succeeded in producing ulmine artificially. I began by ascertaining for certain that, contrary to the opinion of Mr. Thomson, pure ligneous matter is insensibly soluble in potash; but that it is otherwise when, along with that alkali of commerce made caustic, an equal quantity of sawdust and a little water are heated in a silver or iron crucible, in order to torrefy it; for, by taking care continually to agitate the mixture, the sawdust at length softens and dissolves almost instantaneously, swelling considerably.

If the crucible be withdrawn from the fire, and a little water poured into it, the whole matter dissolves with extreme facility, except a slight residuum formed of silex, carbonate of lime, phosphate of lime, and some traces of vegetable matter; and a deep

* *Ann. de Chimie*, t. xxi. p. 44.

brown

brown liquor is obtained which retains in solution the potash combined with ulmine: an acid separates the latter in the form of a very abundant brown precipitate, which only requires to be well washed. If the acid liquor, separated from this precipitate, be saturated with chalk, and evaporated to dryness, alcohol, when digested on it, will separate acetate of potash. Sawdust thus treated with potash can furnish more than a fourth of its weight of dried artificial ulmine. Old linen cloth gives the same results: it disengages nothing but water, and a small quantity of yellow empyreumatic oil.

Dried artificial ulmine is of a black brilliant as jet: it is very brittle, and divides easily into angular fragments. Its fracture is vitreous. It is nearly insipid, and inodorous. In this state of dryness it is insoluble in water; but when it has been precipitated, it dissolves in small quantities, and communicates a yellowish brown colour. The fluid does not contain more than $\frac{1}{2500}$ of ulmine in solution; it bubbles on agitation like a solution of natural ulmine. The same artificial substance gives to boiling water a deep brown colour like prepared coffee. If nitrate of mercury, or nitrate of lead, is poured upon it, it produces immediately brown precipitates, and the liquor is entirely dissolved. It is also precipitated by the nitrate of silver, sulphate of red iron, nitrate of barytes, acetate of alumine, chlorate of calcium, and chlorate of sodium; but the precipitates do not manifest themselves till some time after the mixture. Lime water does not produce any change on it; but if a little lime, in powder, is added, the liquor is greatly discoloured; and with litharge it is entirely so.

I ascertained that the ulmine of the bark of the beech-tree presents similar results to those I have mentioned. I dissolved it in the water of very pure gallic acid with gelatine; no change was produced; but by the solution of artificial ulmine there was deposited a weighty brown elastic matter soluble in an excess of gelatine. Artificial ulmine not dried, and heated, reddens paper teinted blue with turnsole.

The same substance combines with great facility with potash, and neutralizes entirely its properties. This combination is very soluble in water: it is precipitated abundantly by acids, earthy and metallic salts, and lime water; evaporated, it leaves a blackish shining residuum, unalterable in the air, and which, after combustion, leaves potash. This combination may perhaps be useful as a colour. It combines promptly with diluted ammonia: after being evaporated to dryness, there remains a residuum of the nature of varnish, very soluble in water, and which slightly reddens paper teinted with turnsole. Lime disengages the ammonia from it; and the acids produce abundant gelatinous precipitates.

cipitates. This compound imparts a yellow colour to to linen, silk, and cotton.

The same artificial substance is soluble in concentrated sulphuric acid, like ligneous bodies; but it is abundantly precipitated by water.

It is very easily dissolved in alcohol, and yields a deep brown liquor which is precipitated by water. If this solution is allowed to evaporate spontaneously, it forms at its surface pellicles which have a crystalline contexture; but if the evaporation is forced more rapidly, a black shining residuum is obtained which resembles resin. Exposed to the flame of a lamp, it swells, and burns with a slight flame. Twenty grammes (318·10 gr.) of artificial ulmine from old linen were distilled in a glass retort; a liquid product was obtained from it of the weight of seven grammes: consisting of four grammes of a colourless fluid, and three grammes of oil, empyreumatic, brown, fluid, and soluble in any proportion in alcohol in an alkaline ley.

The colourless liquid contains nothing but acetic acid, and some traces of oily matter. There remains a charcoal of the appearance of bronze of the weight of 9·8 grammes, which left after its combustion 0·75 gramme of gray ashes, composed chiefly of carbonate of lime, phosphate and sulphate of lime, silex, and oxide of iron.

The artificial ulmine was treated with six times its weight of nitric acid at 38° Baumé; it was reduced almost to the consistency of honey; and being mixed with a little water, it assumed a deep brown colour, and left an abundance of matter, which well washed and dried was of the colour of Spanish tobacco. This matter, heated in a glass tube, was consumed without emitting light and without fusing; it produced an empyreumatic vapour, which seemed to be slightly nitrous. Its savour is bitter without being acid to the taste, although it reddens the tincture of turnsole. It is partly dissolved in boiling water, and produces a deep brown liquor which does not disturb a solution of isinglass. The brown acid liquid separated by water from this pulverulent matter, still retains something of the latter; it precipitates the animal glue, and furnishes by evaporation crystals of oxalic acid.

Such are the properties which I have recognised in this substance, produced by the action of potash upon wood, and which I have compared to the ulmine which exudes from the ulcers of trees. I may observe that the latter is also produced in analogous circumstances in diseased trees, the wood of which in putrefying separates quite from the potash which must concur in the formation of the ulmine. I ought not to neglect to state that M. Vauquelin had already compared this production of of alkali, by vegetable suppuration, to that which is effected by combustion.

Ulmine exists in many ancient products of the vegetable kingdom. I found it long ago in making the analysis of an earth found in the roots of an old tree*; but I did not then examine all its properties. It even appears that the soluble part of certain earths, which have been ranked with the extractive, is formed of ulmine and ammonia. I have also found ulmine in great abundance in turf as well as in a variety of ligneous earths. It makes, doubtless, too, a constituent part of soot; but I have found it impossible to produce ulmine with oil.

XVII. *An Account of a Membrane in the Eye, now first described.* *By* ARTHUR JACOB, *M.D. Member of the Royal College of Surgeons in Ireland, Demonstrator of Anatomy and Lecturer on Diseases of the Eye in the University of Dublin. Communicated by* JAMES MACARTNEY, *M.D. F.R.S.*†

ANATOMISTS describe the retina as consisting of two portions, the medullary expansion of the nerve, and a membranous or vascular layer. The former externally, next to the choroid coat, and the latter internally, next to the vitreous humour ‡. All however, except Albinus and some of his disciples, agree, that the nervous layer cannot be separated so as to present the appearance of a distinct membrane, though it may be scraped off, leaving the vascular layer perfect. That the medullary expansion of the optic nerve is supported by a vascular layer, does not I think admit of doubt; but it does not appear that Albinus was right in supposing that the nervous layer can be separated in form of a distinct membrane, though shreds of a considerable size may be detached, especially if hardened by acid or spirit.

Exclusive of these two layers, I find that the retina is covered on its external surface by a delicate transparent membrane, united to it by cellular substance and vessels. This structure, not hitherto noticed by anatomists, I first observed in the spring of the last year, and have since so frequently demonstrated, as to leave no doubt on my mind of its existence as a distinct and perfect membrane, apparently of the same nature as that which lines serous cavities. I cannot describe it better, than by detailing the

* *Annales de Chimie*, tom. lxi. p. 191.

† From Philosophical Transactions for 1819, Part II.

‡ Ruysch. Epist. Anat. Prob. xiii. Albinus, Annot. Acad. lib. iii. cap. xiv. Haller, Elem. Phys. t. v. lib. xvi. sect. 2. Zinn. Descrip. Anat. Oculi, cap. iii. sect. iii. Sabatier, Boyer, Charles Bell, Cuvier, &c.

method

method to be adopted for examining and displaying it. Having procured a human eye, within forty-eight hours after death, a thread should be passed through the layers of the cornea, by which the eye may be secured under water, by attaching it to a piece of wax, previously fastened to the bottom of the vessel, the posterior half of the sclerotic having been first removed. With a pair of dissecting forceps in each hand, the choroid coat should be gently torn open and turned down. If the exposed surface be now carefully examined, an experienced eye may perceive, that this is not the appearance usually presented by the retina; instead of the blue-white reticulated surface of that membrane, a uniform villous structure, more or less tinged by the black pigment, presents itself. If the extremity of the ivory handle of a dissecting knife be pushed against this surface, a breach is made in it, and a membrane of great delicacy may be separated and turned down in folds over the choroid coat, presenting the most beautiful specimen of a delicate tissue which the human body affords. If a small opening be made in the membrane, and the blunt end of a probe introduced beneath, it may be separated throughout, without being turned down, remaining loose over the retina; in which state if a small particle of paper or globule of air be introduced under it, it is raised so as to be seen against the light, and is thus displayed to great advantage; or it is sometimes so strong as to support small globules of quicksilver dropped between it and the retina, which renders its membranous nature still more evident. If a few drops of acid be added to the water after the membrane has been separated, it becomes opaque and much firmer, and may thus be preserved for several days, even without being immersed in spirit.

That it is not the nervous layer which I detach, is proved by the most superficial examination; first, because it is impossible to separate that part of the retina, so as to present the appearance I mention *; and, secondly, because I leave the retina uninjured, and presenting the appearance described by anatomists, especially the yellow spot of Soemmerring, which is never seen to advantage until this membrane be removed: and hence it is that that conformation, as well as the fibrous structure of the retina in some animals, becomes better marked from remaining some time in water, by which the membrane I speak of is detached.

The extent and connections of this membrane are sufficiently explained by saying, that it covers the retina from the optic nerve to the ciliary processes. To enter into further investigation on this subject, would lead to a discussion respecting the structure

* See Haller, Zinn. &c. loc. cit.

of

of the optic nerve, and the termination of the retina anteriorly, to which it is my intention to return at a future period.

The appearance of this part I find to vary in the different classes of animals, and in man, according to age and other circumstances. In the fœtus of nine months it is exceedingly delicate, and with difficulty displayed. In youth it is transparent, and scarcely tinged by the black pigment. In the adult it is firmer, and more deeply stained by the pigment, which sometimes adheres to it so closely as to colour it almost as deeply as the choroid coat itself; and to those who have seen it in this state, it must appear extraordinary that it should not have been before observed. In one subject, aged fifty, it possessed so great a degree of strength as to allow me to pass a probe under it, and thus convey the vitreous humour covered by it and the retina from one side of the basin to the other; and in a younger subject I have seen it partially separated from the retina by an effused fluid. In the sheep, ox, horse, or any other individual of the class *mammalia* which I have had an opportunity of examining, it presents the same character as in man; but is not so much tinged by the black pigment, adheres more firmly to the retina, is more uniform in its structure, and presents a more elegant appearance when turned down over the black choroid coat. In the bird, it presents a rich yellow brown tint, and when raised, the blue retina presents itself beneath: in animals of this class, however, it is difficult to separate it to any extent, though I can detach it in small portions. In fishes, the structure of this membrane is peculiar and curious. It has been already described as the medullary layer of the retina by Haller and Cuvier*, but I think incorrectly, as it does not present any of the characters of nervous structure, and the retina is found perfect beneath it. If the sclerotic coat be removed behind, with the choroid coat and gland so called, the black pigment is found resting upon, and attached to, a soft friable thick fleecy structure, which can only be detached in small portions, as it breaks when turned down in large quantity. Or if the cornea and iris be removed anteriorly, and the vitreous humour and lens withdrawn, the retina may be pulled from the membrane, which remains attached to the choroid coat, its inner surface not tinged by the black pigment, but presenting a clear white, not unaptly compared by Haller to snow.

Besides being connected to the retina, I find that the membrane is also attached to the choroid coat, apparently by fine cellular substance and vessels; but its connection with the retina being stronger, it generally remains attached to that membrane,

* Element. Phys. t. v. lib. xvi. sect. ii. Cuvier, Leçons d'Anat. Comp. t. ii. p. 419.

 though

though small portions are sometimes pulled off with the choroid coat. From this fact I think it follows, that the accounts hitherto given of the anatomy of these parts are incorrect. The best anatomists* describe the external surface of the retina as being merely in contact with the choroid coat, as the internal with the vitreous humour, but both totally unconnected by cellular membrane, or vessels, and even having a fluid secreted between them: some indeed speak loosely and generally of vessels passing from the choroid to the retina; but obviously not from actual observation, as I believe no one has ever seen vessels passing from the one membrane to the other. My observations lead me to conclude, that wherever the different parts of the eye are in contact, they are connected to each other by cellular substance, and, consequently, by vessels; for I consider the failure of injections no proof of the want of vascularity in transparent and delicate parts, though some anatomists lay it down as a criterion. Undoubtedly the connection between these parts is exceedingly delicate, and, hence, is destroyed by the common method of examining this organ; but I think it is proved in the following way. I have before me the eye of a sheep killed this day, the cornea secured to a piece of wax fastened under water, and the posterior half of the sclerotic coat carefully removed. I thrust the point of the blade of a pair of sharp scissars through the choroid coat into the vitreous humour, to the depth of about an eighth of an inch, and divide all, so as to insulate a square portion of each membrane, leaving the edges free, and consequently no connection except by surface; yet the choroid does not recede from the membrane I describe, the membrane from the retina, nor the retina from the vitreous humour. I take the end of the portion of choroid in the forceps, turn it half down, and pass a pin through the edge, the weight of which is insufficient to pull it from its connection. I separate the membrane in like manner, but the retina I can scarcely detach from the vitreous humour, so strong is the connection. The same fact may be ascertained by making a transverse vertical section of the eye, removing the vitreous humour from the posterior segment, and taking the retina in the forceps, pulling it gently from the choroid, when it will appear beyond a doubt that there is a connection between them.

Let us contrast this account of the matter with the common one. The retina, a membrane of such delicacy, is described as being extended between the vitreous humour and choroid, from the optic nerve to the ciliary processes, being merely laid between

* See Haller, Elem. Phys. t. v. lib. xvi. sect. ii. Zinn. cap. ii. sect. i. § ii. Boyer, Anat. t. iv. p. 113. Sabatier, t. ii. p. 70. Bichat, Anat. Descr. t. ii. p. 447. Cuvier, Leçons d'Anat. Comp. t. ii. p. 418. Charles Bell, Anat. vol. iii. p. 51. Ribes, Mem. de la Soc. Med. d'Emulation, t. viii. p. 633.

them,

them, without any connection, and the medullary fibres in contact with a coloured mucus retained in its situation by its consistence alone. This account is totally at variance with the general laws of the animal œconomy: in no instance have we parts, so dissimilar in nature, in actual contact: wherever contact without connection exists, each surface is covered by a membrane, from which a fluid is secreted; and wherever parts are united, it is by the medium of cellular membrane, of which serous membrane may be considered as a modification. If the retina be merely in contact with the vitreous humour and choroid, we argue from analogy, that a cavity lined by serous membrane exists both on its internal and external surface; but this is not the fact. In the eye a distinction of parts was necessary, but to accomplish this a serous membrane was not required; it is only demanded where great precision in the motion of parts was indispensable, as in the head, thorax, and abdomen; a single membrane, with the interposition of cellular substance, answers the purpose here. By this explanation we surmount another difficulty: the unphilosophical idea of the colouring matter being laid on the choroid, and retained in its situation by its viscidity, is discarded; as it follows, if this account be correct, that it is secreted into the interstices of fine cellular membrane here, as it is upon the ciliary processes, back of the iris, and pecten, under the conjunctiva, round the cornea, and in the edge of the membrana nictitans and sheath of the optic nerve in many animals. Dissections are recorded where fluids have been found collected between the choroid and retina, by which the structure of the latter membrane was destroyed: the explanation here given is as sufficient to account for the existence of this fluid, as that which attributes it to the increased secretion of a serous membrane.

I take this opportunity of describing the method I adopt for examining and displaying these and other delicate parts, a method which, though simple, will, I expect, prove an important improvement in the means of scrutinizing the structure of animal and vegetable bodies. I procure a hollow sphere of glass from two to three inches in diameter, about one-fourth of which is cut off at the part where it is open, and the edges ground down, so as to fit accurately upon a piece of plate glass, the surface of which is also ground: the object to be examined is attached to a piece of wax fastened upon the plate of glass and immersed in a basin of water, with the cut sphere, which is inverted over it, of course full of water, and the whole withdrawn from the basin. The part may thus be examined under the most favourable circumstances; it floats in water, the only method by which delicate parts can be unfolded and displayed: the globular form of the vessel answers the purpose of a lens of considerable power and perfection, at the same time that it admits light in any quantity or direction to illuminate the object; and, what is of the utmost importance, a preparation of the greatest delicacy may thus be handed round a class in safety.

XVIII. *Notices respecting New Books.*

An Essay on Magnetic Attractions: particularly as respects the Deviation of the Compass on Shipboard, occasioned by the local Influence of the Guns, &c. With an easy practical Method of observing the same in all Parts of the World. By Peter Barlow, of the Royal Military Academy. 8vo. pp. 187, with a Plate. Price 6*s*. 6*d*.

The great practical importance of the subject investigated by the author, and the happy result that has flowed from his assiduous labours, may be partly understood from the following sketch of the contents of this valuable little work; but its full merits can be fully appreciated only by a careful perusal of the essay itself, in which the results of numerous laborious experiments, conducted with much judgement, care and patience, are condensed in tabular forms.

Remarks on the Magnetic Experiments recently published by Mr. Barlow *in his Essay on Magnetic Attractions, &c.*

In No. 258 of the Philosophical Magazine are given two papers from the Philosophical Transactions on the subject of local attraction: the one by W. Scoresby jun. Esq., and the other by Captain Sabine: the latter containing the observations made on board H. M. SS. Isabella and Alexander, in their late voyage in search of a North-west Passage.

The great importance of this error in practical navigation is placed in a very conspicuous point of view by Mr. Bain in his valuable little treatise on the "Variation of the Compass," and the two papers above referred to fully corroborate the statements of that author. It appears indeed from the observations of Captain Sabine, that in certain high latitudes, the deviation arising from this cause becomes immensely great, amounting in some cases to at least half a quadrant.

Mr. Bain is of the opinion, and few will be disposed to say it is ill founded, that many of the distressing losses frequently experienced at sea are owing to this source of error: he shows in fact, that in consequence of local attraction, a vessel in our own latitudes

tudes may frequently be steering a point out of her supposed course; and it requires but little knowledge of navigation to be well convinced of the immense importance of such an error, particularly in the Channel, and in cloudy weather, when the compass is the seaman's only friend and guide.

It appears moreover from the memoir of Captain Sabine, and the same was also pointed out by Captain Flinders and Mr. Bain, that the error from local attraction is not only different with the ship's head on different points of the compass, but that it varies also in different latitudes; the quantity depending upon the inclination of the dipping needle at the place of observation. Captain Flinders indeed does not appear to have made a correct estimate of the effect of the dip in changing the quantity of the deviation; but he had the honour of being the first to establish the connexion between them, and thus conferred a lasting obligation upon the nautical profession, and has given to his name a permanent and perspicuous situation in the annals of that science. An actual deviation caused by the action of the guns being thus rendered obvious by the concurring testimony of so many distinct observers; and the only rule ever yet proposed for the correction having been distinctly shown to be inadequate for the purpose, by all latter observations; it became a fair and laudable subject of contest with men of science, to discover some formula, or other method, for correcting these anomalies in all parts of the world.

Mostly, however, the attempts were confined to the formation of certain empirical formulæ, from the results of observations on ship-board; but these, it is obvious, are at present too limited for such a purpose, when we consider that every degree of latitude gives rise to a new series of deviations.

Mr. Barlow pursued a different course. Being placed in a situation where there is perhaps a greater accumulation of manufactured iron than in any place in the world, he proposed to avail himself of this opportunity, and to undertake a regular series of experiments on different masses of iron, fixing his compass at different distances, and in every variety of position; and by this means he has succeeded, not only in discovering an extremely simple method of correcting the deviation in question, but also in developing several magnetic laws which are highly curious and interesting. It is indeed surprising, considering the interest which the science of magnetism has for more than a century excited, that no course of experiments of this kind has been before undertaken, and that instead of examining in all cases the action of magnet on magnet, the inquiry had not suggested itself, of determining the laws between plain iron and the compass.

pass. Such, however, appears to have been the case; and Mr. Barlow has in consequence the honour of having discovered several important laws which promise to throw considerable light upon this mysterious subject.

After a few minor trials, this gentleman procured from the Royal arsenal a solid 13-inch ball weighing nearly 300 lbs.; and having constructed a proper apparatus, whereby he could place the compass at any required distance from the centre of the ball, and in any proposed situation, he commenced his course of experiments, and was soon led to the discovery of a very remarkable fact, namely, that if we conceive a plane to pass through the centre of any iron ball, inclining from the magnetic north to south, and forming with the horizon an angle of about 20°, a compass placed any where in that plane will not be affected by the iron, but will point duly north and south, the same as if no iron were in its vicinity. It occurred to Mr. Barlow afterwards, that as the dip in this country is at the present time about 70°½, the inclination of the above plane of no attraction, if examined a little more particularly, might be found to be exactly the complement of the dip; and subsequent experiments, on an improved apparatus, and on an excellent dipping needle, confirmed this conjecture; from which coincidence the author is led to infer, that the same obtains in all latitudes; namely, that the plane of no attraction is every where perpendicular to the natural inclination of the dipping needle. We have spoken of the plane of no attraction as passing through the centre of the ball in a direction perpendicular to the inclined needle; but it is obviously the same, if we suppose the plane to pass through the pivot of the compass, and the centre of attraction of the iron to be situated in that plane; which latter conception is to be preferred in all cases where irregular masses of iron are employed.

This very remarkable fact being once well established, Mr. Barlow made it the ground-work of all his subsequent experiments: for example, he very naturally conceived a circle to be described in this plane concentric with the pivot of the needle; and conceiving at the same time an imaginary sphere to be circumscribed about the latter, this circle became to him an equator; the line joining the extremities of the dipping needle, the poles; and the circle passing through those poles, and the east and west points of the horizon, a first or principal meridian; by means of which every particular situation on the sphere might be immediately indicated by its latitude and longitude, the same as on the terrestrial surface.

A long course of experiments were now undertaken, in order to discover the law of deviation, as it depended upon the position of of the ball and compass, the distance between the two being always the same. From these it was discovered, that while the longitude was zero, the tangents of the angles of deviation were proportional to the sine of the double latitude; and while the latitude was constant, the tangents were proportional to the cosine of the longitude: so that in all cases, while the distance and mass were constant, the deviation was expressed by the formula

$$\tan. \Delta = A (\sin. 2\lambda. \cos l.)$$

where Δ denotes the angle of deviation; λ the latitude, and l the longitude, on the ideal sphere above mentioned; and A a constant coefficient to be determined by experiment.

The next object of the author was to ascertain the law as respects the distance: and here again he was equally successful: the results showing in the most unexceptionable manner, that all other things being the same, the tangents of deviation were inversely proportional to the cubes of the distances. It still remained to determine the law as regards the mass; and with this view, balls of different diameters were employed, from which it seemed to follow, that the tangents of deviation were proportional to the masses, as to the cubes of the diameters.

Mr. Barlow appears in the first instance to have adopted this conclusion, and to have considered his experiments on regular bodies as completed; when out of mere accident, or curiosity, he made trials with certain shells of iron of the same diameter as his balls, from which results were obtained as singular as they were unexpected; viz. that the balls and shells of the same diameter gave the same deviation; so that it appeared that the latter was independent of the mass, and had only reference to the quantity of surface. This led to several experiments on iron plates, each a foot square, but of various thicknesses; from which it is made manifest, that after the thickness of the iron surpasses a certain quantity, (about $\frac{1}{30}$th of an inch) no addition of power is gained by an increase of thickness; in fact, that magnetism like electricity resides wholly in the surface.

Introducing all these circumstances, the general formula for expressing the quantity of deviation becomes

$$\tan. \Delta = \frac{D^3}{A\delta^3} (\sin. 2\lambda. \cos l.)$$

where D denotes the diameter of the ball; δ the distance, and Δ, λ and l, the same as before.

All these experiments, it will be observed, were performed on regular masses of iron, and it might still be doubtful whether they would be found to obtain on irregular masses. With a view to this determination, Mr. Barlow commenced a long course of experiments on a 24-pounder, mounted upon what is termed a

 traversing

traversing platform, by means of which it could be traversed, or carried round, in a complete circle. These experiments confirmed all his preceding deductions, showing that the plane of no attraction, and all the laws and circumstances attending regular masses of iron, obtained also on those the most irregular; and consequently that they might be immediately applied to determining the local attraction of the guns, &c. on shipboard. The comparison is accordingly made between the observed deviations on board the Isabella, as given by Captain Sabine, off Shetland, and those computed on the above principles; and the coincidence between them is very striking, considering all the circumstances of the case; viz. the change in the dip, the very irregular distribution of the iron in the ship, the particular situation of the compass, in the vessel in question; and lastly, that the laws themselves had been inferred from experiments made in one latitude only, and on masses of iron of the most regular figure.

One very serious objection may however be made to this, as well as to any other *rule* for computing the quantity of deviation in different latitudes; which is, that the dip at the place of observation is a necessary datum, and it is one that is not readily obtained. This induced Mr. Barlow to look for some other method for determining the error in question, the theory of which may be explained in few words.

Since it was rendered obvious from his experiments on the 24-pounder, that all the action of the mass might be referred to one common centre of attraction; it followed, that so also in a ship, the whole might be reduced to one centre of four; and since the iron and compass will always, during the voyage, preserve the same or nearly the same relative situation, it is assumed, that a single ball of iron might be so pointed in the ship, that its action on the needle would be the same as that of the iron in its distributed state; or, which is still the same, that there is one common resultant. This being admitted, it follows from the laws previously established, that if a line be conceived to be drawn joining that centre with the pivot of the needle, it will be possible to introduce a less ball of iron, having its centre of attraction in the same line, (which is to be approximated so much nearer as it is less,) that shall produce exactly the same effect as the great ball at a greater distance, or as the iron in its distributed state. So that, whatever effect the guns, &c. may produce in any situation of the vessel, the above ball being placed in its fixed situation, that effect will be doubled; whence the quantity of deviation itself becomes determined.

To be a little more explicit: A vessel having got her guns, &c. on board, before she sails, is to be warped round, and the deviation

deviation caused by the guns is to be determined: this being known, a pin is to be fixed experimentally to the binnacle, in such a situation, that when a ball or plate of iron is slipped on, and the binnacle turned about, the deviations arising from the ball or plate shall be the same as those produced in the like situations by the guns.

This being done, the plate (which Mr. B. adopts in preference to the ball) is to be laid aside; then, at any time when it is desirable to ascertain the local effects of the guns, &c. the plate is to be applied in its assigned situation, and it is to be observed how much it draws the needle out of its prior direction; and just so much will the guns have deflected it before the experiment. This being ascertained, and the course of the vessel corrected accordingly, the plate is to be again removed, and laid aside till some new circumstance renders its application again necessary.—Several experiments are then reported as they were made with an attached plate and the 24-pounder above mentioned, which were repeated before some of the Lords of the Admiralty, and apparently much to their satisfaction.

Nothing can be desired more simple than this method of correcting the errors arising from local attraction; and we are glad to find that the Board of Longitude has approved of the principle, and has recommended its adoption to the Admiralty, who it seems are about to give it a proper trial in both hemispheres of the globe.

Having dismissed this subject, Mr. B. turns his attention to an explanation of the cause of the daily variation of the compass; a phenomenon which has never yet been explained in a manner entirely satisfactory, and which certainly appears to have an intimate connexion with the laws and principles discovered in the early part of the experiments above referred to. Mr. B., after enumerating all the known circumstances attending this phenomenon, observes: "It appears then, that any hypothesis which may be advanced to account for the diurnal variation of the needle ought to be competent to the explanation of the following facts, viz.

1. That the general character of the daily change in the direction of the needle shall be westerly in the morning and easterly in the evening.

2. That although the above is the general phenomenon, yet this is sbbject to certain modifications, indicated by the letters W.E.; *e*WE, *e*WE*w*, where the small italics denote a less change than the Roman letters.

3. That the variation in the night, or while the sun is below the horizon, is very inconsiderable with respect to that which takes place in the day-time.

4. That

4. That the greatest daily variation takes place either at, or a little before and after, the summer solstice; and the least at the winter solstice.

5. That the greatest daily variation happens now earlier in the day, than it did at the beginning and middle of the last century.

6. And lastly, that the daily variation is less at the equator than in our latitudes.

Mr. B. then proceeds to explain that all these peculiarities are the necessary consequences of supposing the sun to possess a certain degree of magnetic influence, such as that which belongs to every mass of plain unmagnetized iron; and of admitting the existence of the plane of no attraction, and the other magnetic laws indicated in the preceding part of this article. He shows, for instance, that at one time in the year the sun will, both at its rising and setting, be above the plane of no attraction; that at another it will both rise and set below the plane; while at others it will rise above and set below, &c.; and that this consideration fully explains those modifications in the daily variation as denoted above. He shows, moreover, that the least daily variation ought to happen at the winter solstice, the rectangle (sin 2 λ. cos *l.*) being then the least; that there ought to be a second minimum at the summer solstice, although the rectangle (sin 2 λ. cos *l.*) is then much greater than in the former instance; and that a little before and after this solstice the daily variation ought to be at its maximum. He proves also, that if 14′ be reckoned the greatest daily variation in our latitude, it ought not to exceed 3′ at the equator; and lastly, that it ought to be nothing or very inconsiderable during the night.

We will not presume to give a very decided opinion as to the accuracy of the above view of this interesting phenomenon; but we venture to say, that it is by far the most complete illustration of it that has ever yet been offered, and that we have little doubt it will be generally adopted.

Description of an Apparatus, by which 25,000 *cubic Feet of Gas are obtained from each Chaldron of Coal, without producing either Tar or ammonical Liquor.* By SAMUEL CLEGG, Civil Engineer, Mawbey-place, South Lambeth, Surry. 8vo. pp. 16, with a large Plate of the Apparatus. 2*s*.

In his introductory remarks Mr. Clegg, who has had more experience, and introduced more improvements in the manufacturing of coal gas, in the large way, than perhaps any person who has applied his time and talents to this branch, after alluding to the numerous improvements previously introduced by himself, and

and the various gas establishments which he has erected, proceeds as follows:

"In this wide field of practice, I have had the most favourable opportunities of ascertaining the processes best calculated to produce, œconomise and purify gas; and I am now happy to be able to lay before the public a description of an apparatus (for which I have obtained a patent) that combines the advantages of œconomy, regularity, certainty of operation and neatness. And, by the application of mechanical and chemical powers, the coal undergoes in its distillation an entirely new arrangement of its principles, owing to the rapid decomposition and uniformity of temperature, so that the whole of the hydrogen combines with the charcoal, constituting olefiant gas: thus is the illuminating power increased with the quantity produced; and when passed through cream of lime, we have a gas equal in illuminating power to the gas from common lamp oil *, and as free from sulphuretted hydrogen, with a difference of expense as 1 to 18, as stated page 143.

"On any plan of preparing coal gas hitherto adopted, this result has not been obtained. The coal having never been completely decomposed, a considerable quantity of combustible matter has been suffered to escape in the form of tar and ammoniacal liquor. Thus substances not only of little or no value, but such as were deemed nuisances inseparable from the preparation gas, were produced in great abundance. To obviate these inconveniences, which have in many instances prevented the introduction of this invaluable discovery, I apply the coal in thin strata; by which mode, I have found from repeated experiments upon an extensive scale, that Newcastle Wall's End coal will yield per chaldron 25,000 cubic feet of gas, of a very superior quality; whereas upon the former plan, 10,000 cubic feet is the utmost average quantity which has been obtained †. Besides, as the matter which has usually escaped in the form of tar and ammoniacal liquor is completely decomposed, not only an increased quantity, but a very superior quality of gas is obtained. To produce these effects, the strata of coal ought not to exceed half an inch in thickness, and must be introduced to a red heat, without the retorts being exposed to a mass of fresh coal; for this lowers their temperature so much that several hours are requisite to raise them to their proper degree of heat: hence so large a proportion of tar-water and inferior gas is produced, besides the loss of time and fuel. As the coal is introduced in small portions

* See Dr. Henry's paper in the Literary and Philosophical Transactions of Manchester, vol. iii. Second Series, or the Philosophical Magazine for August 1819, and Accum's last Treatise on Gas Lights, pp. 129, 130.

† The difference in the quantity of gas produced by the two modes of operation bears the same proportion, whatever description of coal is used.

by

by a mechanical process, independent of the stoker, a uniformity of temperature and regularly thin strata are preserved; and as it is applied to the machine only once in twelve hours, at which time also the retorts are cleaned out, there is a considerabls reduction of labour, and an increased certainty of operation.

"A greater quantity of gas being obtained by the mode now described, there will evidently be a considerable saving of expense in the coal necessary to produce it, and in the quantity of fuel employed. The saving in the former will be proportioned to the increase in the quantity of gas; and as that is more than double, the saving will amount, at least, to one half; and as the above-mentioned quantity of gas can be produced at the heat usually applied, and in a considerably shorter period than is now required, there will be a saving also in fuel of more than one half, Besides, one chaldron of coal introduced into the retort upon the proposed plan, produces two chaldrons of coke of excellent quality, which finds a ready sale at 25*s*. per chaldron, when the coal is at 50*s*.

"Another advantage accruing from this mode of operation, is a very material saving in the wear and tear of the apparatus. Every one conversant with gas-works knows, that upon the plans in general use, when 10,000 feet of gas are produced, the retorts must be renewed at least once in six months. Upon my plan they may be kept in good working order eighteen months.

"Another very important advantage consists in avoiding the nuisances hitherto inseparable from the preparation of coal gas. The greatest inconveniences attending it have been the production of tar and ammoniacal liquor, and the accumulation of the lime-water used in purifying the gas. The two first are completely obviated by the proposed process of decomposing the coals, neither tar nor ammoniacal liquor being produced in the operation. The latter inconvenience is prevented by the use of lime and water in a semi-fluid state, which in being discharged from the machine soon becomes consolidated, and may easily be removed, and applied to any purposes for which lime is required.

"To produce these advantages, I have constructed an apparatus, a part of which may be applied to works already established, and of which the following is a description.

Description of the Apparatus.

"The retorts are similar to those described in Accum's last Treatise on Gas-Lights, under the head 'Rotative Retorts,' with this difference, that the *whole* retort is heated to the required temperature, and the space between the arms covered with iron plates, the wide end of which projects upwards to the height to which I propose to allow the coke to accumulate. The whole forms

forms a cylindrical dish, nearly of the same diameter as the retort. On the upper part of the retort, and adjoining the door, I place a hopper, sufficiently large to contain coal for twelve hours' consumption. The coal is previously screened, and all lumps excluded, larger than the required thickness of the strata to be laid in the dish. The lid of the hopper is kept tight by means of an hydraulic joint. To that part of the hopper where the coal is discharged, is attached a cone, the difference of the diameters of whose ends is in exact proportion to the difference of the space described in their revolutions by the parts of the dish over which it is placed. On the surface of this cone, a number of thin bars are fixed longitudinally; each about the depth of the stratum of coal to be applied. The cone is made to revolve upon its horizontal axis, at the same time that the dish revolves horizontally; and by varying their relative velocities, the thickness of the stratum may be varied *ad infinitum* *.

"The products arising from the coal thus introduced, are conveyed by a pipe to the condenser, which condenser forms the principals for supporting the roof over the retorts. From the condenser all the fluids are conveyed back by a pipe to the retort, drop by drop, as they are condensed; and as these drops fall upon the red-hot coke as it revolves, they are all completely decomposed †.

"The lime machine is constructed upon the same principle as that described in Accum's last Treatise, but much simplified and improved. By using lime and water in a semi-fluid state, a smaller quantity of lime is required for absorbing the sulphuretted hydrogen gas ‡, while, at the same time, lime of this consistence is as completely free from the obnoxious fluid produced by lime-water, as if dry lime had been used. As the quantity of gas absorbed in the process of purifying is proportioned to the bulk of water employed, it is obviously more œconomical to use lime and water in a semi-fluid state; to which it may be added, that gas purified by this mode possesses higher illuminating power. In proof of these assertions, I refer to a paper in the Philosophical Magazine for September 1819, by Dr. Henry, pp. 164–166.

"From the lime machine the gas is conveyed to the large meter §, where every foot is registered, and finally into the collapsing gas-holder, a machine preferable to any now used, and constructed at half the expense ||.

* When the stratum of coal is about half an inch in thickness the cylindrical dish makes one revolution in forty minutes, the velocity varying with the thickness of the stratum.

† This is a more œconomical and certain method of decomposing oil, tar, &c., than any which has been offered to the notice of the public.

‡ For the purity of the gas, after having passed through cream of lime, see the Philosophical Magazine for February 1819, p. 138, No. 250.

§ The utility of this machine is illustrated in Accum's Treatise on Gas Light, art. *Gas-Meter.*

|| See Accum's Treatise, art. *Collapsing Gas-holder.*

"The following statement shows the difference in the expense of working an apparatus of the construction here described, and one in which the cylindrical or elliptical retorts are used, each being supposed to produce 50,000 cubic feet of gas in twenty-four hours, taking the average of the whole year.

OLD PLAN.

To annual wear and tear of 60 retorts, including brick-work, &c.	£1020	0	0			
To six workmen, three during the day and three at night, at 1*l.* 6*s.* each the week	405	12	0			
To coals, 1825 chaldron, requisite for producing the gas, at 2*l.* 8*s.* the chaldron	4380	0	0			
To wear and tear of grate bars, fire irons, &c.	42	0	0			
To 556¼ chaldron of coal for fuel, at 2*l.* 1*s.* the chaldron	935	6	3			
Total expense				£6782	18	3
Cr.						
By 1825 chaldron of coke, at 30*s.*	2737	10	0			
By 456¼ of small coke, at 10*s.* the chaldron	228	2	6			
				2965	12	6
				£3817	5	9

NEW PLAN.

To annual wear and tear of two retorts, including brick-works, &c.	£450	0	0			
To six workmen, three by day and three by night, at 1*l.* 6*s.* each the week	405	12	0			
To coals, 800 chaldron, requisite for producing the gas, at 2*l.* 8*s.* the chaldron	1920	0	0			
To 160 chaldron of coal for fuel, at 2*l.* 1*s.* the chaldron	328	0	0			
To wear and tear of grate bars, &c. for two fire places *	20	0	0			
Total expense				£3123	12	0
Cr.						
By 1600 chaldrons of coke, at 25*s.*				2000	0	0
				£1123	12	0
Making a difference, in favour of the New Plan, of				£2693	13	0

* The number of fire places being thus reduced, the excellent plan of Mr. Brunton, of the Eagle Foundry, Birmingham, for burning the smoke, can be applied with great advantage.

"From the above statement it appears that, in addition to a considerable diminution in the amount of capital sunk, (the first cost of the new apparatus being much less than that of the old,) there is in the new plan a saving of three-fourths of the annual expenditure. The tar and ammoniacal liquor have in no instance paid the expense of collecting and casks; I have, therefore, not noticed them. I hope soon to be able to lay before the public the exact difference, in point of value, of the gas, by the two modes of operating: suffice it to say, at present, that where the gas, upon the old plan, sells for 15*s.* per 1000 cubic feet, the gas produced upon the new plan is worth 20*s.* at least, making a difference in the annual rental, where 50,000 cubic feet are sold daily, of 4562*l.* 10*s.*

"The expense of producing an equal quantity of light with oil gas (taking Messrs. Taylors' own statement, as given in the Journal of Sciences and Arts, vol. vi. p. 108,) would amount to the *enormous* sum of 19,010*l.* 8*s.* 4*d.* To prevent discussion, I have allowed, with Messrs. Taylor, that an equal volume of oil gas will produce double the quantity of light of coal gas.

"The annexed table points out the illuminating power of different gases, as well as the quantity of azote, sulphuretted hydrogen, and carbonic acid, formed, from which the injurious effects of letting the coal remain so long in the retorts will be seen. The only correct mode of ascertaining the illuminating power of different gases, is by the quantity of oxygen condensed by a given volume of inflammable gas, that gas having the greatest illuminating power which condenses the greatest quantity of oxygen.

TABLE

Showing the Quality of the Gas from common Coal, at different Periods of the Distillation, likewise the illuminating Power of Gas from Oak, dried Peat, Lamp Oil, Wax, &c.*

	100 m. of impure gas.		100 measures of purified gas.				
	Sulp. Hyd.	Carb. Acid.	Olef. Gas.	Carb. Hyd.	Azote.	Cons. Oxy.	Gives Car. Acid.
1 hour's gas . . .	3	3	10	90		164	91
3 hours' gas . . .	2	2	9	91		163	93
5 hours' gas . . .	3	2	6	94		132	70
7 hours' gas . . .	1	3	5	80	15	120	64
9 hours' gas . . .	1	2½	2	89	9	112	60
Gas from oak . .						54	33
Gas from dried peat .						68	43
Gas from Cannel coal						170	100
Gas from lamp oil .						190	124
Gas from wax . .						220	137
Olefiant gas . . .						284	179

* From Dr. Henry's paper referred to in a preceding note.

"From

"From the above table it appears, that the proportion of illuminating power between gas from lamp oil, and gas from Cannel coal, by the common method of operation and the mean time of the distillation, is as 190 to 170, and from common coal as 190 to 140."

We only add one remark. To increase the product of a chaldron of coals from 10,000 to 25,000 cubic feet, and at the same time of a superior quality, is incomparably the greatest improvement that has yet been made since gas was introduced for the purpose of general illumination.

An Introduction to Solid Geometry, and to the Study of Crystallography, containing an Investigation of some of the Properties belonging to the Platonic Bodies independent of the Sphere. By N. J. Larkin, M. G. S. Teacher of Crystallography and Mathematics. Illustrated with four plates from original drawings by the Author. 8vo. pp. 140.

The work under consideration contains a description of a variety of solids hitherto unnoted, and a number of new and remarkable properties of those solids that have been long known. In tracing the properties of the Platonic bodies, the author shows that they naturally divide themselves into two series, each consisting of five solids; and what is remarkable, that each individual solid, in one of the series, is to be found in great abundance among crystals, whereas not a single individual in the other series has ever been found among such productions. The first he calls the natural; the other, the artificial series. These two series bear a strong resemblance to each other; inasmuch as the last in each series contains all the foregoing in the same series: the angular points of the contained solids may be traced out in the surface of the last solid; and what perhaps is equally remarkable, is, that the whole of the solids, composing the natural series, are commensurable with each other when the first four are contained in the last, and that they are to each other as the numbers 1, 3, 4, 6, and 8. There is another solid whose extremities may be traced out in the surface of the last of the natural series; which solid the author calls a cuboctahedron on account of its being intermediary between the cube and the octahedron: this solid, though it is commensurable with the rest, is not so simple, being as $5\frac{25}{27}$: consequently it is somewhat less than the fourth, being to it as 80 : 81. The author has combined the solids belonging to the natural series in pairs, in every possible manner, and given the ratios of their volumes in two tables: he has likewise given the ratios of a number of remarkable lines in or upon these solids, and has shown how each may be extracted from the others. The ratios between the members of the artificial series appear to be incommensurable,

mensurable, except in one instance, on which account they make a very striking contrast with the natural series, which are all commensurable. He afterwards describes five distinct dodecahedrons, which all admit of indefinite variation, and which, with the two before described, make seven; the whole of which are shown to be singularly related with the cube. The descriptive part is followed by a series of demonstrations contained in fifty-three theorems, concluding with an Appendix, by Dr. Roget, containing a demonstration of the relations subsisting between the numbers of the artificial series, and likewise between their faces and their axes. The whole is illustrated by four plates, engraved in a superior manner by Mr. and Miss Lowry; the third plate is remarkably well éxecuted, and is a flattering specimen of that young ladies abilities.

Upon the whole, the work will be found of great service to prepare the mind for the study of crystallography; and at the same time, highly interesting to the mathematician. Indeed it is the only work in the English language in which the various properties of the geometrical solids are particularly described; on which account it cannot fail to be acceptable.

Lately published,

A Dictionary of Natural History; or Complete Summary of Zoology, embellished with 140 interesting Subjects. 9*s.* coloured 12*s.*

An Elementary Treatise on Mechanics, Vol. 1, containing Dynamics. By W. Whewell. 8vo. 15*s.*

An Analytical Essay on the Construction of Machines, illustrated by 13 lithographic prints. 4to. 18*s.*

A Historical Map of Palestine, or The Holy Land; exhibiting its geographical Features, and those Names of Places which accord with the Scripture Narrative. Size 40 inches by 27$\frac{1}{4}$. 1*l.* 18*s.*

Travels in Nubia and the Interior of Eastern Africa. By J. L. Burckhardt. With a Life and Portrait of the Author. 4to. 2*l.* 8*s.* —This is a very interesting work.

Index Testaceologicus; or, A complete Catalogue of Shells, British and Foreign, arranged according to the Linnæan System, with the English and Latin Names, References to Figures, and Places where found. By W. Wood, F.L.S. 14*s.*

The Natural History of British Quadrupeds, with coloured Figures, accompanied by scientific and general Descriptions of all the Species that are known to inhabit the British Isles. By J. Donovan, F.L.S. Vol. 1. 1*l.* 16*s.*—To be completed in 3 vols. royal 8vo.

Dialogues on Entomology, illustrated with 25 engravings. 12mo. 12*s.* plain, 18*s.* coloured.

In the Press.

Two 4to Volumes, with Maps, A Geographical, Statistical, and Historical Description of Hindostan and the adjacent Country. By Walter Hamilton, Esq.

Taxidermy; or A Complete Treatise on the Art of Preserving every Object of Natural History for Museums; with Lists of those that are rarest or most wanted in European Collections. In a small 8vo Volume.

The Marquis of Worcester's Century of Inventions, from the Original MS. with Notes, a biographical Memoir, and a Portrait.

Journal of a Tour in Greece, Egypt, and the Holy Land; with Excursions to the River Jordan, and along the Banks of the Red Sea to Mount Sinai. By W. Turner, Esq. Foreign Office. 3 vols. 8vo.

A Journal of a Tour through Part of the Snowy Range of the Himala Mountains, and to the Sources of the Jumna and the Ganges, in 1815. By T. B. Frazer, Esq. Royal 8vo.

Twenty Views, illustrative of Mr. Frazer's Journal, elephant folio.

Memoirs of the late R. L. Edgeworth, Esq. partly written by Himself, and continued by his Daughter, Miss Edgeworth. 2 vols. 8vo.

M. Lamouroux, Professor of Natural History in the Royal Academy of Caen, is preparing to publish a work (with 40 plates) on the Marine Polypi found in great abundance in the calcarious Formation of Lower Normandy. This work, which will be useful to Geologists, will include a figure and description of the fossil crocodile discovered near Caen.

XIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Jan. 13. Concluded the reading of Mr. Herschel's elaborate paper, On the Action of crystallized Bodies on Homogeneous Light, and on the Causes of the Deviation from Newton's Scale of Tints which many of them develop on an Exposure to a polarized Ray.

A paper

A paper by Dr. Granville was read, giving an account of a case of Ovario-gestation.

Jan. 20. Commenced reading a paper On some new Combinations of Platinum, by Edmund Davy, Esq.

NEW ASTRONOMICAL SOCIETY.

A new Society has just been formed for the encouragement and promotion of *Astronomy*, which promises to be attended with the most beneficial advantages, and appears highly deserving of public support. Indeed, considering the patronage which is given to most of the Arts and Sciences in this country, it is somewhat singular that no Institution should hitherto have been formed for the advancement of this particular branch of Science—the most noble of them all. The views and objects of this Society will be best seen by the *Address* which they have published; and which we hope to give at full length in our next Number; together with a List of the Officers that have been chosen for the direction and management of the affairs of the Society; and which comprehends some of the first astronomers and mathematicians in the country.

XX. *Intelligence and Miscellaneous Articles.*

CONICAL BALLS.

To Mr. Tilloch.

Chatham, Feb. 5, 1820.

SIR,—THE Philosophical Magazine, Number 261, contains some remarks on what are called conical balls; it may be not altogether uninteresting to your Glasgow correspondent, to know that balls of an egg form were used by the Russian guards in 1811. The inventor, or perhaps more correctly speaking, the person who introduced that form, received from the Emperor the sum of 5000 rubles as a mark of His Imperial Majesty's approbation. It is certainly possible, that the gentleman who claimed as his invention that form of shot, did not know that in England the same shape had been recommended some years before. In the European Magazine for 1804 the above form is strongly recommended; I do not recollect by whom, nor am I *quite certain* with regard to the date: but doubtless it was long before their introduction into the Russian army, and also previous to 1808, the year in which Mr. Robertson read his Essay on that subject.

I believe it is generally admitted that the rockets used in war are not so efficient as might be expected, in consequence of the uncertainty of their striking the intended object. If any of your correspondents will have the goodness to inform me whether an attempt has ever been made, to give them motion round their longitudinal axis in their progress through the air, similar to a rifle ball or arrow, they will much oblige me.

Query—Could the required motion be given by attaching two or more narrow slips of thin iron, in a spiral direction, on the case, similar to the feathers on an arrow, but placed on the contrary end, or by forming two holes diametrically opposite at the anterior end, on the same principle as Dr. Barker's mill, or as fire-works are sometimes exhibited whirling on their axis? So far they would differ, that those have only a circular motion, the rocket should have that combined with its progressive motion: no doubt some portion of the impelling matter would be diverted; consequently the range shortened: even with this deduction, the certainty of the flight might be more than a compensation.

I am, sir,

Your most obedient, &c.

I. K. K.

MILITARY ROCKETS.

Baron de Zach announces, that Capt. Schumaker (brother of the Astronomer Royal, Copenhagen) has invented a rocket superior to Congreve's both in force and in the precision with which they are thrown. A new corps has been formed to use these missiles. They ascend to an immense height, and then exhibit a globe of fire, which may be seen at a distance of seventy miles.

SANDSTONE OF CAITHNESS, &c.

To Mr. Tilloch.

January 1, 1820.

Sir,—In my work on the Western Islands of Scotland, I have suggested (vol. ii. p. 104) that the sandstone of Caithness was similar to that of the western coast of Ross and Sutherland, and have also questioned whether that of Cromarty and Aberdeenshire does not belong to the same species. Since that book was printed I have had an opportunity of examining the districts in question; and, that I may not mislead future geologists, I beg to say that the last of these suggestions is unfounded, and that I have not as yet been able to satisfy myself respecting the geological relations of the sandstone of Caithness. It is not in my power at present to enter into the details necessary for elucidating this subject. I am, sir,

Your obedient servant,

J. MacCulloch.

ISLE OF ELBA.—MAGNETISM.

Baron de Zach announces in his "*Correspondence*," vol. i. that the opinion long entertained, that the Isle of Elba, from the quantity

quantity of iron ore found on it, and especially Mount Calamita, (supposed to be a solid mass of loadstone,) has a sensible effect on the mariner's compass, is unfounded. Mr. Charles Rumken in 1818 could not find, at the distance of two or three or four nautical miles, the declination of his needle affected in the least by the action of the island.

FRENCH ROCK SALT.

In July last a regular search for rock-salt was commenced at Moyenire in the department of La Meurthe. At the depth of 200 feet they found a bed 11 feet thick; below this the workmen perforated a bed of gypsum and clay of 546 feet, when they reached another bed of salt eight feet thick. The salt of the first bed is very white and transparent, and very pure: the second contains a small portion of gypsum and argillaceous substances, and is brownish like clouded flint. It contains hardly any muriate of magnesia or sulphate of lime. Both present a cubical fracture.

COMPARATIVE STRENGTH OF EUROPEANS AND SAVAGES.

[From Bulletin No. 156 of the Society of Encouragement in France.]

M. Peron, the naturalist, has had occasion to observe, that men in a savage state are inferior in strength to men civilized; and he has demonstrated in a very evident manner, that the improvement of social order does not, as some have pretended, destroy our physical powers. The following is the result of experiments which he has made on this subject with the Dynamometer of M. Regnier (described Phil. Mag. Vol. I.)

Comparative Experiments on the Strength of Europeans and Savages.

		Force.	
		With hands.	With traces.
Savages	Of Diemen's Land	50. 6	0. 0
	New Holland	51. 8	14. 8
	Timor	58. 7	16. 2
Europeans	French	69. 2	22. 1
	English	71. 4	23. 8

NAUTICAL EPHEMERIS.

To Mr. Tilloch.

It is but a short time since I submitted to you a few remarks on the contracted supply of the Nautical Almanac, which you honoured with a place in No. 259 of your valuable Journal. My anxious feeling for the safety of British maritime commerce again prompts me to state, that the Nautical Almanac for 1821 has been out of print nearly three months, and the new edition is not yet issued from the press. During this period many valuable ships have sailed on distant voyages; nine of the East India Company's ships to my knowledge: six of these from 1200 to 1350 tons burthen, all destined first to India and afterwards to China: several of these have not been able to procure a Nautical Almanac for 1821; and as some of them will be obliged to proceed from India by an eastern passage to China, they will have to navigate in the Pacific Ocean early in 1821 destitute of a Nautical Almanac for that year, if they do not obtain one prior to their departure from India, which is very improbable: and it is possible some of these valuable ships may be compelled to navigate afterward from China to England, without possessing that essential requisite to the safety of navigation.

Feb. 22, 1820. AN OBSERVER.

MATHEMATICAL PUBLICATIONS.

To Mr. Tilloch.

SIR,—I perfectly concur with the opinion given in your useful Magazine for November 1819, page 368, of the advantage arising from the mathematical periodical publications, and feel confident that the books there noticed, have conjointly with others, and particularly the very respectable and well conducted Gentleman's Mathematical Companion, uniformly afforded a stimulus to mathematical inquiries, which could not fail to produce mathematicians of the first rank. But the reason of my troubling you at present, is to express my regret at not observing the name of the Gentleman's Mathematical Companion, which commenced in 1799 and was annually and regularly continued to the present time, mentioned in an article apparently written for the laudable purpose of drawing the student's attention to a good collection of such publications. As I have been for many years a correspondent to various periodical productions, and am in consequence well acquainted with them, I beg to recommend the above-named work to the notice of those who may be desirous to possess an advantageous selection, through the medium of your Philosophical Magazine. With the idea of replacing what appears to me to be an omission in your valuable pages,

I remain with respect your obedient servant,

A CORRESPONDENT.

NEW BLOCK.

Specification of a new Block for which a Patent has been obtained.

The advantages in the carcase over that of the common block, are,

are, its being formed out of four pieces of elm which are bolted and riveted together, which is much stronger and more durable than morticing out of a solid block of elm.

The pin and cog being riveted with a counter plate in the sheave, gives it a considerable advantage as to the leverage, together with the pin turning, consequently wearing round, which adds to the durability, together with the sheave's working on the ends of the pin, which causes it to run with equal velocity on the sides as perpendicular, nor is the cog so likely to wear loose when the pin is fixed in the cog as otherwise in the common block which wears the upper part of the pin only, and which causes a very great friction; the strength and durability over the common block are in the proportion as 2 to 1.

The above blocks are manufactured by the proprietor, Mr. Sibley, Old Gravel-lane, London Docks.

LIST OF PATENTS FOR NEW INVENTIONS.

To Marc Isambard Brunel, of Chelsea, Middlesex, engineer, for certain improvements in making stereotype plates.—25th Jan. 1820.

To Phillips London the younger, of Cannon-street, London, for his method of destroying or decomposing the offensive vapour arising from animal or vegetable matter when heated.—25th Jan.

To Daniel Treadwell, of the United States of America, but now of Newman's-court, Cornhill, London, for certain improvements in the construction of printing-presses.—25th Jan.

To John Moody, of Margate, Kent, for an inkstand containing carbonaceous and extractive matter in a dry state, which, with the addition of water only, will supply ink.—25th Jan.

To George Shoobridge, of Houndsditch, London, and William Shoobridge, of Marden, Kent, for a substitute for flax or hemp, and for manufacturing the same into articles for which flax or hemp are used.—5th Feb.

To James Huggett, of Hailsham, Sussex, for a machine to be attached to carriages as a substitute for a drag, to regulate the speed, and to prevent accidents in going down-hill, or in other perilous situations.—10th Feb.

HISTORY OF THE WEATHER.

We comply with the request of a meteorological correspondent in giving a place to the following notices of the weather at different periods since the Christian era; agreeing perfectly with him that such data are of essential consequence to the formation of any general conclusions on this hitherto very unsettled branch of inquiry.

"It is very difficult to ascertain the precise coudition of the weather in in distant ages. The thermometer was not invented till 1590 by the celebrated Sanctorio; nor was that valuable instrument reduced to a correct standard before the year 1724, by the skill of Fahrenheit. We have hence no observations of temperature which go further back than a century. Prior to this period we must glean our information from the loose and scanty notices which are scattered through the old chronicles relative to the state of the harvest, the quality of the vintage, or the endurance of frost and snow in the winter. Great allowance, however, should be made for the spirit of exaggeration and the love of the marvellous, which infect all those rude historical monuments. Toaldo and Pilgram have, with incredible industry, prosecuted this research; and, from a bulky work of the latter, printed in the German language at Vienna in 1788, we shall select the most remarkable pastages concerning the state of the weather for more than a thousand years back, and combine with them the observations made by Professor Pfaff, of Kiel. The following years are noted for the severity of the winter:

In A.D. 401, the Black Sea was entirely frozen over.

In 462, the Danube was frozen, so that Theodomer marched over the ice to avenge his brother's death in Swabia.

In 545, the cold was so intense in winter, that the birds allowed themselves to be caught by the hand.

In 763, not only the Black Sea, but the Straits of the Dardanelles was frozen over. The snow in some places rose fifty feet high; and the ice was so heaped in the cities as to push down the walls.

In 800, the winter was intensely cold.

In 822, the great rivers of Europe, such as the Danube, the Elbe, and the Seine, were so hard frozen as to bear heavy waggons for a month.

In 860, the Adriatic was frozen.

In 874, the winter was very long and severe. The snow continued to fall from the beginning of November to the end of March, and incumbered the ground so much, that the forests were inaccessible for the supply of fuel.

In 991, and again in 893, the vines were killed by the frost, and the cattle perished in their stalls.

In 991, the winter lasted very long, with extreme severity. Every thing was frozen, the crops totally failed, and famine and pestilence closed the year.

In 1044, great quantities of snow lay on the ground. The vines and fruit-trees were destroyed, and famine ensued.

In 1067, the cold was so intense, that most of the travellers in Germany were frozen to death on the roads.

In

In 1124, the winter was uncommonly severe, and the snow lay very long.

In 1133, it was extremely cold in Italy; the Po was frozen from Cremona to the sea: the heaps of snow rendered the roads impassable, the wine-casks were burst, and even the trees split, by the action of the frost, with immense noise.

In 1179, the snow was eight feet deep in Austria, and lay till Easter. The crops and vintage failed, and a great murrain consumed the cattle.

The winters of 1209 and 1210 were both of them very severe, insomuch that the cattle died for want of fodder.

In 1216, the Po frozen fifteen ells deep, and wine burst the casks.

In 1234, the Po was again frozen, and loaded waggons crossed the Adriatic to Venice. A pine forest was killed by the frost at Ravenna.

In 1236, the Danube was frozen to the bottom, and remained long in that state.

In 1261, the frost was most intense in Scotland, and the ground bound up. The Cattegat was frozen between Norway and Jutland.

In 1281, such quantities of snow fell in Austria as to bury the very houses.

In 1292, the Rhine was frozen over at Breysach, and bore loaded waggons. One sheet of ice extended between Norway and Jutland, so that travellers passed with ease; and, in Germany, 600 peasants were employed to clear away the snow, for the advance of the Austrian army.

In 1305, the rivers in Germany were frozen; and much distress was occasioned by the scarcity of provisions and forage.

In 1316, the crops wholly failed in Germany. Wheat, which some years before sold in England at 6*s.* a quarter, now rose to 2*l.*

In 1323, the winter was so severe, that both horse and foot passengers travelled over the ice from Denmark to Lubec and Dantzic.

In 1339, the crops failed in Scotland; and such a famine ensued, that the poorer sort of people were reduced to feed on grass, and many of them perished miserably in the fields. Yet, in England, wheat was at this time sold so low as 3*s.* 4*d.* a quarter.

In 1344, it was clear frost from November to March, and all the rivers in Italy were frozen over.

In 1392, the vineyards and orchards were destroyed by frost, and the trees torn to pieces.

The year 1408 had one of the coldest winters ever remembered. Not only the Danube was frozen over, but the sea be-

tween

tween Gothland and Oeland, and between Norway and Denmark, so that wolves driven from their forests came over the ice into Jutland. In France the vineyards and orchards were destroyed.

In 1423, both the North Sea and the Baltic were frozen. Travellers passed on foot from Lubeck to Dantzig. In France the frost penetrated into the very cellars. Corn and wine failed, and men and cattle perished for want of food.

The successive winters of 1432, 1433, and 1434, were uncommonly severe. It snowed forty days without interruption. All the rivers of Germany were frozen: and the very birds took shelter in the towns. The price of wheat rose, in England, to 27*s.* a quarter, but was reduced to 5*s.* in the following year.

In 1460 the Baltic was frozen, and both horse and foot passengers crossed over the ice from Denmark to Sweden. The Danube likewise continued frozen two months; and the vineyards in Germany were destroyed.

In 1468, the winter was so severe in Flanders, that the wine distributed to the soldiers was cut in pieces with hatchets.

In 1544, the same thing happened again, the wine being frozen into solid lumps.

In 1548, the winter was very cold and protracted. Between Denmark and Rostock, sledges drawn by horses or oxen travelled over the ice.

In 1564, and again in 1565, the winter was extremely severe over all Europe. The Scheldt froze so hard as to support loaded weggons for three months.

In 1571, the winter was severe and protracted. All the rivers in France were covered with hard and solid ice; and fruit trees, even in Languedoc, were killed by the frost.

In 1594, the weather was so severe, that the Rhine and the Scheldt were frozen, and even the sea at Venice.

The year 1608 was uncommonly cold, and snow lay of immense depth even at Padua. Wheat rose, in the Windsor market, from 36*s.* to 56*s.* a quarter.

In 1621 and 1622, all the rivers of Europe were frozen, and even the Zuyder Zee. A sheet of ice covered the Hellespont; and the Venetian fleet was choaked up in the lagoons of the Adriatic.

In 1655, the winter was very severe, especially in Sweden. The excessive quantities of snow and rain which fell did great injury in Scotland.

The winters of 1658, 1659, and 1660, were intensely cold.—The rivers in Italy bore heavy carriages; and so much snow had not fallen at Rome for several centuries. It was in 1658 that Charles X. of Sweden crossed the Little Belt over the ice, from Holstein

Holstein to Denmark, with his whole army, foot and horse, followed by the train of baggage and artillery. During these years the price of grain was nearly doubled in England: a circumstance which contributed, among other causes, to the Restoration.

In 1670, the frost was most intense in England and in Denmark, both the Little and Great Belt being frozen.

In 1684, the winter was excessively cold. Many forest trees, and even the oaks in England, were split by the frost. Most of the hollies were killed. Coaches drove along the Thames, which was covered with ice eleven inches thick. Almost all the birds perished.

In 1691, the cold was so excessive, that the famished wolves entered Vienna, and attacked the cattle, and even men.

The winter of 1695 was extremely severe and protracted. The frost in Germany began in October, and continued till April, and many people were frozen to death.

The years 1697 and 1699 were nearly as bad. In England, the price of wheat, which in preceding years had seldom reached to 30*s.* a quarter, now amounted to 71*s.*

In 1709, occurred that famous winter, called, by distinction, the cold winter. All the rivers and lakes were frozen, and even the seas, to the distance of several miles from the shore. The frost is said to have penetrated three yards into the ground.—Birds and wild beasts were strewed dead in the fields; and men perished by thousands in their houses. The more tender shrubs and vegetables in England were killed; and wheat rose in price from 2*l.* to 4*l.* a quarter. In the south of France, the olive plantations were almost entirely destroyed: nor have they yet recovered that fatal dissaster. The Adriatic sea was quite frozen over, and even the coasts of the Mediterranean, about Genoa; and the citron and orange groves suffered extremely in the finest parts of Italy.

In 1716, the winter was very cold. On the Thames booths were erected and fairs held.

In 1726, the winter was so intense, that people travelled in sledges across the Strait, from Copenhagen to the province Scania in Sweden.

In 1729, much injury was done by the frost, which lasted from October till May. In Scotland, multitudes of cattle and sheep were buried in the snow; and many of the forest-trees in other parts of Europe were killed.

The successive winters of 1731 and 1732 were likewise extremely cold.

The cold of 1740 was scarcely inferior to that of 1709. The snow lay eight or ten feet deep in Spain and Portugal. The Zuyder

der Zee was frozen over, and many thousand persons walked or skaited on it. At Leyden, the thermometer fell ten degrees below the zero of Fahrenheit's scale. All the lakes in England froze; and a whole ox was roasted on the Thames. Many trees were killed by the frost; and postillions were benumbed in their saddles. In both the years 1709 and 1740, the General Assembly of the Church of Scotland ordained a national fast to be held, on account of the dearth which then prevailed.

In 1744, the winter was again very cold. The Mayne was covered seven weeks with ice; and at Evora in Portugal people could hardly creep out of their houses for heaps of snow.

The winters during the five successive years, 1745, 1746, 1747, 1748, and 1749, were all of them very cold.

In 1754, and again in 1755, the winters were particularly cold. At Paris, Fahrenheit's thermometer sunk to the beginning of the scale; and in England, the strongest ale, exposed to the air in a glass, was covered, in less than a quarter of an hour, with ice an eighth of an inch thick.

The winters of 1766, 1767, 1768, were very cold all over Europe. In France the thermometer fell six degrees below the zero of Fahrenheit's scale. The large rivers and the most copious springs in many parts were frozen. The thermometer laid on the surface of the snow at Glasgow fell two degrees below zero.

In 1771, the snow lay very deep, and the Elbe was frozen to the bottom.

In 1776, much snow fell, and the cold was intense. The Danube bore ice five feet thick below Vienna. Wine froze in the cellars, both in France and in Holland. Many people were frost-bitten, and vast multitudes both of the feathered and finny tribes perished. Yet the quantity of snow which lay on the ground had checked the penetration of the frost. Van Swinden found in Holland, that the earth was congealed to the depth of twenty-one inches, on a spot of a garden which had been kept cleared, but only nine inches at another place near it, which was covered with four inches of snow.

The successive winters of 1784 and 1785 were uncommonly severe, insomuch that she Little Belt was frozen over.

In 1789, the cold was excessive, and again in 1795, when the republican armies overran Holland.

The successive winters of 1799 and 1800 were both very cold.

In 1809, and again in 1812, the winters were remarkably cold.

The years which were extremely hot and dry, will be more easily enumerated.

In

In 763, the summer was so hot that the springs dried up.

In 860, the heat was so intense that, near Worms, the reapers dropt dead in the fields.

In 993, and again in 994, it was so hot that the corn and fruit were burnt up.

The year 1000 was so hot dry, that in Germany the pools of water disappeared, and the fish, being left to stink in the mud, bred a pestilence.

In 1022, the heat was so excessive, that both men and cattle were struck dead.

In 1130, the earth yawned with drought. Springs and rivers disappeared, and even the Rhine was dried up in Alsace.

In 1159, not a drop of rain fell in Italy after the month of May.

The year 1171 was extremely hot in Germany.

In 1232, the heat was so great, especially in Germany, that it is said that eggs were roasted in the sands.

In 1260, many of the Hungarian soldiers died of excessive heat at the famous battle fought near Bela.

The consecutive years 1276 and 1277 were so hot and dry as to occasion a great scarcity of fodder.

The years 1293 and 1294 were extremely hot, and so were likewise 1303 and 1304, both the Rhine and Danube having dried up.

In 1333, the corn-fields and vineyards were burnt up.

The years 1393 and 1394 were excessively hot and dry.

In 1447, the summer was extremely hot.

In the successive years 1473 and 1474, the whole earth seemed on fire. In Hungary one could wade across the Danube.

The four consecutive years, 1538, 1539, 1540, and 1541, were excessively hot, and the rivers dried up.

In 1556, the drought was so great that the springs failed. In England, wheat rose from 8*s.* so 53*s.* a quarter.

The years 1615 and 1616 were very dry over Europe.

In 1646, it was excessively hot.

In 1652, the warmth was very great, the summer being the driest ever known in Scotland; yet a total eclipse of the sun had happened that year, on Monday the 24th of March, which hence received the appellation of Mirk Monday.

The summer of 1660 was remarkably hot. It is related that one of the minions of tyranny, who, in that calamitous period harassed the poor Presbyterians in Scotland with captious questions, having asked a shepherd in Fife, whether the killing of the notorious Sharp, Archbishop of St. Andrew's, (which had happened

pened in May,) was murder; he replied, that he could not tell, but there had been fine weather ever since.

The first year of the 18th century was very warm, and the two following years were of the same description.

It is a singular coincidence, that in 1718, the distance precisely of 100 years from the present, the weather was extremely hot and dry all over Europe. The air felt so oppressive, that all the theatres were shut in Paris. Scarcely any rain fell for the space of nine months, and the springs and rivers were dried up. The following year was equally hot. The thermometer at Paris rose to 98 degrees by Fahrenheit's scale. The grass and corn were quite parched. In some places the fruit-trees blossomed two or three times.

Both the years 1723 and 1724 were dry and hot.

The year 1745 was remarkably warm and dry, but the following year was still hotter; insomuch that the grass withered, and the leaves dropt from the trees. Neither rain nor dew fell for several months; and on the Continent, prayers were offered up in the churches to implore the bounty of refreshing showers.

In 1748, the summer was again very warm.

In 1754, it was likewise extremely warm.

The years 1760 and 1761 were both of them remarkably hot; and so was the year 1763.

In 1774, it was excessively hot and dry.

Both the years 1778 and 1779 were warm and very dry.

The year 1788 was also very hot and dry; and of the same character was 1811, famous for its excellent vintage, and distinguished by the appearance of a brilliant comet.

MEAN TEMPERATURE OF THE EARTH.

According to Laplace, any actual diminution of the mean temperature of the earth would be detected by a diminution of the length of the day. It appears by computation, that one degree of Fahrenheit's thermometer would make an alteration of nearly one second in the length of a day, and four or five minutes in that of a year.

METEORO-

METEOROLOGICAL TABLE

Extracted from the Register kept at Kinfauns Castle, N. Britain. Lat. 56° 23′ 30″.—Above the level of the Sea 129 feet.

1819.	Morning, 8 o'clock. *Mean height of*		Evening, 10 o'clock. *Mean height of*		Mean Tempr. by Six's	Depth of Rain.	N° of Days.	
	Barom.	Ther.	Barom.	Ther.	Ther.	Inch. 100	Rain or Snow.	Fair.
January.	29·476	37·290	29·472	37·839	38·839	4·20	18	13
February.	29·418	36·071	29·442	36·571	37·785	2·45	14	14
March.	29.720	41·226	29·737	41·161	42·580	1.10	9	22
April.	29.722	44·500	29·704	43·530	45·666	4·50	12	18
May.	29·855	50·350	29·852	47·420	51·129	1·15	12	19
June.	29·728	54·466	29·7[illegible]3	5[illegible]·400	55·433	2·35	17	13
July.	29·895	58·613	29·896	56.549	60·064	1.20	7	24
August.	29·852	60·419	29·886	59·387	63·129	1·40	6	25
September.	29·766	52·466	29·740	5[illegible]·100	53·866	1·70	10	20
October.	29·728	44·806	29·754	44·419	46·677	4·20	14	17
November.	29·640	35·800	29·670	35·700	36·700	1·80	11	19
December.	29·613	32·774	29·608	32.130	33·451	2·55	13	18
Average of the year.	29·701	45·690	29·706	44·850	47·109	28·60	143	222

ANNUAL RESULTS.

MORNING.

Barometer.			*Thermometer.*	
Observations.	*Wind.*		*Wind.*	
Highest, 21st Sept.	NW.	30·45	24th July, W.	67°
Lowest, 31st Aug.	E.	28·76	11th Dec. NW.	18°

EVENING.

Highest, 20th Sept.	NW.	30·40	17th Aug. E.	69°
Lowest, 17th Jan.	NW.	28·79	10th Dec. NW. . . .	17°

Weather.	*Days.*	*Wind.*	*Times.*
Fair	217	N. and NE.	28
Rain or Snow	148	E. and SE.	109
	——	S. and SW.	60
	365	W. and NW.	168
			——
			365

Extreme Cold and Heat, by Six's Thermometer.

Coldest, 11th December . .	Wind NW.	11°
Hottest, 17th August . . .	Wind E.	80°
Mean Temperature for 1819		46° 109′

RESULT OF THREE RAIN GAUGES.	In. 100
No. 1. On a conical detached hill above the level of the Sea 600 feet	22·36
— 2. Centre of the Garden, 20 feet	28 60
— 3. Kinfauns Castle, 129 feet	30·20
Mean of the 3 Gauges	27·05

METEORO-

METEOROLOGICAL TABLE, By Mr. CARY, of the Strand.

[I am happy to be enabled to resume the Register of the Weather, so long supplied by my friend Mr. Cary. The Table subjoined completes the Diary from the 26th of Dec. to the present date (22d Feb.), with the exception of those days on which the unfortunate fire at Mr. C.'s premises, and its immediate consequences, prevented Mr. C. from making any observation.]

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.
Dec. 27	31	32	30	29·52	10	Fair
28	33	33	28	·53	5	Cloudy
29	28	31	24	·67	12	Fair
30	23	30	33	·50	8	Cloudy
31	27	32	24	·40	13	Fair
Jan. 1	20	26	26	·55	10	Fair
2	28	37	37	·58	8	Cloudy
3	31	32	28	.80	9	Fair
4	25	29	24	30·10	10	Fair
5	18	25	28	·22	12	Fair
6	25	35	37	·16	10	Cloudy
7	33	31	25	·45	9	Fair
8	25	27	24	·64	10	Fair
9	24	27	26	·80	18	Fair
10	22	26	28	·47	12	Cloudy
11	26	31	32	29·81	0	Snow
12	26	25	22	30·16	5	Cloudy
13	18	26	22	·19	0	Snow
14	25	25	23	·22	6	Cloudy
15	11	19	24	29·81	7	Fair
16	23	29	24	·77	7	Fair
17	29	35	28	·69	6	Cloudy
Feb. 6	44	48	46	29·99	0	Small Rain
7	46	50	44	30·20	0	Cloudy
8	44	47	44	·19	0	Cloudy
9	40	48	44	29·99	0	Fair
10	44	47	38	30·05	0	Fair
11	37	46	42	·18	0	Fair
12	42	47	41	·03	0	Rain
13	41	46	37	·07	0	Cloudy
14	35	45	36	·35	0	Fair
15	32	37	32	·38	0	Fair
16	26	34	31	·32	0	Fair
17	25	34	29	·19	0	Fair
18	24	32	31	·15	0	Cloudy
19	30	35	32	·17	0	Cloudy
20	31	32	31	29·91	0	Snow
21	30	35	37	·93	0	Rain
22		45	46	·85	0	Rain

XXI. *Hints respecting Naval Architecture.* By A Correspondent.

To Mr. Tilloch.

Chatham, March 1, 1820.

Sir,—It would doubtless be a waste of time to say any thing on the importance to this country, of naval force; consequently every attempt (however limited) for the improvement of ships of war, whether in their construction or œconomy, should at least receive a candid examination.

It is well known that all line-of-battle ships, I mean those of 74 guns, carry a large quantity of iron as ballast—small class 160, large class 185 tons. Now, could not the greater portion of that material be so disposed of as to add to the strength as well as stability of the ship? For example: instead of the usual method of supporting the beams in the middle by perpendicular pieces of timber from the keelson to the orlop-deck beams, would it not be much better to make use of cast-iron? I mean the present ballast re-cast into the requisite forms. The principal pieces should be of a parabolic shape, when seen in the longitudinal section of the ship, with the terminations somewhat enlarged and circular; at each end there should be a round hole, through which a strong pin might pass, for the purpose of connecting their adjoining ends; the same pin should also pass through holes in a plate having projections on it for that purpose, placed at a proper distance to receive the ends of the parabolic pieces; those plates to be let into, and strongly bolted to, the keelson, and to a strong fore and aft piece under the orlop deck beams; to the ends of this piece the stem and stern post should be securely united, the parabolic pieces to be placed at the angle of 45°, intersecting each other two or three times. If judged necessary, the whole might form a reticular frame of the length and depth of the hold, equally calculated to resist tension or compression. The lower deck beams, that they may be capable of resistance in opposite directions, should be of the form proposed for the oblique pieces, if of metal: should timber be preferred, the shape should be the same, as near as is compatible with the nature of that material. Of whatever material they are made, they should, if possible, be sufficiently strong to support the superincumbent weight of the ship, &c. I mean without encroaching on the stowage. As it is intended that the longitudinal frame should be continued without interruption from stem to stern, the hatchways must be increased in width. The masts may be made either to step on, or double at their lower ends so as to pass down by the sides of the frame. I cannot suppose that any objection can be made to using metal in the above manner in a ship's hold. Certainly no change of climate can produce a variation in the temperature of that part, sufficient to effect any material change in the dimensions of the pieces. The method at present is to make deck beams convex on the upper and concave on the under side. It appears from this form of beam, to be the intention of the constructor to destroy rather than to preserve the ship in its original figure.

The deck and beams of this form may be considered as an arch of very large radius, incessantly endeavouring to thrust the sides of the ship to a greater distance, the destructive effects being greatly increased when the ship is but partially supported in a heavy sea; or, what is perhaps nearly as bad, strained by the percussion of the guns in action. It may be said, and no doubt it is true, that most of the beams have perpendicular supports in the middle: but this only converts the beam into two arches; the evil may be diminished, it is certainly not removed.

The way in which the beams are connected to the sides appears to me to be equally exceptionable. What but the most inveterate prejudice could retain the fastenings called knees? The present method of building ships is to place the timbers as near to each other as possible. Would it not be more eligible to increase the depth of every other timber or frame by the addition of the intermediate one? Double the number of timbers, as they are at present disposed, gives double strength; by doubling the depth the strength would be quadruple (or as the square of the depth): those double timbers should be carried as high as the lower gun deck, their termination serving as a support for the beams of the same. This no doubt would diminish the capacity of the hold; but let the external width be increased in the proportion of the augmentation made to the thickness of the timbers: greater breadth is said to be much wanted in most of our ships.

If you will favour this with a place in your very valuable miscellany, it may fall into the hands of some of your readers much more capable of appretiating what has been done, or pointing out what should be done, than

Your most obedient and very humble servant,

I. K. K.

XXII. *Description of an Instrument for gathering Fruit. By Mr.* Thomas Lane, *of Stockwell*.*

Chapel-street, Stockwell, Nov. 3, 1818.

Sir, — I beg leave to lay before the Society for the Encouragement of Arts, Manufactures, and Commerce, a machine for gathering

* From the *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* for 1819. The sum of Ten Guineas was voted to Mr. Lane for this invention, and a specimen of the instrument is preserved in the Society's Repository.

ing fruit either from wall-trees or standards, but more particularly the latter. There are few trees of any magnitude, especially apple- and pear-trees, of which all the fruit can be gathered by hand, even with the assistance of a ladder, and in the attempt many of the best bearing sprays are necessarily injured or torn off. The finest fruit also is that which grows at the ends of the highest branches, where it has the full advantage of the sun and air. But fruit in this situation is precisely that which is the most difficultly got at in the ordinary way of gathering, and hence much of the prime produce of a tree is either shaken or beaten to the ground, in consequence of which it receives so many bruises as to be unfit for market, and incapable of being kept. The instrument which I have the honour of laying before the Society is calculated to gather fruit without bruising it, and without injuring the branches in any degree. I have found it on trial to answer my expectation, and the subjoined certificate from Mr. Phillipps junior confirms the result of my own experience.

I am, sir, &c.

A. Aikin, Esq. Secretary, &c. THOS. LANE.

Certificate.

Exhibition of Fruit Trees, &c.
Portsmouth Road, near Vauxhall, Nov. 19, 1818.

Sir,—Mr. Lane left one of his fruit-gatherers at my establishment the beginning of last autumn, which I have used ever since, finding it very convenient for gathering apples, pears, &c.; have shown it to many persons who have approved of it.

I am, sir, &c.

L. PHILLIPPS junior.

Reference to the Engraving of Mr. LANE'S *Fruit-Gatherer.*
Plate III. figs. 1, 2, 3, 4, 5.

a, a pole of white deal, or any other light stiff wood, ten or eleven feet long.

b, a bent lever, having the long arm turned up into a hook, and the short arm pierced with an eye at the extremity.

c, an iron wire one-eighth of an inch in diameter, secured in a longitudinal groove in the pole, as represented in the section fig. 4, and fastened at one end to the lever *b*, and at the other end to the lever *d*.

d, a bent lever, the long arm of which is terminated by a ring, fig. 3, over which a piece of leather is stitched, fig. 1.

e, a ring fixed to the pole by means of a stem, and placed directly opposite the lever *d*.

The ordinary state of the apparatus is that represented by the shaded parts of figs. 1 and 2. When it is to be made use of, the

 under

under side of the pole is to be grasped by the left hand, while the right hand is slipped within the hook *b*. The end of the pole being brought near the fruit, the right hand is to be raised so as to bring the hook *b* into the position represented by the dotted line; the short end of the lever will of course at the same time press the wire *c* forwards. The impulse of the wire throws the lever *d* into the position represented by the dotted line, and the interval between the two rings becomes such as readily to admit an apple or pear of the largest size. The hook *b* being now again brought nearer to the pole, the ringed extremity of *d* grasps the fruit gently but firmly, and a slight pull detaches it from the branch.

XXIII. *New Method of cultivating in Great Britain the* Papaver somniferum, *and of preparing Opium from it. By* John Young, *Esq. Fellow of the Royal College of Surgeons, Edinburgh* *.

Edinburgh, April 22, 1818.

Dear Sir,—The preparing of opium from poppies grown in Britain having engaged the attention of the Society for the Encouragement of Arts, Manufactures, and Commerce, I request you will do me the favour to present to the Society the inclosed account of a new method of collecting opium in this country, and a proposal for improving the present mode of gathering it in the East Indies. The box contains a specimen of the opium, the instruments used for collecting it, and one of the capsules from which I gathered it. I have affixed to the account two certificates respecting the efficacy of the opium.

I am, sir, &c.

A. Aikin, Esq. Secretary, &c. John Young.

The natural history of opium and the manner of collecting and preparing it in the East Indies and in Persia have been fully detailed by Dr. Samuel Crump, in his Inquiry into the Nature and Properties of Opium. He examined the different accounts related by authors, from Dioscorides, Pliny, Kæmpfer, and many others, till the year 1792, when his very interesting work was completed.

The preparation of opium in Britain has long been a desideratum. Premiums have been offered by the Society of Arts, and more recently by the Caledonian Horticultural Society. Speci-

* From the *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* for 1819. The Gold Isis medal of the Society was conferred on Mr. Young for these communications, and specimens of the instruments are preserved in the Society's Repository.

mens

mens of British opium have been produced, and proved to be in no degree inferior to the best foreign opium; but it has not yet been ascertained that this valuable drug can be cultivated in Britain with profit to the grower.

The few experiments which have been made, were conducted according to the Eastern mode. But the temperature, winds, and rain of this climate have hitherto been justly considered as insuperable obstacles. Of these the temperature may be held as the least objectionable, for the large white poppy (*Papaver somniferum* of Linnæus), from which foreign opium is obtained, comes to maturity in this climate. But it is further objected, that the high winds beat down the plants, and the rains wash off the opium, before it can be collected, when the Eastern mode of gathering it is practised. It has therefore been proposed to cultivate the garden poppy of this country, because it is not so liable to be damaged by wind as the large white poppy.

It is the object of this communication to describe a method by which these obstacles have been completely removed, and to demonstrate from the result of experiment, that opium, superior in quality to the best Turkey opium, can be procured in Britain, in sufficient quantity not only for home consumption, but also for exportation. It is proposed to cultivate the poppy not only for its opium, but also for its oil; and it will appear that a crop of early potatoes may be raised upon the same space of ground with the opium and oil by the same culture, and that such a crop will, in a good season, yield a clear profit of from 50*l.* to 80*l.* per acre, allowing 60*l.* for expenses.

The monopoly of the opium produced from the culture of the poppy, is the third principal branch of the East India Company's *territorial revenue in India**.

In 1773, the contract or exclusive privilege for providing opium was granted to Meer Munkeer, in preference (as was stated by Government) to any one else, because, being the person employed by the gentlemen of Patna in that business, he was the best acquainted with the proper mode of managing it, and would account for any outstanding balances. He was to deliver the Bahar opium at 320 rupees, and the Oude at 350 rupees per maund.

Since that time, the East India Company's annual revenue upon that article alone, has risen from eight to upwards of eighty lacs of rupees, or more than a million sterling. By a Report, dated East India House, 29th February 1816, which was at that time laid before parliament, the sale of opium in Bengal for the year 1813-14, amounted to 96 lacs 40,729 current rupees, the advances and charges upon which only amounted to 10 lacs 77,638 current rupees.

* Parliamentary Reports, vol. vii. p. 23.

But the opium used in Britain is principally supplied from Turkey. The gross amount of duty upon opium imported into Great Britain in the year 1816, was only 2,651*l.* 13*s.*, while the average quantity consumed in Britain is 14,400 lb., which is chargeable with a duty of 8*s.* 8*d.* per pound. There are, besides, from 250 to 300 chests of opium imported from Turkey, and lodged in bond warehouses for exportation, each chest containing from 150 lb. to 200 lb. of opium. This statement is from a member of the Turkey Company in London.

As the method of gathering opium, about to be proposed, differs materially from any other hitherto in use, it may be proper to observe, that Mr. Ball, who obtained a premium of fifty guineas from the Society of Arts, collected his opium according to the Bengal method, which is accurately described by Mr. Kerr*, who was an ocular witness, and by A. W. Davis†, whose accounts agree with that given by Kæmpfer‡ respecting the mode of collecting opium in Persia. The seeds, according to Mr. Kerr, are sown in quadrangular areas, the intervals of which are formed into aqueducts for conveying water into each area. The plants are allowed to grow six or eight inches from each other, and are plentifully supplied with water till they are six or eight inches high, when a nutrient compost of dung, ashes, and nitrous earth, is laid over the areas. A little before the flowers appear, they are again well watered till the capsules are half grown, when the watering is stopped, and they begin to collect the opium. This they effect by making, at sunset, two longitudinal incisions from below upwards, without penetrating the cavity, with an instrument that has two points as fine and sharp as a lancet. The incisions are repeated every evening, until each capsule has received six or eight wounds, and they are then allowed to ripen their seeds. The juice which exudes is collected in the morning, and being inspissated to a proper consistence, by working it in an earthen pot in the sun's heat, it is formed into cakes for sale.

In this manner Mr. Ball collected four ounces of opium from one fall and twenty-eight square yards of ground, which is at the rate of 22 lb. 8 oz. per acre. But, in another place, he observes, that by a calculation which he made, supposing one poppy growing in one square foot of earth, and producing one grain of opium, more than 50 lb. will be collected from one statute acre of land. But if I take his proposition, and calculate by the rule used by land-measurers, the produce in that case would only be 5 lb. 11 oz. and 1 dr. per acre. If Mr. Ball's assertions with respect to the probable produce had been correct, there can be no doubt that opium would

* Medical Observations and Inquiries, vol. v. art. 28.

† Transactions of Society of Arts, vol. xvi. p. 273.

‡ *Amœnitates Exoticæ,* fasc. 3. obs. 15.

would have been prepared in this country to a considerable extent.

It is probable that Mr. Thomas Jones, who was a candidate for the premium offered by the Society of Arts, was misled by the speculations of Mr. Ball. Mr. Jones only collected 21 lb. 7 oz. of opium from five acres and upwards of poppies, and obtained the premium of fifty guineas for the largest specimen. He collected his opium according to the Bengal method; but some of his poppies, he says, became stunted, and others were entirely destroyed by remarkably dry weather, which continued six weeks from the beginning of May. This may be considered as the reason why he obtained so little from five acres. In another place he says, the largest quantity which his man, seven children, and himself, were able to procure in one morning from 5 to 9 o'clock, was one pound and a half. This happened when the dew was remarkably great, and succeeded one of the warmest days of the summer. And as he admits, in another place, that the opium (which appeared upon the heads in a soft ash-coloured substance), when first collected, is, from its union with the dew, much too soft to be formed into a proper consistence; making a proper allowance for the evaporation of its watery part, I conclude that he gathered only in one morning, after a warm day, in the same ratio that they gather opium in the East Indies. They have no rain in India during the season of gathering opium, and Mr. Kerr says, that there one acre of poppies yields 60 lb. of opium.

These observations, collected from Mr. Jones's paper to the Society of Arts, should be kept in view, as they may help to illustrate one of the objects of this essay, and confirm the superiority of my method of collecting opium in Britain.

Dr. Howison, who was for some time inspector of opium in Bengal, is the only other person, so far as I know, who has given an account of the result of his experiments for making opium in this country. Although he was not the first who collected the milky juice of the poppy in a fluid state, it is supposed he is the first who, in this country, has given the preference to that mode. Dr. Alston collected the milky juice in the fluid state according to Dioscorides *, and also in the Persian way described by Kæmpfer, from several varieties of the poppy. He also collected the true tear, as he calls it, by cutting off the star from several of the heads, bending them down, and suffering the milk to drop into a tea-cup; yet he says that he collected more by the Persian way than by that described by Dioscorides.

The instrument used by Dr. Howison for wounding the poppy-heads, consists of a brass ring, made to fit the middle finger of

* *De Papavere sativo et sylvestri*, lib. iv. cap. 65, p. 427.

the operator, in which is fixed a wheel set with lancets, which, when put in motion by drawing the hand along the poppy head, makes with great expedition whatever number of perforations are wanted, each giving out its distinct drop of milk, by which a great surface is afforded both for support and evaporation, and the flowing milk is prevented from running upon the ground, *the unavoidable consequence of the method formerly in use.* And for gathering the opium, he employs a tin flask, flattened at the mouth about half an inch, with which he *scrapes* off the opium. By means of these instruments, Dr. Howison obtained a cake of opium that weighed 8½ oz. and which was collected from a field of poppies measuring about five falls, which is at the rate of 17 lb. weight of opium per acre.

Dr. Howison's puncturing instrument and collecting flask may certainly be considered as a material improvement upon the Hindoo instruments, and he found that they answered his purpose to a certain extent in gathering opium from the garden poppy. But when the unevenness upon the surface of the capsules of the white poppy is considered, it will be found impossible to adapt the mouth of the flask so as to collect the whole of the juice without materially injuring the capsule, and much of the juice would still remain in the interstices of the ridges, which are for the most part found upon the capsules of the white poppy. Besides, the juice very soon acquires a ropiness, and adheres to the mouth of the flask, which must interrupt the gathering, and there is a chance of the juice being spilt by having the flask suspended to the body of the gatherer.

Dr. Howison has stated several objections to the cultivation of the large white poppy in this country, and has given the preference to the double red garden poppy and its varieties. He says that the white poppy, from its large head and very considerable height, is of all others the most liable to be hurt by winds; and unless they be cultivated in a sheltered situation, few will be found standing when the season for gathering the opium arrives. But independent of this, he says, that it never arrives at such perfection in this climate as to yield milk of proper consistence for making good opium, and that the few that do come to afford milk, continue in that state only for a day, and any attempt to bleed them a little sooner or later would be without success.

Mr. Kerr *, however, informs us, that the large white poppy grows in Britain, without care, to be a much statelier plant than it does in India with the utmost art; and Dr. Alston †, after commenting upon the controversy, whether opium is got from the white poppy or from the black, concludes that, as a medicine, it

* Edin. Med. Essays, vol. v. p. 103.

† Lond. Med. Observ. vol. v. p. 321.

it is of no consequence whether it be taken from the one or from the other. Dr. Crump also observes that the white variety is to be preferred, as affording opium in greater quantity than any of the rest, and there can be no doubt that this poppy yields the largest and most juicy heads.

Dr. Howison has stated that 200,000 lb. of opium are made annually in Bengal; and that notwithstanding all the care that is taken in collecting it, one-third of the crop is lost; but there is reason to believe that the waste is much greater than he supposes. For in whatever way the incisions are made, the milky juice instantly flows in a wasteful stream, and by running upon the ground or upon the leaves, one-third of the crop at least must be lost before the gathering commences in the morning. In this climate, he remarks, where the serenest day is often followed by a night of deluging rain, the adoption of the Bengal method would be worse than trusting our fortune to the chance of a lottery.

Although Dr. Howison was convinced that the juice of the poppy undergoes no change in its properties by exposure to the air, further than acquiring a greater consistence from the evaporation of its watery part, he states in another place, that in Bengal, where there is no rain during the opium-gathering season, the custom of allowing the milk to thicken, by remaining for some time on the capsule, is highly judicious. While, in another part of his account, he admits that that custom is the only reason why they lose one-third of their crop.

Supposing that 200,000 lb. of opium give the East India Company 100,000*l.* sterling annually, by Dr. Howison's account they lose more than 30,000*l.* But were the loss only to amount to half that sum, sufficient importance, it is to be supposed, would be attached to the means by which such a saving could be effected.

Mr. Kerr states, that there are about 600,000 lb. of opium annually exported from the Ganges, independent of what is consumed in the interior. He also states, that it is frequently mixed with cow-dung, with the extract obtained by boiling the plants, and with other additions which are kept secret. It is, indeed, frequently so much adulterated, that considerable quantities are burnt at Calcutta by order of the government.

In the summer of 1817, I cultivated a small field of poppies, containing about 20,000 plants of the *Papaver somniferum* of Linnæus, out of which I selected two beds, measursing one fall and fourteen square yards, for the purpose of ascertaining what quantity of opium it would produce. I collected the opium from that part selected for the experiment myself, while the rest of the crop was gathered by the people I employed. I collected as much of the milky juice as was equal to one drachm of solid opium

opium in the space of an hour; but as my professional avocations prevented me from regularly superintending the people at work, they did not gather so much as I expected. I ascertained, however, that they could gather at the rate of one drachm in the hour.

I had my poppies sown in three different ways. The first broad-cast upon beds three feet wide with an alley between, and thinned out to the distance of four and five inches, when the plants were about two inches high above the ground. The second on beds three feet wide, in rows, six rows to a bed, and six inches between the plants. The third on the spaces between rows of asparagus, two rows of poppies on each space, eight inches between each row, and six inches between the plants; two feet four inches between each double row of poppies being occupied by the asparagus.

The first produced only one capsule, the second two, and the third three capsules per plant.

Having ascertained that the white poppy, when cultivated upon the wide drill plan that I have adopted, not only yields more capsules, but much larger ones than when cultivated in the broad-cast way, or in close rows; it is evident there must be a great saving of labour, for it will take as much time to gather the juice from a small head, as it would do to collect three times the quantity of juice from a large head.

The plants between the asparagus rows having more room to grow, had not only more capsules, but they were much larger than those sown broad-coast, or in beds in close rows; and as early potatoes, cultivated in a piece of ground adjoining my crop, were sold for a high price before my plants began to flower, I proposed the following year to have, by this mode of culture, the same quantity of opium with a crop of early potatoes, as I obtained from an equal measurement of ground where there was nothing but poppies.

Accordingly, in 1818 I selected a piece of ground in the highest state of cultivation, well manured with horse-dung, in which I planted early potatoes, in rows four feet wide. Furrows were first drawn; in these furrows the dung was laid; then the sets were dropped on the dung, about nine inches asunder, and covered by the hoe. The potatoes were planted the first week of February; and the poppies were sown about the middle of April, on the middle space between the potatoe rows, two rows of poppies on each space, and twelve inches between the rows. When the poppy plants were about two inches above the ground, they were at first thinned out by the hoe, and afterwards by the fingers, to the distance of eight inches between the plants.

In this manner I raised a crop of early potatoes equal to thirty-six

six bolls per acre. Although the potatoes will be ready for immediate use before the gathering of opium commences, the whole crop will not be entirely ripe for lifting till after the opium is collected. The early potatoe gives out but a small stem; but where the soil is rich, some of them may spread in the areas; yet they can be easily pushed over to one side, so as to allow the opium gatherers to walk along the areas without trampling upon them.

The distance between the poppy plants being wider than last year, upon an average they produced four full grown capsules each, and some of them produced seven or eight capsules; and I gathered this season at the rate of two drachms of solid opium in one hour, while, by the same method of gathering, I could not collect more than one drachm in the same time last year.

Supposing one acre had been cultivated in the same manner as that piece of ground on which my experiment was made, the produce in that case would have been equal to 57 lb. 9 oz. 4 dr. and 48 gr. of solid opium, which is just twice as much as I collected the year before. But the season of 1818 being so much more favourable than the preceding year, will in a great measure account for the success of this experiment. Therefore the quantity of opium that may be collected depends greatly upon the season; yet the comparative view of the result of the experiment made in 1817, although the season was extremely unfavourable, is sufficient to prove that my method of extracting and gathering opium has a decided advantage over any other that has been recommended.

As my poppies were sown about the middle of April, they were ready for bleeding about the middle of July.

The instrument which I used for making the incisions consisted of two convex-edged scalpels, the blades of which were covered with sealing-wax, except about one-sixteenth part of an inch of the edge, and being wound round the handles with waxed thread, the two were fastened together with other thread twisted round them, and thus held at the distance of about half an inch between each blade, (Pl. III. figs. 6 and 7). It is obvious that the blades are covered with sealing-wax for the purpose of preventing the knife from penetrating the cavity of the capsules; and it can be easily removed and applied again, when the knives require to be sharpened: or the blades may be mounted with a metal sheath or guard for this purpose.

With this instrument I make one or more double incisions, according to the size of the head, at first longitudinally, and afterwards obliquely upwards from the stalk, (fig. 10.)

When the capsule is sufficiently scarified in the manner described, I then cut off, with a sharp scalpel, the capitellum or star,

star, with a thin slice of the external rind round it (*c c*, fig. 10); and by this last incision I obtain more juice than from a scarification upon the side of the head.

It is my method of gathering the milky juice of the poppy in the fluid state, that differs materially from any other that has been used, and it is on that account that I have been more successful than any other that has tried the experiment.

In my communication to Dr. Duncan relative to *Lactucarium* or Lettuce-opium, published in the second edition of his Observations on Pulmonary Consumption, I proposed to gather the opium by means of a sponge. But when I began to collect opium in that way, I soon found that it would not do; for although the sponge removes the juice more effectually than the flask proposed by Dr. Howison, it cannot be again entirely expressed, because the sponge decomposes or separates the component principles of the milky juice, and the resinous part adheres to the sponge, and soon clogs its pores. I therefore adopted the use of a small common hair-brush used by painters, and known to the trade by the name of sashtool (Pl. III. fig. 8.), which answers the purpose most completely, and with which I gathered the milky juice, even though some of the plants were laid by wind and rain, as well as if they had been standing erect. I used a camel-hair brush, but found the same objection to it as to the sponge. The common sash-tool, rounded a little at the point, without being ground, is that which I prefer.

For the sake of experiment, I exposed myself one morning to a shower of rain for half an hour, while making the incisions and gathering the opium, and succeeded as well as when there was no rain, without any other inconvenience than being wet, and having an additional quantity of water with the opium.

When the brush is sufficiently charged with juice, I scrape it off upon the slip of tin *b*, fig. 9, fixed in the mouth of a tin flask, fastened to the breast of the gatherer by straps through the holes *a a*, and capable of holding more than a day's gathering.

The gatherers follow the bleeders immediately. One bleeder will occupy two gatherers, and if he be very expert at using the knife, he may keep three gatherers constantly employed. When I performed both operations myself, I held the knife between the thumb, fore and middle fingers, and the brush between the ring and little fingers of the right hand, while I held the poppy by the stalk with the left hand.

The juice is afterwards formed into cakes or balls by spontaneous evaporation in shallow earthen dishes, placed in a close room, stirring it occasionally during the evaporation of its watery part, to be afterwards kept in bladders.

The

The operation for gathering cannot be repeated with advantage oftener than three times a week, upon the same capsules, for no more juice will flow from one wound than what may be collected immediately, and a certain time must elapse before the plant forms more juice. But it is evident a number of hands may be kept constantly employed upon a large field, till the plants cease to give out juice.

One acre will keep twelve gatherers and six cutters constantly employed for thirty days. That number can only gather a third part of an acre in one day, and by the time they have gone through the crop, the capsules at that place where they began to gather, will be ready for the operation being repeated. So that when the milky juice ceases to flow, five operations as already described will have been made upon each capsule.

Supposing twelve gatherers to work ten hours in the day, and that each gathers two ounces and a half, or as much of the juice as will make that quantity of solid opium; in thirty days they will gather fifty-six pounds of opium from one acre.

One acre of poppies cultivated according to my method, will yield 1000 lb. of seed, and this quantity of seed will give by expression 375 lb. of oil.

Although the produce of such a crop has not yet been clearly ascertained upon a large scale, the following may be taken as the estimate of one acre, from what has actually been produced in my experiment.

Estimated value of the produce of one acre.	£	*s.*	*d.*
56 lb. opium, at 36*s.*	100	16	0
36 bolls early potatoes, at 24*s.*	43	4	0
250 lb. of oil, cold drawn, at 1*s.* 6*d.* ..	18	15	0
125 lb. ditto, warm, at 6*d.*	3	2	6
500 oil cakes, at 18*s.* per 100	4	10	0
	170	7	6
Expenses	60	0	0
Total of profit ..	110	7	6

Or it may be taken this way:

56 lb. opium, at 17*s.* 6*d.*	49	0	0
36 bolls of potatoes, at 24*s.*	43	4	0
250 lb. of oil, cold drawn, at 1*s.* 6*d.* ..	18	15	0
125 lb. ditto, warm, at 6*d.*	3	2	6
500 oil cakes, at 18*s.* per 100	4	10	0
	118	11	6
Expenses	60	0	0
Profit	58	11	6

Opium

Opium costs the wholesale druggist in London at this time (May 1819) 17*s.* 6*d.* per pound, which, with a duty of 8*s.* 8*d.* per pound, makes it 1*l.* 6*s.* 6*d.*, and they charge the apothecaries 36*s.* per pound, which is the present London price of the article. The London price of opium varies so much, that about twelve years ago it was as high as 84*s.* per pound, and it seldom falls so low as 24*s.*

If the crop can be got off the ground by the middle of August, it is proposed to have a second crop of potatoes or turnips, which will give, it is supposed, about 30*l.* more.

Comparative View of the Experiments of Messrs. Ball, Howison and Young, for ascertaining what Quantity of Opium might be prepared in Britain.

	lb.	oz.	dr.	gr.
Mr. Ball, from 576 square feet, or one fall and 28 square yards, obtained about	0	4	0	0
Dr. Howison from about five falls, obtained ..	0	8	4	0
In 1817, from one fall and 14 square yards, containing 1800 plants, I obtained	0	4	0	0
According to Mr. Ball's method, one fall produced two ounces and two drachms, or per acre ..	22	8	0	0
According to Dr. Howison's method, one fall produced one ounce five drachms and 36 grains, or per acre	17	0	0	0
By my method, one fall produced two ounces seven drachms two grains and $\frac{3}{5}$, or per acre	28	12	6	24
In 1818, from one fall I obtained five ounces six drachms four grains and +, or per acre ..	57	9	4	48

Certificates.

Edinburgh, Dec. 22, 1817.

Dear Sir,—During the course of last summer I repeatedly visited your plantation of poppies, for the manufacture of opium, in the neighbourhood of Edinburgh.

I admired very much the method that was followed for collecting the opium from the plants. For, by your plan, the very great inconvenience, which arises from the unsteadiness of our climate, if the Bengal method of collection, recommended by Dr. Howison in the Memoirs of the Caledonian Horticultural Society, be adopted, is completely obviated.

I have lately prescribed your opium to many different patients, both in private and hospital practice, with the best effects. I have particularly prescribed what you put into my hands to several patients in the clinical wards of the Royal Infirmary, and, as I was inclined to infer from its appearance, I have found it remarkably efficacious, in no degree inferior to the best Turkey opium.

opium. I have, therefore, no doubt in considering your plan as an improvement in the arts, which may be adopted with great advantage in Britain.

I am, sir, &c.

John Young, Esq. Surgeon, Edin. ANDREW DUNCAN, Sen. M.D.P. Physician, Edin.

Royal Infirmary, Edinburgh, April 20, 1818.

Dear Sir,—During the last year I have frequently administered the opium made by you to patients in the Royal Infirmary, and it gives me much pleasure to report to you, that I have found it produce the same effects as the best foreign preparation of the medicine, and I think that a smaller quantity is necessary than of the foreign opium.

I am, sir, &c.

John Young, Esq. Surgeon. A. GILLESPIE.

XXIV. *Memoir of the late* GASPARD MONGE. *From Historical Essay on his Services and Scientific Works, by M.* C. DUPIN, *a Pupil of* Monge, *and Member of the French Institute.*

G. MONGE was born at Beaune in 1746. His progress was such that they gave him the office of Professor of Natural Philosophy in the college at Lyons, although he had only begun to study it the year before. Returning to Beaune in the vacation, he set about the survey of that town. As he had not proper instruments for that purpose, he made some himself. He dedicated his work to the administration of his native place, and they recompensed the young author, as far as the limited finances of the place would allow. A lieutenant-colonel of the engineers, who happened to be at Beaune, obtained for Monge an appointment as draughtsman and pupil in the *Ecole d'Apparailleurs et de Conducteurs des Travaux des Fortifications* (equivalent to our Drawing School in the Tower). As he was an excellent draughtsman, his manual dexterity was alone considered. He, however, already knew his own strength, and saw with great indignation the value that was exclusively bestowed on his mechanical talents. "I was tempted," said he, "a long time afterwards, a thousand times, to tear my drawings, out of spite for the value set on them, just as if I had been good for nothing else." The director of the school ordered him to calculate a particular case of *defilement*, an operation in which the relief and ground-work of fortifications are to be combined together with the smallest possible charge, but so that the defenders may be sheltered from the shot of the assailants. Monge abandoned the method hitherto followed, and discovered the first general geometrical method that was known for this important operation.

operation.—By applying, at different times, his mathematical talents to questions of a similar nature, and always generalizing his manner of conceiving and working them, he, at last, formed a scientific work on the subject; this was his Descriptive Geometry. For more than twenty years he found it impossible to show to the corps stationed at Mezieres the application of his geometry to carpentry. He was more successful in its application to masonry; he studied with great care the methods hitherto employed, and simplifying them, he brought them to perfection by his geometry.

His scientific works caused him to be appointed Acting Professor of the Mathematics and Natural Philosophy, in the room of Nollet and Bossut; afterwards he was appointed Honorary Professor: he then turned his views toward the study of many phænomena of nature; he made numerous experiments upon electricity; he explained the phænomena which arise from capillary attraction; was the creator of an ingenious system of meteorology; he examined the composition of water, having made that great discovery without having any knowledge of the experiments which had just before been made by Lavoisier, Laplace, and Cavendish. He did not content himself with explaining to his pupils in the theatre of the school the theories of science and their application: he loved to conduct his disciples wherever the phænomena of nature, or the works of art, could render these applications apparent and interesting. He communicated his own ardour and enthusiasm to his pupils, and changed those observations and researches into desirable pleasures, which would have appeared to be a disagreeable study in the confinement of a school, and clothed only in abstract ideas.

In order to bring Monge to Paris, he was appointed in 1780 assistant to Bossut, Professor of the Hydrodynamic Course, instituted by Turgot. That he might reconcile the duty of the two places which he now held, he lived six months at Mezieres, and six months at Paris. The same year he was admitted into the Academy of Sciences; and on the death of Bezout in 1783, he was chosen to succeed that celebrated examiner of the naval service. The Marquis de Castries invited Monge several times to write another elementary course of the mathematics for the youths of the naval service, but Monge always refused to comply. "Bezout," said he, "has left a widow with no other fortune than her late husband's works, and I do not wish to take away the bread from the widow of one who has rendered important services to science and to his country." The only elementary work which Monge published was his *Traité de Statique;* and, with the exception of a few passages in which greater rigour might be desirable, the *Statique* of Monge is a model of logic, clearness, and simplicity.

At

At that period when the public distress called forth all the useful talents and courage of the superior classes to the assistance of their country menaced with invasion, Monge was created Minister of the Marine. He did every thing he could to keep those men who were distinguished for their merit or bravery in France. He even descended to entreaties to procure the continuation of Borda's services, and he had the happiness to succeed. He was one of the most active men in those scientific employments which the preservation of the state required. The construction of the new grinding-machines erected in the powder-mills at Grenoble was his, and also the drilling-machines constructed upon the barges of the Seine. He spent his days in giving instructions and superintending the workmen, and his nights in writing his treatise on the casting of artillery, a work designed for the use of directors of foundries, and for workmen.

It was in his course at the Normal school that he first gave his lectures of descriptive geometry, the secrets of which he had not been able to reveal sooner. Another establishment, which had been originally conceived before the Normal school, but which, having had less attention paid to it by the inventors, followed it in the order of execution, realized some part of the hopes which had been looked for in vain on the establishment of the first Encyclopedic school that had been opened in France. Monge brought into it his long experience at Mezieres, and joined to this new and profound views; he drew up the plan of study, marked out their succession, and proposed scientific methods of execution. Out of four hundred pupils originally placed in the Polytechnic school, fifty of the choicest were collected into a preparatory school. Monge was almost the only one that taught these pupils. He remained the whole of the day among them, giving them, in turn, lectures on geometry and analysis;—exhorting them, encouraging them, inflaming them, with that ardour, that kindness, that impetuosity of genius, which made him explain to these pupils the truths of science with an irresistible force and charm. In the evening, when these labours were finished, Monge began others of a different kind; he wrote the sketches which were to serve as a text to his next lectures, and the following day he was to be found with his pupils at the very moment of their meeting. The good nature of Monge was neither the cold calculation of the sage, nor even the effect of education; it was a simple benevolence, which arose from his happy organization. He was born to love and to admire. His admiration was excessive, like his love; in consequence of which he did not always keep within the limits that cold and unfeeling reason would have prescribed.—As he was the father of his pupils in the school, so he was in camp the father of the soldier.

In traversing Italy to collect the statues and pictures that had been ceded to France, Monge was struck with the singular contrast between the Grecian monuments of the arts, and those of the Egyptians, transported by Augustus and his successors to the shores of the Tiber. The comparative characters of the ancient monuments were the frequent subject of conversation between the conqueror of Italy and the commissary who collected for his country the most precious fruits of victory. Monge conceived the idea of extending the domain of history beyond the fabulous ages of Greece; of learning with the certainty of a geometer what were the labours of the ancient sages of the East; of discovering afresh, by the contemplation of their monuments, what had been the processes of their arts, the usages of their public life, the order and the majesty of their feasts, and of their ceremonies.

Monge, charged by the General-in-chief to carry to the Directory the treaty of Campo Formio, was a short time afterwards placed in the first rank of the literary men who composed the Commission of sciences and arts which were to accompany the expedition to Egypt. He was the first that was appointed President of the Institute of Egypt formed on the model of the French Institute. He visited the pyramids twice, he saw the obelisk and the grand ruins of Heliopolis, he studied the remains of antiquity scattered round Cairo and Alexandria. It was during a tedious march in the middle of the Desert that he discovered the cause of that wonderful phænomenon known by the name of *mirage*. At the time of the revolt of Cairo there were in the city only a few detachments of the troops. The palace of the Institute was guarded by the members themselves; and it was proposed to sally out and join the main guard; but Monge and Berthollet, considering that the palace contained the books, manuscripts, plans, and antiquities, which were the fruits of the expedition, maintained that it was the duty of the members to guard this precious deposit, and that they ought to defend that treasure at the hazard of their lives.

Monge presided in the Commission of the sciences and the arts in Egypt; he contributed by his counsels to form that wise plan, and by arranging and proportioning the various parts, endeavoured to execute it in the utmost perfection.

Monge had an admirable method of exposing the most abstract truths, and of rendering them plain by the language of action. Nevertheless it was only by combating with nature that he was able to become an excellent Professor: he spoke with difficulty, and almost stammered; the prosody of his discourse was vicious, for he lengthened some syllables falsely, and shortened others. His physiognomy, naturally calm, exhibited the appearance of meditation; but as soon as he spoke, he appeared quite another

man;

man; his eyes acquired a sudden brilliancy; his countenance became animated, and his figure seemed as if inspired.

Monge, debilitated by age, was at last the victim of an imagination which, according as the times were adverse or prosperous, carried him beyond either just fears or just hopes. His last moments were without last thoughts—without last effusions—without any adieu: he sunk in silence—without agonies—without terror—and without hopes.

XXV. *Method of making Ivory Paper for the Use of Artists. By Mr.* S. EINSLE, *of Strutton-Ground, Westminster**.

THE properties which render ivory so desirable a substance for the miniature painter and other artists are, the evenness and fineness of its grain, its allowing all water colours laid on its surface to be washed out with a soft wet brush, and the facility with which the artist may scrape off the colour from any particular part, by means of the point of a knife or other convenient instrument, and thus heighten and add brilliancy to the lights in his painting more expeditiously and efficaciously than can be done in any other way.

The objections to ivory are, its high price, the impossibility of obtaining plates exceeding very moderate dimensions, and the coarseness of grain in the larger of these; its liability, when thin, to warp by changes of the weather, and its property of turning yellow by long exposure to the light, owing to the oil which it contains.

The candidate produced before the Committee several specimens of his ivory paper about an eighth of an inch thick, and of superficial dimensions much larger than the largest ivory: the surface was hard, smooth, and perfectly even. On trial of these by some of the artists, members of the Society, it appears that colours may be washed off the ivory paper more completely than from ivory itself, and that the process may be repeated three or four times on the same surface, without rubbing up the grain of the paper. It will also, with proper care, bear to be scraped with the edge of a knife without becoming rough.

Traces made on the surface of this paper by a hard black-lead pencil are much easier effaced by means of India rubber than from common drawing paper; which circumstance, together with the extremely fine lines which its hard and even surface is capa-

* From the *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1819. The sum of 30 guineas was voted to Mr. Einsle for this communication, and specimens of the ivory paper are preserved in the Society's Repository.

 ble

ble of receiving, peculiarly adapts it for the reception of the most delicate kind of pencil drawings and outlines.

An artist of eminence in miniature painting (not a member of the Society) stated, that he has frequently used the ivory paper, and finds it to be superior to ivory itself in the whiteness of the surface, in the facility with which it receives colour, and in the greater brilliancy of the colours when laid on, owing to the superior whiteness of the ground. Colours on ivory are apt to be injured by the transudation of the animal oil, a defect which the ivory paper is free from.

Some highly respectable dealers in drawing materials stated, that they have had samples of the ivory paper in their possession for a considerable time, and that it does not appear to become yellow or discoloured by keeping.

The valuable properties of the paper appearing thus to be satisfactorily established, a day was fixed for the candidate to disclose his process, and to prepare a specimen of the paper in presence of the Committee. Accordingly, at the appointed time, Mr. Einsle attended, and exhibited before the committee his method of preparing the paper: he also gave in a written account of the process; from which, and from the information obtained during the sitting of the committee, the following account has been drawn up:

Take a quarter of a pound of clean parchment cuttings, and put them into a two-quart pan with nearly as much water as it will hold; boil the mixture gently for four or five hours, adding water from time to time to supply the place of that driven off by evaporation; then carefully strain the liquor from the dregs through a cloth, and when cold it will form a strong jelly, which may be called size (No. 1).

Return the dregs of the preceding process into the pan, fill it up with water, and again boil it as before for four or five hours: then strain off the liquor, and call it size (No. 2).

Take three sheets of drawing paper (outsides will answer the purpose perfectly well, and being much cheaper, are therefore to be preferred), wet them on both sides with a soft sponge dipped in water, and paste them together with the size (No. 2). While they are still wet, lay them on a table, and place upon them a smooth slab of writing slate, of a size somewhat smaller than the paper. Turn up the edges of the paper, and paste them on the back of the slate, and then allow the paper to dry gradually. Wet, as before, three more sheets of the same kind of paper, and paste them on the others, one at a time; cut off with a knife what projects beyond the edges of the slate, and when the whole has become perfectly dry, wrap a small flat piece of slate in coarse sand-

sand-paper, and with this rubber make the surface of the paper quite even and smooth. Then paste on an inside sheet, which must be quite free from spots or dirt of any kind; cut off the projecting edges as before, and when dry, rub it with fine glass-paper, which will produce a perfectly smooth surface. Now take half a pint of the size (No. 1), melt it by a gentle heat, and then stir into it three table-spoonfuls of fine plaster of Paris; when the mixture is completed, pour it out on the paper, and with a soft wet sponge distribute it as evenly over the surface as possible. Then allow the surface to dry slowly, and rub it again with fine glass-paper. Lastly, take a few spoonfuls of the size (No. 1), and mix it with three-fourths its quantity of water; unite the two by a gentle heat, and when the mass has cooled, so as to be in a semi-gelatinous state, pour about one-third of it on the surface of the paper, and spread it evenly with the sponge: when this has dried, pour on another portion, and afterwards the remainder: when the whole has again become dry, rub it over lightly with fine glass-paper, and the process is completed: it may, accordingly, be cut away from the slab of slate, and is ready for use.

The quantity of ingredients above mentioned is sufficient for a piece of paper 17 and a half inches by 15 and a half.

Paris plaster gives a perfectly white surface; oxide of zinc, mixed with Paris plaster, in the proportion of four parts of the former to three of the latter, gives a tint very nearly resembling ivory; precipitated carbonate of barytes gives a tint intermediate between the two.

XXVI. *Process for giving to Linen, Silk, Cotton, &c, a fine Mineral Yellow Colour.* *By M.* Henri Braconnot*.

Excepting Prussian blue, which we have not as yet been able to impart to all cloths, the prussiate of copper and oxide of iron, which give colours rather more solid than brilliant, all the other colours used in the art of dyeing are drawn from the organic (vegetable) kingdom, because they are generally easier applied than mineral colours, although more or less alterable in the course of time. The yellows are more subject to this sort of changeableness; and if the madder colour with the help of mordants acquires in the end a certain stability, it is always at the expense of its first brilliancy.

The mineral substance which I have succeeded in fixing on cloths, and which I now recommend to dyers as the most brilliant

* From the *Ann. de Chimie*, Dec. 1819.

liant yellow colour which can be imagined, and which is not attended with the same inconveniences as the preceding, is the sulphuret of arsenic, or realgar*, which gives also to paint a very lively permanent colour, when care is taken not to associate it with certain metallic oxides, which soon tarnish its brilliancy.

It is by dissolving this sulphuret of arsenic in ammonia, that I have obtained a liquor fit for dyeing; but in order that this solution may be effected easily, it is necessary that the sulphuret should be in a certain state of division. To bring it to this state, observe the following process:

Mix together one part of sulphur, two parts of white oxide of arsenic, and five parts of the potash of commerce; melt the whole in a crucible till it is nearly red hot, and a yellow mass will be the result. Dissolve this mass in warm water; then filter the liquor to separate it from a sediment formed principally of metallic arsenic in brilliant spangles, and partly of a small quantity of a flocculent matter of a chocolate colour, which appears to be a sub-sulphuret of arsenic. Pour into the filtered liquor a quantity of water; next add some dilute sulphuric acid, which will cause a flocculent precipitate of a superb yellow colour. This precipitate washed on a cloth dissolves with extreme facility in ammonia, and yields a yellowish liquor, to which an excess of ammonia is added to render it colourless. It is in this liquor that the wool, silk, cotton, or flax to be dyed is plunged; and it is more or less diluted with water, according to the different shades which it is wished to obtain †. It is necessary carefully to avoid making use of metallic utensils. When the stuffs are withdrawn from this bath, they appear colourless; but insensibly they assume a yellow colour as the ammonia evaporates. They are exposed to the open air in such a manner that the whole of their

* I know that generally arsenical preparations inspire a degree of horror; but though the sulphuret of native arsenic, which is often mixed with the oxide of arsenic, may not be exempt from danger, the sulphuret of artificial arsenic obtained by precipitation and well washed, does not appear to me to have any pernicious influence on the animal œconomy. At least I have administered it in considerable doses to dogs or cats without their appearing incommoded by it.

† It appears that when the sulphuret of arsenic is dissolved in the ammonia a small quantity of the arsenic oxidizes; for if into the liquor there is poured an excess of lime water, a white precipitate of arseniate of lime is produced. This oxidation seems even to keep going on; and when a solution sufficiently concentrated of sulphuret of arsenic in ammonia is left to stand for some time, small crystals of arseniate of ammonia are deposited: if an acid is added, it produces a precipitate of sulphuret of arsenic mixed with sulphuret of the oxide of arsenic of a palish yellow. It may be proper therefore not to dissolve the sulphuret of arsenic in ammonia, except in proportion as it may be required.

surface

surface shall be equally affected, and when they have taken the colour till nothing more is to be gained in point of intensity, they are washed and dried.

When wool is to be dyed, it should be stamped in the ammoniacal bath, and remain there until it has been all equally impregnated; it may afterwards be wrung very slightly and uniformly; or it may be left to dry of itself. Silk, cotton, hemp and flax require only to be plunged in the dyeing liquor; they take the colour very easily. It is necessary, however, to wring them.

The sulphuret of arsenic can give to stuffs all imaginable shades from the finest golden yellow to a marigold yellow. This beautiful colour has the precious advantage of lasting for an indefinite time in all its brilliancy; nay, of lasting longer than the cloths themselves. It resists in fact all agents, if we except alkalies; but this inconvenience is well compensated by the other advantages which this colour presents. It may be of great advantage in the fabrication of rich tapestries, velvets, and other household stuffs which do not require washing in soap, and in which fixity of colour is one of the most precious qualities.

I think that the moderateness of the price of this dye, and the extreme simplicity of its application, will induce dyers to make use of it, and that it will thereby become an interesting acquisition to the art of dyeing. The ammoniacal solution of the sulphuret of arsenic may also be employed in the fabrication of coloured papers.

Nancy, Dec. 31, 1819.

XXVII. *An Essay on a Property in Light which hitherto has been unobserved by Philosophers.* By *Captain* FORMAN, *R. N.*

It is a principle in catoptrics, that when light is reflected from any body, the angle of reflection is always equal to the angle of incidence; and this principle is proved to be true, because it always corresponds with facts. A person, for instance, standing before a looking-glass, in the line of a reflecting angle, sees the image of the flame of a candle (or any other object when there is light) expressed upon its surface; but he can only see it when he is looking in the direction of that line, because the rays are reflected from every other part of the glass in angles that do not come near his eye.

Now, if there was no other light derived from a luminous body, besides these *primary* rays (which is all that philosophers have supposed), the greatest part of objects would be invisible, or at least, only so much of them would be visible as reflected the rays into the eye by their proper angles; we should see no more of the glass than that part of it upon which the image of the candle was expressed, the moon would be a speck *, the planets invisible; and, whenever we turned our backs upon the sun, as all his light would be reflected from and not into the eye, we should be enveloped in darkness even at noon day.

"There is no glass or speculum," says Sir Isaac Newton in his Treatise on Optics, "how well soever polished, but, besides the light which it refracts or reflects regularly, *scatters every way irregularly a faint light*, by means of which the polished surface, when illuminated in a dark room by a beam of the sun's light, may be easily seen in all positions of the eye."

Now, without stopping to examine whether the irregularities on the moon's and the earth's surfaces would not rather exhibit a patchwork of light and darkness than a regular diffusion of light, by means of which all objects would be equally visible, it is easy to show that there are many situations in which light is present, when its appearance cannot be explained by any such hypothesis. The only opening, for instance, in a chamber, by which light can be admitted, is a window facing the east, and the sun is setting in the west. This window, moreover, overlooks the brow of a steep hill which casts a shadow over all the objects within the visible horizon. Now it is impossible that light, under these circumstances, can be reflected into the room, because all the sun's light that would strike upon these objects, and might be reflected into the room, is intercepted by the hill; and therefore it is to some other principle that we must look for an explanation of this phænomenon.

It will probably occur to the reader, that this principle may be found in the reflection of the particles of which the atmosphere is composed; and the idea is not unreasonable, because we can only account for the blue colour of the heavens by supposing that the rays of blue colour are reflected, in this manner, into the eye

* If two lines are subtended from the moon's centre in an angle of half a degree, they would just touch the surface of the sun, for that is about the angle his diameter measures to an inhabitant of the moon. Now, by the above principle of reflection, all the direct rays of light from the sun that passed outside of those lines would be reflected outside of the sun, so that only the three hundred and sixtieth part of the moon's diameter could throw back any light into the sun; and these rays, instead of meeting in a focus, would diverge and spread over the whole of the sun's hemisphere. It follows therefore that not more than the five or six hundredth part of the moon's diameter could reflect the sun's light into a focus, at the distance this earth is from the moon; and consequently, the moon's diameter, instead of measuring an angle of fifteen or sixteen minutes, would hardly measure half a dozen seconds.

by

by the particles of air. It is evident however that they do not; because, if they reflected all colours alike, the sky would be white, and not blue; and if, as they do, they reflected the blue rays in much greater abundance than all the rest, the looking-glass, as well as the sky, would be blue, and not white; for it is impossible that the same cause could produce such different phænomena*.

I can imagine only one principle that will account for this universal diffusion of light, without opposing some other of its phænomena, or violating some of the known laws of nature. The particles of light, the moment they are emitted from a luminous body, may emit other rays of a similar but fainter character; in the same manner, these again may emit others still fainter; and so on, till the stock is exhausted and the light entirely dissipated.

That the rays of light do transmit other rays, in the manner I have supposed, is proved by a beam of light being visible, in a dark room, when it is crossing and not entering the eye. There is here no surface to reflect the light into the eye, unless the particles of air be considered as such, and then the beam ought to be blue, and not white.

I know that it has been attempted to account for this beam of light being visible, by supposing that the minute particles of dust which are light enough to be lifted by the air, and are floating in the beam, reflect this light into the eye. But, admitting that these particles do reflect light into the eye, they can only reflect light from the spaces which they themselves occupy; and therefore, unless we can suppose that the whole beam is nothing but a mass of dust, the moment they moved from one place to another, if light still came into the eye from the places they had left, it is clear that there must be light brought into the eye independent of these particles, and that can only be by the agency of the light itself. Glass reflects light only from its surfaces, and that but partially, for it suffers a great portion of light to pass through it; when it is pounded into dust it becomes opake, because its surfaces are so infinitely multiplied that not a particle of light can escape and pass through the whole; and it is whiter than it was before, because more light is consequently reflected from it†. Now, by the same rule, if there were only a few particles of dust floating in the beam, supposing that they did reflect light, they would appear like stars surrounded by darkness; which is not the case: and if they were thick enough to reflect

* If our atmosphere reflects light, we must suppose that the moon's does also; in that case, it would be the moon's atmosphere, and not herself that we behold; and what then becomes of the mountains astronomers have discovered upon her globe?

† This is the reason why snow is whiter than either water or ice.

such

such a body of light as does come into the eye, they would occupy the whole of space, and then (to say nothing of the absurdity of supposing that such a mass of dust could not be felt) they would no longer be transparent; the beam at furthest could not extend a foot beyond the hole through which it entered. Besides, if this dust is light enough to be lifted by the air, it may be blown away by a puff of wind, and the experiment is easily tried: if we can dissipate this light with a pair of bellows, it is the dust that produces it; but if we cannot, it must be owing to some other cause.

By admitting this property to be in light—and I cannot conceive how it can be disputed—we have a principle by which we can account for a great many of the phænomena of light, which cannot satisfactorily be explained in any other way. By this principle we are able to account for the following phænomena, without supposing anything but what may be easily conceived, and is extremely probable; which cannot be said in favour of any other hypothesis.

1. Why light from objects at a distance is always fainter than when it proceeds from anything that is near.

2. Why the stars are not visible in the day-time, except from a mine, or the bottom of a well.

3. The tail of a comet.

4. The zodiacal light.

5. The twilight; and, I believe,

6. The *aurora borealis*.

1. No one will dispute this proposition, that the rays of light grow fainter in proportion to the distance they travel. An apple in the moon (or anything else occupying no more space) viewed through a telescope at a focus where parallel rays meet, ought, by the laws of refraction, to be as much magnified as an apple in the hand; and if the rays coming from it were equally strong, the colours it gave out would be as fresh, and it would be equally visible. We can only account for the apple not being visible, by supposing that the rays coming from it lose a great part of their substance; and we cannot account for this loss of substance in a more philosophical way than in the manner I have supposed: if the rays lost none of their substance, distance cannot account for their faintness; if they do, the cause of it is explained.

It will perhaps be objected to this principle, that if the rays of light lost so much of their substance in travelling the short distance between the moon and our earth, before they arrived at *Saturn*, or the *Georgium Sidus*, they would be entirely extinct, and the sun would be invisible, or dimly seen to the inhabitants of those planets. In answer to this: If the moon was just twice as far from the sun as the earth is, the angle of the sun's diameter

ter to an inhabitant of the moon would only measure half as much as it does to us; and consequently, the same number of rays from the sun would only occupy half the space that they do here. What each individual ray therefore loses in substance may be made up by additional numbers: the apple in the moon, in consequence, may give out as vivid a colour as the apple in the hand; but the light from one has only two or three feet to travel, while the other is emitting rays for the space of two hundred and forty thousand miles before any part of it can enter the eye. If we suppose the fixed stars to be suns of equal magnitude with our own, every star must send down so much more light upon the earth than the sun does, in proportion as its angle (which cannot be measured) is smaller than the sun's angle: if the stars therefore did not lose a very great proportion of their light in consequence of the great distance from us, the light of the sun would be lost in the greater splendour of the stars' light, and the light of a single star entering the eye would be intolerable.

2. If the sun's primary rays did not emit secondary rays in the manner I have described, whenever we turned our eyes towards those parts of the heavens where he was not, the stars would be visible, because there could be no light enter the eye but what came from them. The reason then why we do not see the stars in the day-time is, because these secondary rays going in all directions, strike in great numbers upon the pupil of the eye, and are refracted down to the retina, which they cover with a complete coat of light; and as the light of the stars, travelling so great a distance, is fainter than this light, it cannot be distinguished from the mass; and the stars, in consequence, are invisible. When a man, however, happens to be at the bottom of a deep well, or coal-mine, a great part of these secondary rays are intercepted by the sides of the well; the coat of light on the retina is then fainter than the light of the stars; the sky, in consequence, looks dark, and the stars are visible.

If it be objected that these secondary rays, coming into the eye from every direction, would create a great deal of confusion, and prevent us from perceiving any object distinctly; I answer, that we can only account for the same object being visible to different persons at the same time, by supposing that the rays of light or colour, coming from all objects, are crossing each other in every direction; and therefore this can make no argument against my position, even if it could not be answered. But, in fact, no confusion can arise from this diffusion of light, because the primary rays reflected from any object are stronger than the secondary, and the secondary than those in the third degree: thus, for instance, if we look towards a red spot on a blue ground, both these colours will be distinctly marked upon the retina, because the direct rays from each are strong enough to drown the secondary rays from the other; and even if the ground was black, which reflects no light, instead of blue, these secondary rays from the red colour will not be perceived, because they are not sufficiently strong to overpower the other light which strikes upon the same parts of the retina. It may be necessary here to observe, that what we call darkness and black, is not in general the total absence of light, but the contrast between those bodies that reflect a great deal, and those that reflect only a small quantity of it. A man, on going out of day-light into a dungeon, fancies the darkness is absolute, while the persons residing there are capable of distinguishing one another very easily: and again, a man, on coming out of a dark dungeon, finds the glare of day-light as painful to his eyes as the direct light of the sun is to any one else. I mention this, because, otherwise it might appear to the reader a contradiction to suppose that a black spot could be represented on the retina, every part of which is covered with a coat of light.

3. Philosophers have taken a great deal of pains to find out what the tail of a comet is, and how it is produced; but not one of them, that I have ever heard of, has thought it necessary to explain in what manner this light is brought down to the eye. Whether a comet's tail be produced by electricity, or any other cause (unless it be ignited gas, like the flame of a candle, and then it would put on a very different form), it is evidently light passing across the eye, and not towards it, and can only be visible by sending down its secondary rays according to the principle I have supposed; for, otherwise, if it was refracted by any substance, either surrounding the comet or in our atmosphere, the tail to be visible must be pointed towards us; and if it was reflected, it would be seen in confusion; for it is impossible that particles of light reflected at random by particles of any fluid, could put on so regular and constant a form as the comet's tail always assumes.

In order to show why the comet's tail is always opposite the sun, it is necessary to show what it is; and to do this I must clear the way by showing that the opinions generally entertained concerning a comet's tail are erroneous.

Sir Isaac Newton supposed that the tail was somehow or other produced by the heat of the sun; that the reason that it was always turned from the sun was, because the comet passed through the sun's atmosphere; and that this ignited vapour, or whatever it is, was lifted up by the sun's atmosphere in the same manner as smoke, vapour and other light substances are by our atmosphere.

sphere. Now, it appears to me a sufficient argument to show the fallacy of this opinion, that a part of the coma or hair of the comet, which is evidently of the same nature with the tail, is going directly towards the sun in defiance to this rule; and even if we suppose this fluid, or *effluvium*, on its first issuing from the comet, to be too strong to be turned back by the force of the sun's atmosphere, we should see the extremities of this coma waving round on each side of the comet, in the same manner that water does when two streams meet, neither of which is strong enough to turn back the other. Besides, as the comet's tail is often visible when it is outside the earth's orbit, the sun's atmosphere must therefore extend so far; and there would be a constant resistance opposed to the velocity of the earth's annual and diurnal motions, which in time must overcome them. Sir Isaac Newton acknowledged this himself, with respect to the comets; and the laws of nature are general; for he supposed that the power of the sun's attraction would increase so much upon the comet in consequence of this loss of velocity, that in time it would be drawn in to the sun and serve to supply him with fresh fuel. Now as the sun was not meant to be eternal, it is a great deal more likely that he was supplied with a sufficient quantity of light and heat to last out his time, than that he should have to look for it in the comets; which by the way, as they are not drawn plump into the sun, but approach him very gradually if they do at all, all that is combustible in them would most likely be consumed; and all they could supply him with would be a mass of ether which would not burn.

Those who suppose the tail of a comet to be an electric fluid, are bound to show that electricity may be excited in bodies by being simply placed in contact with the sun's rays; or, at least, to show in what manner this electric matter may be excited. And after all, for what purpose is this waste of fluid? I know it will be answered, To furnish the planets with a constant supply in return for what they are continually expending. But have we any proof that the planets are continually expending it? If they are, why is it not visible in the planets, as well as in the comets? The comets, at all events, appear to be expending it much faster; and it is more likely that they, who are so prodigal, would be in want of a supply, than that they should be able to spare any to the planets, who know so much better how to husband it.

Upon the principle that God has made nothing in vain, I cannot imagine a sufficient cause for the creation of comets, except by supposing that, like the planets, they are habitable worlds; and then the phænomenon of their extraordinary light is accounted for in the necessity they have of possessing, in themselves, a moderate stock of light and heat, the want of which would be se-

verely

verely felt by them, when they were at a distance from the sun, if they had no other supply than the scanty portion which, in that situation, he would be able to afford them.

It will be acknowledged by every one, that although the sun is the source of heat, we are chiefly indebted to the atmosphere for all the warmth that we enjoy. Without an atmosphere, or with a very thin one, we should be insensible to the sun's heat, as people never feel any warmth from a fire till the air in the room is well heated by it. The denser the atmosphere is, that is, the more substance it contains, the more heat it receives and the longer it retains it; or the top of a high mountain would not be covered with snow when the foot of it was scorched up with heat; and as the weight of atmosphere above presses the particles below into a smaller space, the density of the atmosphere is always proportioned to its depth or quantity.

In whatever part of the system therefore the power of the sun's heat may be the greatest, whether in Mercury or Jupiter, and one seems as probable as the other*, a greater or a less quantity of atmosphere would give to all the planets an equal temperature, and none of them would suffer greater extremes of heat and cold than we do in our summer and winter.

Now, though a greater quantity of atmosphere would give to a distant planet all the heat that a nearer one enjoyed, this rule will not answer for the comets; because the same quantity of atmosphere which would be necessary to give to their inhabitants a sufficient quantity of heat, for the purposes of existence when they were furthest from the sun, would destroy them when they were near. The only way I can imagine to remedy this inconvenience is to give to the comets a moderate heat of their own, with

* If the sun be a heated body, like red hot iron or a burning coal, it is probable that his greatest heat is immediately upon his surface; but if he be an incombustible body sending out rays of light and heat; (and if it were otherwise, we ought to see symptoms of his decay;) it is then most likely that his greatest heat is at a considerable distance beyond his surface, even though the rays of heat, like the rays of light, lose a great deal of their substance, or intensity, before they arrive at that point. If we suppose a person to be standing on the surface of the sun, all the rays that could possibly strike upon him, would be included within his visible horizon (a circumference of not more than twenty miles): if afterwards he was lifted up till his eye took in the whole of the sun's hemisphere (a circumference of more than two millions of miles), he would have two hundred thousand times more rays strike upon him than when he was on the sun's surface; and therefore the rays must be two hundred thousand times less heated here than when they first issued from the sun, which is not at all likely, before he could suffer any diminution of their heat. Even beyond that point, every time he doubled his distance from the sun, and at first this distance would be very short, he would have twice the quantity of rays striking upon him; and until the sun's angle became very fine, that is, until he had a great way to travel in order to

with a *less* quantity of atmosphere; and the consequence would be, that the heat of the sun would never overpower them, even where it was greatest, for want of a denser atmosphere; and when they were furthest from him, his faint heat added to their own would give them a healthy temperature, equal to ours in the winter time. With respect to light, the effect would be precisely the same; for though the atmosphere does not magnify the heavenly bodies, it produces a great deal of additional light by bringing down to us a number of diverging rays, that would otherwise pass away from us, as well as by compressing them together and making them brighter. Now if the comets had no light of their own, their day, when at a distance from the sun, would either not equal the brightness of our moonlight; or else, when they were near, the light of their day would be intolerable. By giving them thus a light of their own, equal perhaps to our summer twilight, with a small quantity of atmosphere, the addition of the sun's light to their own when they are at a distance, may about equal our light in winter; and, when they are close to him, may be no more than what we have in summer. By this rule, the comets would never be in perfect darkness, their nights would be always equal to our twilight; with this exception, they would have the same light and heat that we have, and perhaps every other accommodation, whilst their philosophers, in the course of their travels, would have opportunities of making discoveries that must ever be hid from our eyes.

Having converted the ominous and ill-boding comet into a comfortable habitation for man, I now come to show what the tail is, and the cause of its appearing always opposite the sun. I consider the tail to be nothing else than a continuation of the

to make a sensible alteration in the sun's angle, it is very possible that the increased number of rays would more than equal the diminution of their heat. Mercury, for instance, is only thirty-two millions of miles from the sun; and by travelling thirty-two millions of miles further, he would have twice as many rays striking upon him as Mercury received; whereas if he was at the Georgium Sidus, he must travel eighteen hundred millions of miles in order to double the quantity of rays: it is probable, therefore, that long before he arrived at this planet, the diminution of heat would be a great deal more than could be made up by the accession of numbers, and the Georgum Sidus would want an additional quantity of atmosphere to make up for the deficiency.

The place in the heavens where the sun's heat is strongest must always rest upon mere conjecture, because we have no means of ascertaining how much of the sun's heat his rays lose in a given distance: but there is strong ground to believe that the sun's greatest heat is not near himself; because, in Sir Isaac Newton's time, a comet passed within the length of his diameter of him without appearing to suffer any damage; whereas, had the sun's heat, in that place, been so strong as Sir Isaac supposed, the comet must have been burned to a cinder, if he had been formed of any materials of which we have any knowledge.

coma,

coma, which are rays of light proceeding from the comet in the same manner that the rays do from the sun; with this difference, that as the sun is intended to give light and heat to other worlds, his rays proceed in all directions, and consequently exhibit an entire body of light wherever he is beheld (see Plate III. fig. 13), while the comets (see fig. 12), as their light is intended only for their own use, and as they do not need so much, send out their light only in radii.

These rays when they first proceed from the comet are close together, and to a certain extent (the length of the coma), in consequence of being so thick, send down a stronger light into the eye than the secondary rays of the sun, and for that reason are visible: beyond that point, these rays, by diverging as well as diminishing, become as faint or fainter than the sun's; and consequently their light cannot be distinguished from his, except that part of it which is directly behind the comet; for, as the sun's rays are intercepted by the comet, the whole space which the tail occupies, comparatively speaking, is in darkness, and the tail is visible because there is no other light to drown it.

If we suppose S (fig. 11) to be the sun, and C a comet, all the rays from the sun will be intercepted by the comet from the space between the lines D D; and in that space, but for the comet's rays, there would be total darkness; from D to T the sun's rays are gradually admitted; and his light gets stronger and stronger till we come to T, where none of them are excluded.

This figure so entirely corresponds with all the phænomena relating to the comet, that I do not see how the principle can be disputed: it is natural to expect that that part of the comet's light which is contrasted with the greatest darkness, would be the most conspicuous; and accordingly we find that the light of a comet's tail is brightest between the lines D D, and fades away gradually as it approaches towards T.

The shape of the comet's tail is marked out by the figure, and its appearance exactly corresponds with the radii I have described fig. 12.

As the primary rays of the comets do not proceed in every direction like those of the sun, but are merely radii, by the time they reach the earth they are so separated that not more than one ray can enter the eye of the same person*. It is clear then that all the light we see in the body of the comets is derived from the sun, and this will account for the phases they sometimes exhibit.

The reason why the comets are not so brilliant as the planets,

* Even to the inhabitant of the comet these rays will be so scanty that he never will be able to distinguish them, even in the night-time, further than as a faint light overspreading the retina, blended with the secondary rays reflected into his eye at the same time.

is

is not, I imagine, because there is any thing peculiar either in themselves or their atmospheres, but because they are either smaller than the planets; or else, are further removed from us. Mars is not so bright as Jupiter, although he is much nearer, because, as his bulk is smaller, and the sun's rays in consequence diverge more from him than from Jupiter, he cannot send down to us so great (compact) a body of light as Jupiter does. If a beam of the sun's light be let into a dark room and strike upon a concave mirror; another beam upon a plain, and a third upon a convex, mirror; and the persons in the room so disposed that none of the direct rays will be reflected from either into their eyes; although I have never tried the experiment, I am fully persuaded that the concave mirror will give out the greatest body of light, because the rays of light there will be more compressed and close together; and the convex, by the same rule, will give out less than the plain mirror, because the rays striking upon it diverge more, and are consequently much thinner. If in the place of the plain mirror we substitute a very large convex mirror, the large mirror will give out a greater body of light from that part of it on which the rays strike than the small, because its surface approaches nearer to the plain, and its rays in consequence do not diverge so much. Apply this rule to the heavenly bodies, and we can easily account for their different degrees of brightness. I have never seen any comet whose disk would measure so great an angle as that of Venus; and therefore they must either have been further off, or their bulk must have been smaller; and in either case they could not give out so great a body of light.

There is one other phænomenon in the comet which I notice, because it has excited the curiosity of a great many, although my principle will not be any way affected whether I rightly account for it or not. The comet's tail sometimes, though not always, appears to be a little curved, and the reason for it, in my opinion, is this: When the comet is at a distance and going almost directly towards the sun, the direction of the tail does not sensibly alter in the course of several hours, because the sun, which is always opposite the tail, continues with respect to the comet, to be very nearly in the same part of the heavens: but when the comet is going round the sun, and has greatly increased its velocity, every moment changes in a small degree the direction of the tail, and the impression of its rays does not go off instantaneously from the retina. Thus a burnt stick, waving about, appears like a riband of fire, and the same cause may also produce the apparent curve in a comet's tail.

4. In order to account for the zodiacal light, it can hardly be necessary to say more than that I suppose it to be neither

more nor less than the sun's secondary rays sending light down into the eye. I have seen it frequently in the East Indies; and the appearance it puts on is exactly what I should have anticipated, had I conceived my principle before I saw it. It resembles very much the stars in the Milky Way, and in the centre near the horizon is very nearly as bright; it is broad at the base, pointed like a leaf, and fades away imperceptibly at the edges. This light always marks the track of the sun, and therefore is evidently derived from him; and that the sun may send his light down in this way is proved by the moon, a fainter light, doing the same thing. The zodiacal light, the tail of a comet, and the stars in the Milky Way are very nearly eclipsed by the greater splendour of the moon's light, though to look at either of these, we must turn our back towards her; and as her direct light in this situation cannot enter the eye, it is impossible to account for this but by supposing that her secondary rays cover the retina with as bright a light as these phænomena would otherwise exhibit. This phænomenon cannot in any way be accounted for by supposing that it has anything to do with the sun's atmosphere, for, if the sun's atmosphere did extend so far, it would no longer be the sun's atmosphere: the only reason why our own atmosphere, water, stones, and other loose bodies do not fly off into the sun, is because the earth's attraction, which is close, is stronger than the sun's, which is at a distance; and therefore, if the sun's atmosphere did come so near to the earth, the earth's attraction would seize hold of a great part of it, and sad confusion would ensue.

5. The phænomenon of the twilight bears so close a resemblance to the zodiacal light, that if my principle be admitted in the one case it will hardly be disputed in the other. The twilight, like the zodiacal light, is evidently derived from the sun; for it is always strongest in that part of the horizon which is opposite to him; and the only difference between them is, that owing to the greater density of the atmosphere in our climate, the sun's rays are refracted and pressed closer together, and consequently send down a greater body of light; and this is the reason why the figure of the twilight is not so perfect as that of the zodiacal light. Although the refraction of the atmosphere will account for the greater light of the twilight and the flatness of its figure, we cannot upon that principle account for its light being brought into the eye; because, if the primary rays of the sun were still refracted into the eye, all the consequence would be, that the sun would appear to rise higher and be longer in setting; for upon no principle of refraction can we account for the sun's rays diverging upward being brought down by refraction into the eye, without supposing that those that were not directed so high would

would be refracted down to our feet; and therefore the sun's rays could not occupy a greater space upon the retina after sunset than they did before, and that is no larger than the angle of his diameter—half a degree.

Those who argue that the sun's diverging rays, after he has set, and the atmosphere, in consequence of increased cold, become denser, are reflected into the eye from all parts of the heavens, by the particles of air, are bound to show in what manner the rays of blue colour are brought into the eye; for, if the blue colour of the heavens be owing to the particles of air reflecting the blue rays in greater abundance than all the rest, an increased quantity of light would only produce a more intense blue; and there is no other way in which the twilight can be produced, but in the manner I have supposed.

In the summer the sun lingers a long while on the horizon before he sets, and the twilight is very full; whereas in the winter he sets almost immediately, and the twilight is hardly perceptible; and as the twilight is owing to the density of the atmosphere, and this density is increased by cold, it will perhaps occur to the reader to demand why the twilight is not stronger in winter than in summer. To this I answer, That in the summer the sun is very far to the northward, and after he has set, his light, to come to us, passes in great measure through that part of the atmosphere which is densest and refracts light the most; and accordingly, we find that the twilight is always stronger on the north than the west side of him; and while it extends but a very little way towards the west, it stretches over the whole of the north part of the horizon. In winter, on the contrary, the sun's rays at setting, in order to come to us, must pass through the torrid zone, where the atmosphere is rarest and refracts light the least; and not only this, but immediately after he has set, his rays, to come to us, must travel a hundred degrees of latitude further than they do in the summer; and that part of the twilight which appears to us to be but just above the horizon, is very near the zenith to those who are in the same degree of south latitude, and consequently is almost too faint to be visible.

6. I have never had the good fortune to witness any of the phænomena of the *aurora borealis*, and the idea my imagination has formed of it from description may be very different to what it is in reality. From all accounts however it appears, in the groundwork at least, to bear a very close resemblance to the twilight, so much so that it is sometimes called the northern twilight; and if I could but satisfactorily account for the crackling noise that is sometimes heard with it, I should pronounce it to be no other than a twilight attended with some additional phænomena

 which

which may easily be explained in the increased density and elasticity of the air.

The prevailing opinion however, at present, respecting the *aurora borealis*, is, that it is an electric fluid, though from all I have read upon the subject, the only resemblance it has to an electric fluid is, that light is to be found in both phænomena. We are told that the use of this meteor is to furnish lightning to the torrid zone; but in what manner it communicates with the reservoirs of the electric fluid, the clouds, has not been explained. For my own part, I think it a great deal more likely that the combustible matter, or, if it be more philosophical, the electric fluid in a dormant state, with which the clouds are loaded, is carried up from the earth in exhalations; and that when these exhalations are condensed into clouds, and excited, perhaps, by a heated atmosphere, they explode and go off in lightning; and this, most likely, is the reason why there is more lightning in the torrid than the temperate zone, in the summer than the winter; for people always prognosticate a thunderstorm when the air appears to be unusually heated. But, whatever may be the cause of the lightning, the *aurora borealis* is by no means fitted to furnish a supply of electric matter to the torrid zone; for, if the electric fluid (lightning) expends itself in flame, the *aurora borealis* also goes off in flame, and its whole stock of electric matter would be expended before it got to the torrid regions; and if it does not expend itself in flame, then it can stand in no need of a supply.

So far then I have cleared the way for the introduction of my own hypothesis; and if I can but satisfactorily account for the crackling, I can imagine no other objection that will at all affect it, because all the other phænomena, upon my principle, may be proved by analogy, and accounted for by the known laws of nature.

I do not know whether any one has ever yet attempted to explain the cause of the refracting power of the atmosphere; but it must be obvious to every one that its principle is totally different to that of every other medium. In every other medium the angle of refraction is always in proportion to the angle of incidence, let that angle be what it will; but in the atmosphere this is not the case, its figure has nothing to do with it, and the degree of refraction is always in proportion to the density of its particles. Light was supposed by Sir Isaac Newton, if not by all the philosophers, to partake of the nature of matter, and therefore should be subject to the laws of attraction as well as all other matter. The power of attraction is always in proportion to the velocity of the body in motion; and the reason why light is refracted more in a dense atmosphere than in a rare one, is not because there is

any

any refracting power in the atmosphere, but because, by diminishing the velocity of light in a greater degree, it increases the power of the earth's attraction.

If we admit this hypothesis, and it is the only one that will account for the refraction of the atmosphere, the crackling of the *aurora borealis* is explained in the rays of light forcing their way against the resistance of the atmosphere, and the crackling of electricity may be owing to the same cause. If it be objected that, as light is constantly passing through the air, and the air as constantly resisting it, this noise ought to be heard at all times; I reply by referring to another sense, the sense of smelling. In all warm climates there is a regular land and sea breeze: the wind blows constantly upon the land during the greatest part of the night; but a little before the dawn the land breeze springs up, and brings with it all the scents that the earth exhales as far as it extends, which I have sometimes known, on the coast of Sumatra, to be more than forty miles. The scent of the land is clearly perceived by every person who happens to be upon deck the moment this breeze reaches him: but it lasts only for a moment; for as soon as the sense of smelling is accustomed to these effluvia which the wind brings to it, the effect is destroyed, and it can only be excited by something that is more powerful. A person on first going into a flower garden, may be almost overcome with a delicious scent; and yet in a few minutes he will only be able to perceive it by going close to the flowers from whence it proceeds. The scent of the garden is as strong as ever; but after the mind is accustomed to it, it requires a stronger excitement to be able to perceive it: and in like manner the faint noise that the light may be always making in passing through the air is so uniform, that the mind is incapable of perceiving it: but when a body of light very suddenly forces its way through the atmosphere, within perhaps a few feet of us, where there was none before, there is a new incitement, and totally different from the uniformity that prevails every where else.

The *aurora borealis*, by the descriptions I have read of it, is a faint light, like the twilight, strongest at the horizon, and fading away gradually as it rises towards the zenith: it is always strongest in that part of the horizon which is in a line with the sun; and so far their relationship may be traced in their resemblance, and the phænomena be attributed to the same cause. The *aurora borealis* however is frequently in motion, while the twilight is always still; and it is only by assigning a sufficient cause for this phænomenon that I can have any pretensions of establishing my hypothesis.

It was the custom in Lord Collingwood's fleet off Cadiz to set the bearings of the high land over that town every morning at day dawn; and as the ships generally made a good offing during the night, it frequently happened that, after sunrise and the air became heated, we had to run seven or eight leagues directly towards it, before we were able to see the very same land that we had seen when we were more than twenty miles further off from it. Here there is sufficient proof that heat rarefies the air, and cold condenses it; and that the refraction of light passing through the atmosphere is always in proportion to its density.

It was remarked in Captain Ross's voyage to discover the north-west passage, that the motion of a field of ice very frequently produced the strangest optical delusions; that the atmosphere, in consequence, becoming suddenly condensed, the surrounding objects would in a moment be magnified three or four times beyond their ordinary size, and, on its going off, would as suddenly resume their former shape. Now the very same principle that will account for this phænomenon will also explain the motion of the *aurora borealis.* In consequence of the motion of an iceberg, the atmosphere may become more condensed; and the rays of light, which before were too faint to be perceived, will be condensed into a solid mass, and send down a body of light into the eye: or, what is more likely, the secondary rays from the sun, which before were passing outside of the eye, are now brought into it in a body by the increased refraction of the atmosphere, and, according to the circumstances, exhibit the various phænomena which have been recorded of this meteor.

Whatever may be the true principle of the atmosphere, we cannot better explain its effect, as far as regards refraction, than by comparing the particles of which it is composed to balls of fungus, which may be compressed into a very small space, but always endeavour to recover their original state; and this action and reaction of the particles of air is the only way by which we can account for the twinkling of the stars. In the dead of the night, when the *aurora borealis* is to be seen, the sun is in the opposite hemisphere; and his heat, expanding the particles of air in the torrid and temperate zones, causes them to press upon those that are in the frigid zone, and compress them into a smaller space: these particles always endeavour to recover their former state; and the motion of an iceberg, or the change of a hot wind for a cold one, will produce a very sensible effect in those regions where the refraction is so great.

Even the variety of colour which the *aurora borealis* is sometimes said to assume, may be accounted for by the principle of refraction; for, as refraction separates the rays of colour, where there is a great deal of refraction we may reasonably expect it; and if a streak of light is sometimes seen in the air unconnected with any light below, we may account for it by supposing that the wind,

wind, blowing over an iceberg which intercepts all that is below, causes the atmosphere to be denser above than it is below, and consequently the light above passes into the eye, while that which is below passes outside of it.

XXVIII. *An Account of a Peach Tree produced from the Seed of the Almond Tree; with some Observations on the Origin of the Peach Tree. By* THOMAS ANDREW KNIGHT, *Esq. F.R.S. F.L.S. &c. President**.

I BEG leave to send to the Society a couple of peaches, of a new variety: not, however, on account of any merits which I suppose the variety to possess, but solely on account of the singularity of its origin, it being the offspring of a sweet almond and of the pollen only of a peach. The tree produced six peaches besides those I have sent you; three of which cleft open, like almonds, when nearly ripe, whilst the others retained the form and character of peaches, and the flesh of all was perfectly soft and melting. One of these was considerably larger than the largest you receive, having measured eight inches in circumference; and as the tree grew in a pot, which did not contain a square foot of mould, and the first fruit of every seedling tree has proved, in all my experiments, to be of much less size than its subsequent produce, I imagine that the future fruit of this variety will a good deal exceed the bulk indicated by the present sample.

The general character and quality of the fruit I send, and the diminished size of its stone, comparatively with that of the almond, will, I fear, induce the Society to apprehend some error in the experiment: but I beg to assure them that none can possibly have occurred; and that the result was as unexpected by me as it would have been by them; for I did not entertain the slightest hope that a tree capable of producing a melting peach could have been, by any means, obtained immediately from an almond. I had, however, long before entertained an opinion, that the common almond and the peach tree constituted only a single species, and that the almond might, by proper culture, through many successive generations, be ultimately converted into a peach or nectarine.

Many circumstances in the ancient history of the peach conjoined to lead me to this conclusion. It does not appear to have been known in Europe till about the reign of the Emperor Claudius: and it is, I believe, first mentioned by Columella. Pliny has given the first accurate description of it; and he states it to have come through Egypt and Rhodes into Italy from Persia,

* From the Transactions of the Horticultural Society of London.

which is universally understood to have been its native country. Yet it could not have existed in Persia a few centuries previous to the period of its appearance in Europe, or the Greeks must necessarily have known it, as much intercourse constantly took place between the Asiatic Greeks and the Persians, and the kings of Persia usually entertained Greek physicians, who were botanists, in their court. The *tuberes* of Pliny also appear to have been something intermediate between the almond and peach; for he states the trees which produced this fruit to have been propagated by being grafted upon plum stocks, and to have blossomed later than the apricot; and that the fruit itself was covered thickly with down, like the quince.

The *tuberes* must, therefore, I conceive, have been swollen almonds, or imperfect peaches (for their merit, as fruit, appears to have been very inconsiderable); and Du Hamel has given an account of a fruit, which accurately corresponds with this description, being sometimes produced by a variety of almond-tree; and which, he says, is bitter, and not eatable in its crude state.

The bitterness in this case, I conclude, can only arise from the presence of the prussic acid; and as this acid, without being extracted by distillation, operates very injuriously upon many constitutions, some explanation appears to be given of the cause why the peach was reported to possess deleterious qualities, when it first came from Persia into the Roman empire.

The fact, if ever so decisively established, of the specific identity of the peach and almond, is probably of little importance to the gardener, further, than that it points out to him the extensive changes that culture is capable of producing, in the forms and qualities of fruits: and I made the experiments, which are the subject of this communication, with scarcely any other view, than that of simply ascertaining the specific identity, or diversity, of the peach and almond, and with a good deal of indifference relative to the result. Nevertheless, as the wood of the almond tree ripens much earlier, and more perfectly, than that of the peach tree, in our climate, and as its blossoms are more hardy, I am not without hopes, from observations which I have made upon the habits of my seedling plants, that some valuable varieties of the peach will be obtained, in a second or third generation, from the almond. I have, at present, seen the fruit of one seedling plant only, and that not one of promising character; but I have others which will produce blossoms in the next season, one of which, a descendant from the early violet nectarine, as its male parent, presents very large and beautiful foliage, with a purple bark, and all the character of a peach tree of the most improved kind; and I look forward to the pleasure of sending next season a fruit of much superior quality.

Note

Note by the Secretary.

The two peaches alluded to in this paper were of a perfectly globular form, the largest exceeding seven inches in circumference. The *skin*, which was covered with a rather thick down, was of a delicate yellow, tinted on the sunny side with pale red, and beautifully marbled with a deeper shade of the same colour. The *flesh* was of a pleasant pale citron tint, and round the stone of a very brilliant carmine red; it was perfectly melting, sweet, and very juicy, though not very high flavoured, but it had suffered in this quality by the injury sustained in carriage. The *stone* was large in proportion to the fruit, nearly round, with a small point at the top, very rugged, and had much of the same kind of farina on its surface as is usually seen on that of the almond in its fresh state; it also separated very clearly from the flesh, some short filaments only adhering to it.

XXIX. *Address of the Astronomical Society of London: established February* 8, 1820.

In a country like Great Britain, in which the sciences in general are diligently cultivated, and Astronomy in particular has made extensive progress and attracted a large share of attention, it must seem strange that no Society should exist peculiarly devoted to the cultivation of this science; and that (while chemistry, mineralogy, geology, natural history, and many other important departments both of science and of art are promoted by associated bodies, which direct, while they stimulate, the highest exertion of individual talent) Astronomy, the sublimest branch of human knowledge, has remained up to the present time unassisted by that most powerful aid; and has relied for its advancement on the labours of insulated and independent individuals.

It may be conceived by some, that astronomy stands less in need of assistance of this kind than any other of the sciences; and that, in the state of perfection which its physical theory has already reached, its ulterior progress may safely be intrusted to individual zeal, and to the great national establishment exclusively appropriated to celestial observations; or, at all events, to those public institutions and academies in all civilized nations, whose object is the general cultivation of the mathematical and physical sciences. It may therefore be necessary to state the useful objects which may be accomplished, and the impediments which may be removed, by the formation of a Society devoted solely to the encouragement and promotion of astronomy.

Owing

Owing to the great perfection which the construction of optical instruments has attained in England, and the taste for scientific research universally prevalent, there have arisen in various parts of the kingdom a number of private and public observatories, in which the celestial phænomena are watched, and registered with assiduity and accuracy, by men whose leisure and talents peculiarly adapt them for such pursuits: while others, with a less splendid establishment, but by the sacrifice of more valuable time, pursue the same end with equal zeal and perseverance. Considerable collections of valuable observations have thus originated; by far the greater part of which, however, owing to the expense and difficulty of publication and various other causes, must inevitably perish, or at least remain buried in obscurity, and be lost to all useful purposes; unless collected and brought together by the establishment of a common centre of communication and classification, to which they may respectively be imparted.

This great desideratum, it is presumed, will be attained by a Society founded on the model of other scientific institutions, having for one of its objects the formation of a collection or deposit of manuscript observations, &c. open at all times to inspection; to which the industrious observer may consign the result of his labours, with the certainty of their finding a place, among the materials of knowledge so amassed, exactly proportioned to their intrinsic value. At the same time it will thus be rendered practicable to form a connected series from a mass of detached and incomplete fragments; and the society will render a valuable service to science, by publishing, from time to time, from this collection, such communications or digests as seem calculated, by their nature and accuracy, either to supply deficiencies, or to afford useful materials to the theoretical astronomer.

It will also be an object worthy of the society, to promote an examination of the heavens in minute detail; by parcelling them out, in portions of a very moderate extent, among those members who may find leisure and inclination to direct their attention more peculiarly and constantly to such portions (selection being made as to those which may best accord with the situation of their observatories and their own general convenience); thereby to ascertain the places, and if possible the proper motions, of all the objects, large or minute, which may fall within their respective limits; and to pass them continually in review, so that no new celestial body of a cometary or planetary nature, traversing their boundaries, may escape detection. For, amongst the vast multitude of similar objects which are scattered over the wide expanse of the heavens, and which equally solicit and distract the attention

attention of the insulated observer, no one of them in particular can be expected to undergo any very rigorous examination, unless distinguished by magnitude or some peculiarity of appearance.

The knowledge of our own peculiar system, and that more extended branch of astronomical science to which the name Cosmology is best adapted, may alike be benefited by this division of labour, and systematic mode of examination. In the planetary system, a wide field of investigation has of late been opened by the discovery of some links in the chain of connexion which no doubt exists between bodies of a cometary and planetary nature. And it is possible that some bodies, of a nature altogether new, and whose discovery may tend in future to disclose important secrets in the system of the universe, may be concealed under the appearance of minute stars, no way distinguishable from others of a less interesting character, but by the test of careful and often repeated observations. Indeed it is worthy of notice, that, of the five small bodies lately ascertained to be permanent members of our system, four were discovered in the short space of seven years, by the partial adoption, on the continent, of this very plan of separate examination; which seems to have been first suggested by the late Rev. F. Wollaston. This diligent astronomer, in a paper published in the Philosophical Transactions for 1784, thus remarks: "The first idea which occurred to me was to make a proposal to astronomers in general, that each should undertake a strict examination of a certain district in the heavens; and not only by a re-examination of the catalogues hitherto published, but by taking the right ascension and declination of every star in their several allotments, to frame an exact map of it, with a corresponding catalogue; and to communicate their observations to one common centre. This is what I should be glad to see begun. Every astronomer must wish it, and therefore every one should be ready to take his share in it." In fact, Mr. Wollaston not only proposed the plan, but, as far as an individual could do so, put it in execution, by undertaking the examination of the circumpolar regions himself.

Beyond the limits however of our own system, all at present is obscurity. Some vast and general views on the construction of the heavens, and the laws which may regulate the formation and motions of sidereal systems, have, it is true, been struck out; but, like the theories of the earth which have so long occupied the speculations of geologists, they remain to be supported or refuted by the slow accumulation of a mass of facts: and it is here, as in the science just alluded to, that the advantages of associated labour will appear more eminently conspicuous.

One of the first great steps towards an accurate knowledge of the construction of the heavens, is an acquaintance with the individual

dividual objects they present: in other words, the formation of a complete catalogue of stars and of other bodies, upon a scale infinitely more extensive than any that has yet been undertaken; and that shall comprehend the most minute objects visible in good astronomical telescopes. To form such a catalogue, however, is an undertaking of such overwhelming labour, as to defy the utmost exertions of individual industry. Is is a task which, to be accomplished, *must* be divided among numbers: but so divided as to preserve a perfect unity of design, and prevent the loss of labour which must result from several observers working at once on the same region, while others are left unexamined. The idea now understood to be entertained of establishing an observatory at the southern extremity of Africa, under the auspices of the Admiralty, may serve to show the general sense entertained of the importance of this subject, and the necessity of giving every possible perfection to our catalogue of the fixed stars. Deeply impressed also with the importance of this task, and fully aware of its difficulty, the Astronomical Society might call upon the observers of Europe and of the world to lend their aid in its prosecution. Should similar institutions be formed in other countries, the Astronomical Society (rejecting all views but that of benefiting science) might profess themselves ready and desirous to divide at once the labour and the glory of this Herculean attempt, and to act in concert together in such manner as should be judged most conducive to the end in view.

Another beneficial result to be expected from this institution is the diffusion of a spirit of inquiry in practical astronomy; and, as a necessary consequence, a corresponding diffusion of a general knowledge of the mode of performing and computing astronomical and geodesical observations, and of the use of instruments; especially such as are likely to be found in the hands of travellers, nautical men, and others who may be placed in interesting situations in remote parts of the world. Widely scattered as Englishmen are, over the surface of the globe, the advantages which might accrue to science from a more general diffusion of such knowledge, are incalculable: yet it is painful to reflect in how few cases, comparatively, among the numbers of our countrymen whose prospects in life lead them to distant climates, the actual use of even the simplest astronomical instruments and tables has formed a part of their education or study. In a national point of view, every thing which may tend to diffuse a knowledge of practical astronomy is obviously of the utmost importance, on account of its application to nautical purposes. Besides which, the difficulty of finding practical observers calculated to fill situations as assistants in observatories, in expeditions of discovery, or on other occasions, at moderate salaries, has been felt in various

rious instances. Were there establishments in our universities and other places of public education, in which young men might be taught the use of astronomical instruments and tables, not only would the theoretical knowledge of astronomy which they are led to acquire in those admirable establishments make a deeper impression, but a greater number of good observers would thus be annually trained up, to the great benefit not only of themselves but of their country. It is understood that at the Royal Military Seminaries some establishment of this kind exists.

It is almost unnecessary to enumerate the advantages likely to accrue from the encouragement which an Astronomical Society may hold out: but among others may be mentioned the perfecting of our knowledge of the latitudes and longitudes of places in every region of the globe; the improvement of the lunar theory, and that of the figure of the earth, by occultations, appulses, and eclipses simultaneously observed in different situations; the advancement of our knowledge of the laws of atmospherical refraction in different climates, by corresponding observations of the fixed stars; the means of determining more correctly the orbits of comets by observations made in the most distant parts of the world: and in general, the frequent opportunities, afforded to a society holding extensive correspondence, of amassing materials which (though separately of small importance) may by their union become not only interesting at the present time, but also valuable as subjects of reference in future.

By means of corresponding members, or associates, in distant countries, the society may hope to unite the labours of foreign observers with their own; and by thus establishing communications with eminent astronomers and institutions in all parts of the world, to obtain the earliest intelligence of new discoveries or improvements; which it may, perhaps, be desirable to circulate among such of its members as may profess themselves anxious to receive it, without loss of time.

The circulation also of notices of remarkable celestial phænomena about to happen, (with the view of drawing the attention of observers to points which may serve important purposes in the determination of elements or coefficients) may form another, and perhaps not the least interesting object of the society. To have the same phænomena watched for by many observers, in a climate so uncertain as our own, is the only sure way of having them observed by some: and moreover, the attention of an astronomer may frequently be aroused by a formal notice, especially when accompanied with directions for observing the phænomenon in the most effective way, when probably the mere ordinary mention of it in an ephemeris might fail to attract his observation.

One

One of the collateral advantages of a society including many practical astronomers among its members, (but which will appear of no small importance to those who possess good instruments) will be the mutual understanding which will be propagated among amateur astronomers, by frequent meetings and discussion, as to the relative merits of their instruments; and as to the talents and ingenuity of the various artists both of our own and of foreign nations: not to mention the emulation which this must naturally excite to possess the best instruments; and the consequent tendency of such discussion towards a further improvement in their construction, or to the discovery of new ones. Well-made instruments will thus unavoidably acquire a reputation, not merely among a few eminently skilful observers in Britain, but throughout the whole astronomical world: and individuals, who have at great expense and trouble amassed a collection of valuable ones, will thus be spared the mortification of knowing that they may at some future time be put up to sale and be disposed of for a half or a third of their value, for want of their merits being known; a consideration which probably has some weight with those who may be collecting instruments at an advanced period of their life.

As the extent of the funds of the society must depend on the number of its members, it is impossible to conjecture at present how far its views respecting their application may extend. Besides the ordinary expenses attending an institution of this nature, the occasional or annual publication of communicated observations;—the payment of computers employed in the reduction and arrangement of observations, or in computing the orbits of new planets, comets, or other interesting bodies;—the formation of an extensive astronomical library, not only of manuscript but also of printed books;—and perhaps, at some future period, the proposal of prizes for the encouragement of particular departments of the science, either theoretical or practical; or for the improvement of astronomical instruments, or tables, may be mentioned as worthy objects on which they may be bestowed.

Such are the principal considerations which have actuated a number of individuals to form themselves into a Society under the name of the *Astronomical Society of London*, and to give this publicity to their determination, with a view of inviting others to unite in the prosecution of their plans. They have at their very commencement met with the most flattering success, which induces them to hope that, in a short time, every assiduous cultivator of the science will be found to have added his name to the list of members.

The objects of the original members may be sufficiently gathered from

from what has been already said, and may be thus summed up in few words: viz. To encourage and promote their peculiar science by every means in their power, but especially—by collecting, reducing, and publishing useful observations and tables—by setting on foot a minute and systematic examination of the heavens—by encouraging a general spirit of inquiry in practical astronomy—by establishing communications with foreign observers—by circulating notices of all remarkable phænomena about to happen, and of discoveries as they arise—by comparing the merits of different artists eminent in the construction of astronomical instruments—by proposing prizes for the improvement of particular departments, and bestowing medals or rewards on successful research in all—and finally, by acting as far as possible, in concert with every institution, both in England and abroad, whose objects have any thing in common with their own; but avoiding all interference with the objects and interests of established scientific bodies.

XXX. *On the Method of extracting Iodine from Kelp.* By *M.* Van Mons*.

Those chemists who have directed their attention to a method for obtaining iodine by the decomposition of the iodate of potash existing in kelp, have proposed several methods for the purpose. Some recommend to separate from the lye of the kelp all the crystallizable salts, and to treat the mother-water with sulphuric acid;—others advise that the sub-carbonate of soda alone should be separated, and the liquid or the remaining salts treated with sulphuric acid;—a third class suggest methods more or less complicated, among which they cite as advantageous the addition of the superoxide of manganese, or the hypo-superoxide of lead;—and lastly, it has been prescribed to separate the iodate from the other salts, by taking advantage of its solubility in alcohol.

Each of these processes has its inconveniences. In adopting the first, there is a risk of making the sulphuric acid act on the mother water, which often contains no more than one atom of salt to furnish the iodine. We have experienced this, and have remarked, that from the moment that the greater part of the salts became separated by crystallization, and that the lye began to thicken, when it was desired to carry the separation further, the salt containing the iodic acid was at the same time carried away; already even more or less of it had passed with the first crystals; so that, when the mother-water contained no more crystallizable salts, there ordinarily remained no more iodate.

* From *Annales Generales des Sciences Physiques.*

The

The second process is attended with the inconvenience of subjecting a large mass of salt to the decomposing action of sulphuric acid, before the latter can begin to operate upon the iodate; for it is not till after the other salts, and especially the muriates, are decomposed, that an excess of sulphuric acid attacks the iodate of potash, and separates from it the alkali, which at the same instant oxygenizes itself at the expense of a portion of the same acid. If in place of muriates the solution contains nitrates, the decomposition of the latter suffices to oxygenate the iodic acid, on account of the facility with which the nitric acid susceptible of suboxygenation gives forth its oxygen. By extracting from the lye nothing but the sub-carbonate of soda, we obtain what is of little or no use; for the different sodas of Normandy with which we have made experiments, do not even contain as much of this salt as will produce a change in the tincture of curcuma. How, besides, can it be conceived that there exists at the same time in a lye a free alkali and deliquescent muriates? The express condition that the muriates shall be decomposed previous to the iodate, occasions an excessive expenditure of acid, besides the inconvenience of prolonging the operation, and allowing much iodine to escape, if the precaution is neglected of not allowing the matter to get into ebullition before the greater part of the muriatic acid is expelled.

The process in which they mingle the superoxide of manganese with the residue is scarcely any better; it occasions a production of chlorine which contains the iodine in combination. The addition of the superoxide may be more or less permitted, where iodate exempt from muriate is made use of; the superoxide may then serve rather to regenerate the sulphuric acid by the oxygenation of the sulphurous acid, than to oxygenate the iodic acid. This addition, taking care not to add at once all the sulphuric acid, may further have the effect of facilitating the separation of the muriatic acid, by converting it into chlorine; but the presence of chlorine produces such a disposition to the formation of iodine, and the affinity between the two bodies is so manifest, that a very great loss of iodine must be expected.

We shall not say any thing of the process by which the iodate of potash is isolated by means of alcohol: it requires that the muriates of lime and magnesia contained in the lye should be previously decomposed by the subcarbonate of potash; otherwise these muriates, being soluble in alcohol, would become confounded with the iodine. Besides, this process can only be employed in demonstrations.

Our results are infinitely more advantageous. We reduce the rough kelp into powder, and pass it through a hair-sieve; when it is not dry enough to be submitted to that pulverization, we pound

pound it well in a mortar with water, and make it boil for half an hour, stirring it continually with an iron spatula; we then filter the mixture and set it to evaporate, taking care to lift up with a skimmer the salts, a certain quantity of which the hot lye allows to be deposited. If evaporated till crystalization takes place by cooling, the salts become confounded in the crystallization, and a great deal of iodate remains adhering to the crystals.

The crystals, however weak may have been the lye in which they are formed, are always impregnated with iodate, which is in a state of such strong combination, that these crystals, although perfectly dried, redden strongly with sulphuric acid. It is better, therefore, to allow the salt to be deposited in the hot lye, where the muriates are not much more soluble than in the cold lye, where the iodate remains a much longer time in solution. The salt on being separated from the lye, and while it is still hot, should be spread on a hair-sieve, where it may dry itself. When about half of the salt has been dried, it is put aside in order to be afterwards pulverised and washed. The evaporation is continued with the same precaution—that is to say, removing the salt in proportion as the crystals are formed, until it ceases to form any more. The lye is then united with the drain water, and evaporated to dryness. A single lixiviation will then suffice to exhaust the kelp of its salt.

We would advise that this salt should not be thrown away, as there still adheres to it a good deal of iodate, until it has been passed through a hot funnel. As the first crystallizations of this salt redden with sulphuric acid as much as the last, they equally contain iodate, which it is necessary to separate. For this purpose the salt is well shaken in large glass funnels; and by little and little warm water is passed though it, trying at each washing whether the salt continues to redden with sulphuric acid.

The residue of the lye evaporated to dryness gets soon damp in the air. It is necessary, therefore, to preserve it in flasks well corked, if the iodine is not immediately wanted: when the latter is the case, the residue is put into an earthen pan, and pounded continually, incorporating with it by instillation concentrated sulphuric acid; we say by instillation; for if a drop of acid rests on the place where it falls, a spot of iodine is soon manifested, and a degree of heat developed which it is of importance to avoid.

When enough of acid has been added to decompose the muriate and the iodate, the mixture is transferred to a glass or earthen alembic in order to be distilled. The heat at first being moderate, ought not to be carried to that degree of ebullition that no more muriatic acid is seen to pass, otherwise the iodine, which at that temperature instantly evaporates, may be in part dissolved by that acid.

 After

After iodine has ceased to be disengaged, in order to try if the residue is exhausted, a new quantity of sulphuric acid is poured upon it while it is still hot; if at the moment of contact a white vapour without any tinge of red arises, it is a proof that the iodine is all disengaged. This white vapour is produced by an acid substance, in which we have recognised the following properties: It does not dissolve iodine; it possesses a flavour similar to that of dilute sulphuric acid; it gives no precipitate with muriate of barytes, but with nitrate of silver and the oxymuriate of mercury it does (the latter precipitate of a pale red colour); and fills the atmosphere with the odour of chlorine. We purpose afterwards to examine this product.

The iodine may be disengaged at the same time with the muriatic acid, and as soon as that acid, by applying at once a boiling heat. The vapours which appear are violet, and the little muriatic acid which passes at the same time is not particularly coloured. It has been said, that at the degree of heat at which iodine can be produced, the action of the sulphuric acid tends rather to produce this body than to disengage the muriatic acid. It is true that the strongest affinity is then in favour of the muriates, and that the weakest acids, perhaps even sulphurous acid; may decompose the oxygenated iodates. The operation, whatever may be the quantity of salt, is finished in an hour at the most. The saline residue, in an experiment which had been interrupted after the disengagement of iodine, was composed in its liquid part, which was decanted warm, of super-sulphate of soda and super-sulphate of potash, besides free sulphuric acid; the remaining part, concrete when warm, consisted of muriate of potash and muriate of soda. This proves that muriatic acid had concurred in the decomposition of the oxygenated iodate of potash. It appeared singular that acidulated sulphates and free sulphuric acid had been able to exist together, and to be heated to ebullition, without being mutually decomposed. After having distinguished the different salts, we joined them to the mother-waters, and distilled the mixture to dryness. A great deal of muriatic acid passed, and there only remained neutral sulphates of potash and soda.

In a similar experiment, but which was not interrupted after the disengagement of the iodine, a great deal of muriatic acid passed at the distillation. This acid was absolutely colourless, and almost without odour. However, a good deal of muriate of iron was found in the residue, and the acid obtained had enough of concentration to fume if it had been otherwise prepared.

We made the experiment of washing with a little water the rough kelp finely pulverised. The solution contained more iodate and less of other salts than the lye made in a large quantity of water;

water; but there remained after this mode of extraction, still too much iodate in the residue to make it of advantage to follow it.

If the separation of the crystallizable salts is pushed too far, and especially if it is sought to obtain them by the recooling of the lye, nothing, as we have said before, will be preserved but a mother-water exhausted of iodate, and which will not yield the least part of iodine. It is from not finding iodine in the mother-water of a soda known to contain it, that we have thought of seeking for it in the crystallized salt.

It has been stated that, from Scotch kelp, iodate of soda as well as iodate of potash may be extracted. That kelp cannot therefore reckon amongst its salts either the sulphate or muriate of potash, which would be decomposed by the iodic acid of the iodate of soda, although there may be found in the same lye the sulphate of soda and the muriate of potash. It is impossible, however, to admit, as some authors have done, the existence of these different salts with the iodates of magnesia and of lime.

The soda with which we have experimented was of the species known in commerce under the name of common soda of Fecamp. The pieces recently broken presented in their fracture a blueish gray colour; they were besprinkled with opaque white crystalline knots; the parts which had been exposed to the contact of the air were humid, of a black colour, and contained also knots of salt. Their lye gave about a fourth of their weight in saline products, but no sub-carbonate of soda. The water of a first cold washing contained muriate of lime, and the lye obtained by boiling contained muriate of magnesia. The former muriate was no longer to be found in the lye, having been decomposed by the sulphate of soda. Common soda preserved for thirty years, and which after being damped was completely dried, did not yield less iodine than kelp newly bought. In Belgium, where considerable use is made of kelp for the fabrication of common glass, it is found to give a very good frit, and that the salts supply to that what it wants of alkali; the solution of its residue after the separation of the iodine, leaving on the filter a good deal of silex. The alkali may be considered as being partly in the state of liquid silicate; which explains how its lye in the neutral alkali saturates a certain quantity of acid.

We have tried to decompose at the outset the muriatic salts, by pouring into the lye a quantity of sulphuric acid—at first, because that method accelerated the operation—afterwards, because we found that in operating with a salt which is not very dry, or which has got moist in the air, the muriatic acid which passes, and the expulsion of which demands a greater degree of heat, is always much coloured with iodine; but we have in experiments on a large scale dissipated in air the muriatic gas by decomposing with a slow fire the salt mixed with the sulphuric acid.

In following our process, no more need at first be incorporated than the sulphuric acid indispensable for the decomposition of muriates; and after that decomposition, the remainder of the acid may be added by spreading it uniformly over the whole mass of salt; but this precaution is not necessary when the process is followed throughout in the way which we have pointed out.

The separation of the salts may be also attempted by subjecting their mixture in a dry state to a heat capable only of liquifying the iodate of potash, and the sulphate of soda; the supernatant liquid will cover two muriates, and the sulphate of potash not melted.

We expected to have found the means of separating the iodate with the muriatic acid, when we had poured the ammoniacal liquid on the acid, which passed red at the distillation. There was not at first any mixture between the two liquids; but on coming in contact they were observed to separate, and crystallize in irregularly faced pyramids with their base turned upwards. The crystals were very numerous. We expected that by agitation they would have fallen to the bottom of the liquid, but it made them disappear; some new alkali reproduced them, and that until the ammonia was in excess;—the colour of the acid, far from disappearing, became livelier. We repeated this experiment several times, and always with the same result. We made the experiment each time with three or four ounces of liquid. Thus, in that operation which without contradiction had been the simplest and the easiest, our attempt was unfortunately abortive.

We made an experiment on the volatility of iodine in air: five grains of crystal of that substance were placed in the morning on the reverse of a cup;—towards evening a great part had been already dissipated; and next morning the whole had disappeared. The temperature of the apartment varied from 8° to 10° R., and the odour of chlorine continued for several days.

Two grains of iodine placed in the palm of the hand, dissipated in less than twelve minutes; the spot remained much longer. It was when at another time making the same experiment, with the view of ascertaining the identity of the odour of iodine with that of chlorine, that we discovered the property which the former substance has of colouring starch blue. A napkin passed through starch, with which we wiped our hands, was covered with large blue spots, which by little and little became violet, and ultimately disappeared. Since then we have employed as a reagent for iodine small strips of cloth steeped in starch water.

When

When the existence of iodic acid is suspected in a salt, add to a solution of this salt a little of the water of chlorine; a reddish-brown colour will immediately appear, from the iodine dissolving in the muriate; by adding a little starch-water, a deep blue colour will be obtained: if an excess of chlorine is added, the red-brown colour disappears, and the starch-water is no longer coloured blue;—the water thus coloured, loses itself all its colour by the addition of a little water of chlorine. The very marked development of the muriatic acid denotes that the oxygen passes to the iodine. When in these experiments the iodine is in a greater quantity than the starch, the colour is of a deep cærulean blue;—if, on the contrary, the starch is in excess, the colour is of a brownish black; and at the point of saturation it is a beautiful violet. A particle of water containing iodate, dropped on paper which has been starched and placed above a bottle containing chlorine, produces instantly a beautiful blue spot.

Such have been hitherto the results of our experiments, which we purpose yet to extend as far as the object will allow.

XXXI. *Present State of the Ruins of Babylon. By Captain* EDWARD FREDERICK*.

THE interesting descriptions given by Captain F. will doubtless have great future importance in guiding travellers to the site of these famous ruins of the East, the way to which he shows very accurately. He observes, "that the ruins of the mounds lie on the left, a short distance off the direct road from Hillah; and a traveller merely sees Belus's tower as he rides along, and must turn out of his way if he wishes to examine it, which will occupy a longer time than the travellers generally have leisure for, as appears from their own acknowledgements, not to notice their dread of being surprised by the wandering Arabs.

"As to the other travellers who have visited this celebrated spot, it would be carrying complaisance too far to place implicit confidence on their relations, as they appear merely to have passed over the ground, and sometimes not even to know that they were amidst the ruins, until their guides told them it was Babel they were riding over. They of course had no time to examine the heaps of rubbish. Other travellers visited only one bank of the Euphrates, not caring to risk meeting with the Arabs while gratifying their curiosity on the other. From Belus's tower (which is four miles from Hillah in a direct line) there are no more

* From the Transactions of the Literary Society of Bombay.

mounds on the bank of the river for the distance of twelve miles above the tower, when you are shown a small heap of white and red furnace-baked bricks, called by the Arabs the hummum or bath. I strongly suspect this to be the remains of a modern building, from the size, colour, and general appearance of the bricks, which, in my opinion, bear not the slightest resemblance to those I had previously seen. This spot, I should imagine, had not been visited by any traveller, as it lies at a great distance from the main road from Hillah to Bagdad; indeed, no one mentions ever having seen it.

"These are all the mounds, or ruins, as they are called, of Babylon, that are genreally shown to travellers under the general denomination of Babel. I however discovered, after much inquiry, that there were some heaps on the right bank, at the distance of some miles from Hillah, between the village of Karakoolee and the river. I accordingly rode to them, and perceived that, for the space of about half a mile square, the country was covered with fragments of different kinds of bricks; but none of them led me to conclude that they were of the same size and composition as those found either at Belus's tower, or the mound mentioned to be situated between it and Hillah; I therefore returned, somewhat disappointed.

"Having now gratified my curiosity in examining every mound or spot described either by Rennell, or pointed out by the natives as belonging to Babel, I next began to search for the remains of the ditch and city-wall that had encompassed Babylon, which was the principal object of my journey, and still remained to be accomplished. Neither of these have been seen by any modern travellers, nor do they give any intimation that they had even looked for them. All my inquiries amongst the Arabs on this subject completely failed in producing the smallest effect. Desirous, however, of verifying the conjectures of Major Rennell, I commenced my search, first by riding five miles down the stream, and next by following the windings of the river sixteen miles to the northward from Hillah, on the eastern side of the river. The western I ranged exactly in the same manner, and discovered not the least appearance or trace of any deep excavation running in a line, or the remains of any rubbish or mounds that could possibly lead to a conclusion that either a ditch or wall had existed within the range of twenty-one miles. On the western bank, in returning home, I left the winding of the river, and proceeded in a straight line from the village of Karakoolee, fifteen miles to the northward and westward of Hillah, to the latter place. The next day I rode in a perpendicular direction from the river at Belus's tower, six miles east and as many west; so that, within a space of twenty-one miles in length, along the banks of the

Euphrates,

Euphrates, and twelve miles across it in breadth, I was unable to perceive any thing that could admit of my imagining that either a wall or ditch had existed within this extensive area. This leads, however, only to this conclusion;—that, if any remains do exist, they must have been of greater circumference than is allowed by modern geographers. I may possibly have been deceived, but I spared no pains to prevent it; I never was employed in riding and walking less than eight hours a-day for six successive days, and upwards of twelve on the seventh.

"That part of the Euphrates which lies between Karakoolee and Hillah, a distance of upwards of sixteen miles, winds extremely, and particularly where it passes Belus's tower a quarter of a mile distant. Arguing from the well established fact, that streams, on so soft a bottom and level a surface, in the course of years change their beds, we may, without violating probability, presume that the Euphrates had anciently flowed between Belus's tower and the other large mound lying about three quarters of a mile to the west of it, mentioned in this account as the one with the walls of a large house still standing in it, and the decayed tree; for, where the remains of the palace could have been situated, if not at this mound, I am at a loss to conjecture. But if we admit that the river may have changed its course from what it held in those ancient times, and that it now flows to the westward of both the palace and the tower, instead of passing between them, as it is said to have done, the positions of the palace and tower are then exactly marked by these two mounds; for, with the exception of Niebuhr's watch-tower, there is not a single mound on the western bank to be found, nor do the natives ever procure any bricks from that side, though the principal part of the town of Hillah is situated on it. If this conjecture be admissible, then the ancients and moderns agree in their accounts of this far-famed city with regard to the site of its two principal edifices; but if it be rejected as improbable, we still remain as much in the dark as ever, when we come to look for the remains of the palace. I shall however lay no stress upon what I have here advanced, but only offer it as a conjecture that struck me as probable, from the modern appearances of the river, ruins, and country in their vicinity, at the time I was examining them."

The author having taken his survey in every thing worthy of notice, concludes with equally important observations on the probable dimensions of the Babylonian tower, and the several kinds of bricks found; and lastly, notices the navigation of the country.

"Della Valle and Beauchamp make the square of the tower of Belus from six hundred and forty to six hundred and sixty feet.

I paced the circumference, and found the four faces amount to nine hundred paces, or 2,250 feet: the slope, as you descend the face, is gradual, and generally easy. We might not have measured it exactly at the same place; but the difference which appears between us is immaterial, as a lapse of two centuries may in all probability have occasioned considerable alterations. The altitude of the south-west angle, which is the loftiest part of the whole, is computed at two hundred feet. I had no means of ascertaining the truth of this, but should imagine it is fully that height. Della Valle mentions two kinds of bricks, furnace-baked and sun-dried; and Beauchamp met only with the former. I saw both these, and another sort of deep-red, apparently high-baked, the colour of an English brick. This latter is in the greatest abundance at Niebuhr's watch-tower, and generally has an inscription on it, but in a small character. I could not procure any of this kind whole; they were always in small pieces. The tower of Belus, the mound opposite to it, and the watch-tower, had these two kinds used in their construction; but the large clay sun-dried brick was to be found only at Bèlus's tower, the whole interior body of which was composed of it; and the employment of reeds and bitumen as a cement, appears to have been but seldom introduced in other parts of the ruins, except at the one denominated the tower of Belus, where it was universally seen as the cement for the sun-dried brick, and at every course; whereas, at Aggurkeef, near Bagdad, which is certainly a Babylonish building, it is found at every sixth, seventh, and eighth course, though the same sort of brick is used in the building. The reeds and bitumen were evidently but seldom used with the furnace-baked, which I observed most generally cemented with a thin layer of lime and sand. The dimensions of the bricks were —clay sun-dried, four inches seven-tenths thick, seventeen inches and a half broad; furnace baked, three inches thick, twelve inches broad, and generally weighed thirty-one pounds.

"The Euphrates, as far as Kerna, which is one hundred and twenty miles from the head of the Persian Gulf, is navigable for vessels of three hundred tons, and from thence to Hillah, boats not exceeding eighty can come up during six months in the year. Their construction is singular: they have one very large mast with a latteen sail; the body almost a half-moon, no keel, and a rudder of the most awkward shape: the hull is extremely ill constructed, the ribs and planks being roughly nailed together, and the outside covered with bitumen. When they are going to Korna or Bussora from Hillah, they sail if the wind be fair, or float down the stream if it be foul. In returning or ascending the stream, they have one end of a long rope tied to the head of the

the mast, four or six men take hold of the other end, and by this means pull her against the current.

"It is curious to observe, notwithstanding the lapse of ages, how some local customs and usages continue in practice. The circular boats made of reeds, and in form of a shield, which attracted the notice of Herodotus so much, and which, in his time, were used on the river between Babylon and Armenia, differ hardly at all from those in use at the present day; which perfectly agree with the description given by that venerable historian. Another curious method of navigation exists in these times, which is noticed as early as the time of Xenophon. Merchants in Armenia, when embarking on the Tigris, collect a great number of goatskins, which, having inflated, they fasten together, forming a kind of square raft; these are from fifty to a hundred in number; over them are placed mats, then the merchandize, and upon the top of all, the owners and passengers. It is then set adrift, and, floating down the stream, it occasionally strikes against islands and shallow parts of the river, the bottom of which being of a soft nature, seldom destroys the skins.

"The flowing of the tide at Korna is a singular sight: it prevails against the stream of the Euphrates, but finds the current of the Tigris too powerful; and, as you stand at the confluence of the two rivers, you see the flood-tide flowing up the Euphrates on the one hand, and forced back by the strength of the Tigris on the other, forming, by this contrary direction of two currents, a violent eddy between them. The tides of the Persian Gulf are sensibly felt in the Euphrates twenty miles above Korna, or one hundred and forty miles from the mouth of the river. The depth of the river at Hillah, from what I could collect from the natives, exceeds forty feet when nearly full: at the time I saw it, the surface of the stream was within three feet of the edge of the bank, and must, I should conceive, have been fully of that depth. It had arrived very nearly at is greatest height, this being the period of its annual swell. It is broader, but not so rapid, as the Dijla or Tigris: that part of it between Karakoolee and the mounds was very narrow: after which, as it approaches Hillah, it widens considerably, and close to the mound it forms a sudden bend, flowing almost between the tower of Belus and the large mound opposite to it; which appearance and formation induced me to hazard a conjecture that it might formerly have passed between them, instead of running to the westward of them both as it now does. The inundations of the river do not tend to fertilize the land; the cultivation is carried on entirely by irrigation, the water being thrown up into a trough by means of a very simple machine constructed on the edge of the bank, and easily worked by by one man; thence it is conducted through narrow channels to any part of the fields. The perpendicular mud pillars upon which the cross-bar rests are about two feet in diameter, and the basket that takes up the water is of an oval form, three feet long by sixteen or eighteen inches broad, made of reeds, and covered with bitumen.

"On account of the decayed state of the water-courses, cultivation is confined to the banks of the river, and the few canals that admit the water at the annual increase of the river:—thus that country, which has been considered the richest in the world, has more the appearance of a desert, than of lands that had formerly yielded four hundred-fold to the industry of the husbandman.

"It is worthy of remark, that after leaving Korna, which is situated forty miles above Bussora, at the confluence of the Euphrates and Tigris, no date-trees are to be seen on the banks of the latter river; and that the sides of the former are lined with them up to Babylon, and even a very considerable distance above it. The date-fruit to the present day constitute so essential a part of the food of the inhabitants, that it may, without any impropriety of either language or ideas, be esteemed the bread of the people; and from it also a fermented liquor is made, into which aniseed is put, to give it a flavour. It is well known that the ancients were not very delicate with regard to the flavour of their wines, and that any fermented liquor passed under that denomination. The Babylonians, however, might have possessed the art of extracting the sap, and making a liquor of it, or a wine, as Herodotus would have called it, by fermentation,—an art which the Arabs of the present day are unacquainted with.

"Hillah, which is in lat. 32° 28′ N., observed by Niebuhr, and said to be built on the site of ancient Babylon, is a good-sized town, containing from ten to twelve thousand inhabitants, with the Euphrates flowing through the midst of it. The two divisions of the place communicate by means of a bridge of boats of a very rude construction, and connected with each other by a couple of large iron chains, and platforms of date-trees, mats, and mud. A great number of date-trees are interspersed amongst the buildings, which, at a distance, give it the appearance of a large town situated in the midst of a grove.

"The road to it from Bagdad is good, and the surrounding country, as far as the eye can reach, perfectly flat, intersected with canals, which had been cut formely across the Jezzera from the Tigris to the Euphrates, but at present they can only be traced by their decayed banks.

"The climate of this country has been considered particularly clear,

clear, fine, and healthy, though extremely hot from April to October; and the water of the Euphrates is held in almost as high estimation at the present day by the Arabs, as that of the Choaspes (the modern Karoon) was regarded by the imperial lords of Ecbatana in ancient times."

XXXII. *Final Remarks, by Mr.* Edward Riddle, *on finding the Longitude by Lunar Observations, &c.*

To Mr. Tilloch.

Sir, — Having now seen both Mr. Meikle's reply and his continuation of it, I take this opportunity of making a few and final observations on what he has advanced in his defence. I shall take his letters in the order in which they are published. First then, for his letter of November 2d: I had observed that he misunderstood the term *parallax*, and in answer to the accusation he has taken the trouble of proving that he does so. His exposition of Playfair's definition would certainly have produced a smile from the venerable philosopher. I defined parallax to be "the angular distance between the places of the centre of a celestial body, as seen at the surface and at the centre of the earth;" or, which amounts to the same thing, "the angle under which the earth's semidiameter would appear if viewed from the centre of the celestial object." Mr. M. asserts that this is a "*counterfeit* definition;" and that it is "*invented to serve a particular purpose.*" It is almost needless to quote authorities on such a subject; but the following extract from Rees's *Cyclopædia*, article Parallax, will show that my definition was neither counterfeit, nor invented to serve a particular purpose: "Parallax is used for the angle made in the centre of the star by two right lines drawn, the one from the centre, the other from the surface of the earth." It is in this sense that all astronomers understand the term. The following extract from Playfair, too, will show how far his notions of parallax agree with those which Mr. M. ascribes to him. "The parallax affects the position of a body only by depressing it in the direction of a vertical plane." —Outlines of Nat. Phil. vol. ii. art. 79. This is the "common popular doctrine," which Mr. M. endeavoured to prove was not true; yet he asserts that Playfair understood parallax in the same sense that he does.

In the example of correcting the moon's altitude, Mr. Meikle says, I have *concealed* "a great deal of needless labour." On this assertion it is sufficient to observe, that I concealed no labour whatever,

whatever, except that of turning over ONE leaf in Mendoza Rios's tables. My object was to show that in finding the *true altitude* of the moon's centre above the sensible horizon, Mr. M.'s method was not wrong only because it was not really different from that commonly practised; and that his method of finding the *apparent altitude* above the same place was *wrong* exactly by what it differed from the method which he was desirous of superseding. In both these respects I succeeded; and setting *experience* aside, it would scarcely have been believed that he would have asserted that his crude misconceptions on this subject were "sanctioned by the authority of Maskelyne, Pond and Brinkley."

On the subject of the quadrant, I pointed out the principle on which Mr. M. proposed that the operation of the instruments should be explained; and I showed that there was nothing vague, unsatisfactory, or incorrect in the explanation which is commonly given of it; and this was all I proposed to effect. Mr. M. is right in observing, that "we can never be too simple in our explanations;" but though his demonstration of the principle of the quadrant is true, it has no very evident pretensions to simplicity.

We have seen that he does not correctly understand what astronomers mean by the term *parallax;* it appears also that he does not know what is meant by the *augmentation* of the semidiameter. Indeed in his letter of November 2d, his misconceptions on this subject have led him to make statements too absurd to be defended; and he has for once, at least, acknowledged that he was in an error. But the merit of this concession is materially lessened by an accompanying circumstance. He does not state the true cause of his falling into the mistake. He affirmed that the *augmentation* was not the same for every semidiameter, and he deduced this curious conclusion very fairly, *on the supposition that the moon is flat*; this was the true cause of his mistake, not that which he thinks proper to assign.

Mr. Meikle is exceedingly out of temper at the diagram on the subject of parallax, and his anger is easily accounted for. That diagram altogether overturns his assertion, that the difference of the parallax of any two diametrically opposite limbs is the augmentation of the diameter; and he cannot good-humouredly look on and see such havoc made among the creations of his fancy. D A F — G B E in that figure is the augmentation of every semidiameter, while the altitude of the object continues the same. But if Mr. M.'s statement on this subject and his interpretation of the term parallax were correct, the diameter parallel to the horizon would have *no augmentation*, as the *difference* of the parallax of those "two diametrically opposite limbs"

limbs" is nothing; and it is this *difference* which, as he says, "constitutes the augmentation." He is, perhaps, not *now* inclined to deduce from this fact so absurd a consequence.

Mackay's mistake respecting the correction of the azimuth arose from his writing the *numerator* in place of the *denominator*, in the analytical expression which furnishes the rule. The whole of the investigation is so simple and so short, that if Mr. M. had it not to seek, I cannot conceive why he should have thought it worth while to announce that it might form the subject of a future communication. Put A = the azimuth, a = the declination, c = the latitude, B = the hour angle, and r = rad. Then Keith's Trig. chap. ix. prop. vii. edit. 2d. $\dot{A} : \dot{a} :: r^2 : \cos c . \sin B$; whence $\dot{A} = \frac{\dot{a} r^2}{\cos c . \sin B} = \dot{a} . \operatorname{sect} c . \operatorname{cosect} B$, radius unity.

So much for the first part of his reply. We come now to the second; and here, in strict accordance with his determination to be *original*, he has adopted a line of defence perfectly new, I believe, in the annals of *printed* controversy. I had proved that all his objections to the common method of finding the distance in lunars were without foundation. He read my observations, and in reply declared, that he had not yet found that in his objections there was "anything amiss;" and that those objections were "truths which rested securely on their own basis." He now comes forward in a letter bearing date December 3, 1819, and says in effect: "Sir, all that you say on the subject of correcting the altitudes and clearing the distance is quite true, *but I never made any such objections as those which you impute to me.*" In answer to this sweeping asseveration, I shall merely contrast Mr. Meikle with himself, and in his own words.

In his letter of December 3d, 1819, page 401 of your Magazine for that month, he says "it is not the method with the *reduced* semidiameter on which I animadverted with so much severity." And again in the same page, "in the common method the reduction of the semidiameter by refraction is neglected altogether, *and it was for this very reason that I animadverted on it.*" And again, "Mr. R. has been at great pains to prove trifles, the truth of which nobody doubts, *and which I never denied.*"

The animadversions to which he refers were printed in your Magazine for last July. Let him state for himself *on what he did there animadvert*. In page 35 of the Number referred to he says, "Some of the greatest efforts to attain exactness are frequently productive of greater errors than those they are intended to remove. Thus where perfection is aimed at an allowance is made for the effect of refraction in diminishing the vertical semidiameters. Sometimes also an allowance is made for the contraction

traction of that semidiameter applied to the distance. This, as we shall afterwards see, is only an elaborate way of creating new errors." This is the main object of his letter, and he subsequently demonstrated a property of the ellipse with which all mathematical men were very well acquainted, and referred to that property as a demonstrative proof that an *augmentation* ought to be applied, not a *deduction* made from the semidiameter applied to the distance. How, in the face of these circumstances, Mr. M. has had the courage to lay before the public the assertions contained in the above extracts from his letter of December 3, I am totally at a loss to conjecture.

He says I "cannot produce the appearance" of his *not knowing* that the distance of the centres is the object of the computation. He is here again wrong, for the task is very easy. I showed that by the method practised by careful computers in correcting the semidiameters applied to the observed altitudes and distance, the true altitude of the centres and their apparent altitudes and distance would be correctly obtained, and that hence the true distance of the centres might be computed; and further, that the true distance of the centres was what was wanted. What Mr. M. *knew* on this subject may be gathered from what he *said*. Take the following extracts from his letter already referred to in your July Magazine. "The centre is not necessarily the apparent place of the angular point of the triangle." "The centre is seldom the point from which the apparent distance should be reckoned." "The *mistake* has no doubt arisen from the old established habit of estimating the apparent distance from the centre." The "appearance" of his not knowing that the distance of the centres is the object of the computation is therefore pretty clearly made out.

How I could insinuate that Mr. M. has "inculcated the observance of such nicety at sea" will appear strange enough to those who consider that the principal object of his letter was to show that the application of these corrections, under any circumstances, was productive of error; and that my object was to defend them from this imputation.

On the subject of "trifles" to which we have seen he has adverted, let us take *his former estimate* of their value. He has told us "that in the common method the reduction of the semidiameter is neglected altogether." And in page 36, July Magazine, In "the method for correcting the altitude for the distortion of the disk, by a table answering to the diminution of the vertical semidiameter, what is gained in getting the true altitude of the centre is lost in departing from the point from which the distance should be taken, and *to which the common way of working often makes a much nearer approach.*" And further, "I do

"I do not consider an error of seven or eight minutes of longitude unworthy of avoiding; and a much greater error may sometimes be produced by neglecting the quantity under consideration." Let these three extracts be compared.

But even supposing the deduction from the vertical semidiameter omitted, as it usually is in the "slovenly" practice of mariners at sea, it is right to observe again that the error of the *correction* of altitude at 6° or 7° is only the change of parallax and refraction corresponding to a change of 18″ in altitude. Mr. M. perhaps knows how small a part of 18″ this change of correction is.

He says in the last place, that I have "reluctantly admitted" that he correctly stated the errors of certain TABLES. Here again is a mis-statement. I pointed out ONE table which had the error which he imputed to several, and I know of no other such erroneous table. He was declaring such tables *generally* to be worse than useless, and he mentioned a specific error that I showed to exist in *one* table, and he has in his last letter again referred to the error in *that table*. If he cannot point out *other tables*, now in use among mariners, which have the "gross error" which is found in that one, he will find it difficult to show that he has not made a disingenuous use of his argument. If he can point out any other such erroneous tables now in use among nautical men, he is entitled to the credit—to the benefit of the discovery.

I am, sir, your obedient servant,

Trinity House School, Newcastle,
8th January, 1820.

EDWARD RIDDLE.

XXXIII. *Notices respecting New Books.*

Lately published,

AN Historical and Statistical Account of the Principalities of Wallachia and Moldavia, with Political Observations. By William Wilkinson, Esq., late British Consul to these Principalities. 8vo.

Brackendridge's Voyage to South America. 2 vols. 8vo.

Moss's St. Saviour's Church. 4to. 1*l*. 11*s*. 6*d*. India proofs, 2*l*. 12*s*. 6*d*.

Vitruvius Britannicus. Imperial folio. vols. 4 and 5, 10*l*. 10*s*.

In the Press.

Mr. James Grey Jackson, late British Consul at Santa Cruz, South Barbary, and resident Merchant upwards of sixteen years in in various parts of the Empire of Marocco, Professor of Arabic, and author of an Account of the Empire of Marocco, and the Districts of Suse, Tafilelt, Timbuctoo, &c. has in the press, An Account of Timbuctoo and Housa, Territories in the Interior of Africa, by El Hage Abd Salam Shabeenie, a native of Marocco, who personally visited and resided as a Merchant in those interesting Countries. With Notes critical and explanatory.

To which will be added, Letters descriptive of several Journeys through West and South Barbary, and across the Mountains of Atlas (a Journey never before nor since performed by any European), personally performed by Mr. Jackson between the years 1790 and 1805. Also, his Translations of several very interesting Letters in the original Arabic, from Muhamedan Potentates to Christian Kings, exemplifying the peculiar Phraseology of that oriental Language; African Anecdotes, Fragments, &c.

A Tour through Normandy; illustrated by numerous Etchings of Antiquities, and other interesting Subjects. By Dawson Turner, Esq.

Illustrations of Cases of *Tic Douloureux* successfully treated. By Mr. B. Hutchinson, Member of the Royal College of Surgeons.

Preparing for Publication.

Annals of Oriental Literature. To be published quarterly, the first number to appear on the 1st of May. 6*s*.

A Mineralogical Dictionary, comprising an Alphabetical Nomenclature of Mineralogical Synonymes, and a Description of each Substance.—To which is prefixed an Explanation of the Terms used in describing the external Characters and crystalline Structure and Forms of Minerals.

This work will be illustrated by numerous plates. Many of those relating to the Theory of Crystallography are entirely original. —The whole to be engraved by Mr. and Miss Lowry.

Travels in various Countries of the East; being a Continuation of Memoirs relating to European and Asiatic Turkey, &c. Edited by Robert Walpole, M.A. Containing the last Travels of the late Mr. Browne; a Biographical Memoir of him; a Journey from Suez to Sinai; an Account of some remarkable Monuments of Antiquity discovered at Susa, in Persia; Travels in Syria, Asia Minor, Greece, and the Archipelago; with remarks on the Antiquities, Natural History, Manners, Customs, &c.

The History of the Anglo-Saxons, from their first Appearance in Europe to the End of their Dynasty of England; comprising the

the History of England from the earliest Period of the Norman Conquest. By Sharon Turner, F.A.S. The third edition, corrected and improved. In 3 vols. 8vo.

An Essay on Croup; illustrating a new and successful Mode of treating that Disease. By Mr. Asbury, Surgeon, Enfield.

An Inquiry into the Nature and Medical Treatment of Diseases connected with a deranged Action of the Urinary Organs, particularly Gravel and Calculus. By Dr. Prout.

Dr. Barrow's Inquiry respecting the Origin of Tubercles and Tumours. In 4to. with illustrative Engravings.

XXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 17. Concluded reading of paper On some new Combinations of Platina, by Edmund Davy, Esq.

24. Read a paper On the Methods of cutting Rock Crystal for Micrometers, by Dr. William Hyde Wollaston.

March 9. Read a paper On a new Principle of constructing Ships in the Mercantile Navy, by Sir Robert Seppings; also another On a Peculiarity in the Structure of the Eye of the Balæna Mysticetus, by J. A. Ransome, Esq.

16. A paper was read On the Law of the Variation of the Flexibility of the Canadian Fir, by M. Charles Dupin.

ASTRONOMICAL SOCIETY.

The first meeting of this Society was held on the 10th instant, at the house of the Geological Society, Bedford-street, Covent Garden; and was very numerously attended. A paper, by the Rev. Dr. Pearson, was read on the subject of a new *Micrometer*, which he has invented for measuring small distances in the field of a telescope. It is founded on the doubly refracting property of rock-crystal, and promises to be a great acquisition to astronomical instruments,. Several valuable works on the subject of astronomy were presented to the Society, as the foundation of a Library: and many new members were proposed.

Officers for 1820.

President.—His Grace the Duke of Somerset, F.R.S. & S.A.

Vice-Presidents.

H. T. Colebrooke, Esq. F.R.S. & L.S.	Sir Wm. Herschel, LL.D. F.R.S.
S. Groombridge, Esq. F.R.S.	J. Pond, Esq. *Ast. Roy.* F.R.S.

GEOLOGICAL SOCIETY OF LONDON.

From the Report of the Council to the General Meeting, 4th February 1820, it appears that the prosperity which has hitherto attended the exertions of the Society, continues undiminished: the number of the members is still increasing; the expenditure of the last year has fallen within the income; the geological collections have been considerably enlarged; the supply of communications has been constant; and many gratifying proofs have been afforded of the high estimation in which the Society is held both at home and abroad.

The annexed statement will show the number of members, according to the different classes, at the last Anniversary Meeting and the present.

	5th Feb. 1819.	4th Feb. 1820.	Increase.	Dec.
Ordinary Members having compounded	23 ..	26 ..	3	..
——— contributing	182 ..	183 ..	1	..
——— non-resident	109 ..	115 ..	6	..
Honorary	82 ..	79 ..	..	.. 3
Foreign	37 ..	39 ..	2	..
	433	442	12	3

The income of the Society during the last year was 946*l.* 11*s.* and the expenditure 798*l.* 16*s.* 10*d.*

In consequence of the œconomy by which the expenditure has hitherto been regulated, the Society has been compelled to forgo many accommodations, which, if not essential, are at least extremely desirable. The want of cabinets for the display of the foreign specimens, and the defective state of the library, and collection

lection of maps, have been more particularly felt and regretted: but there is great reason to believe that these desiderata, at least, will be speedily supplied. The public spirit of certain members has induced them to open a subscription, which already amounts to 600*l.* as the basis of a fund applicable to the purchase of new cabinets and of the most useful books and maps. In justice to the liberality of the subscribers, and for the information of those who may wish to co-operate with them, the Council thought it proper to state this circumstance to the Annual Meeting, observing, at the same time, that it is the wish of the parties with whom the subscription originated, that it should be, in the strictest sense of the word, voluntary; and that the only invitation to members to contribute to the fund in question should be the knowledge now communicated to them that such a fund is in existence.

During the preceding year, the following papers have been read at the meetings of the Society:

A concise Account of the calcareous Rocks of Plymouth, containing Remarks connected with, and Illustrations of, their Natural History. By the Rev. Richard Hennah.—An Account of the Rocks, with their Fossils, of the Coast extending from Bridport Harbour, Dorset, to Hope's Nose, the eastern Point of Torbay, Devon. By H. J. De la Beche, Esq. M.G.S.—Observations on a new Pentacrinite from Lyme, a new Encrinus, and a Briarean Pentacrinite. By Geo. Cumberland, Esq. Hon. Mem. G.S.—Description of the Strata in the Brook Pulcovca, near the village of Great Pulcovca. By the Hon. W. T. H. F. Strangways, M.G.S.—Extract of a Letter to G. B. Greenough, Esq. on the Island of Barbuda, in the West Indies. By Dr. Nugent, Hon. Mem. G.S.—On the Geological Situation of the Ryegate Firestone, and of the Fullers'-Earth at Nutfield. By Thomas Webster, Esq. Sec. G.S.—Geological Description of the Valley of the Ligovca. By the Hon. W. T. H. F. Strangways, M.G.S.—Description of the Rocks which occur along a Portion of the Coast of the Isle of Mull. By the Right Hon. Earl Compton, Pres. G.S.—On the Smelting of Tin Ores in Cornwall and Devonshire. By John Taylor, Esq. Treas. G.S.—Extract of a Letter from D. Scott, Esq. Commissioner at Cooch Behan, communicated by H. J. Colebrooke, Esq. V.P. G.S.—On the Geological Relations of the Environs of Tortworth, and of the Mendip Range in Somersetshire. By Thomas Weaver, Esq.—Some additional Remarks upon the Fossils in the Vicinity of Lyme Regis, Dorsetshire. By H. T. De la Beche, Esq. M.G.S.—A Sketch of the Geology of Antigua, accompanied by a coloured Map, sectional Views, and a Collection of Specimens. By Nicholas Nugent, Esq. Hon. Mem. G.S.—Notice respecting the Tottenhoe Stone. By

Edward Hanmer, Esq. M.G.S.—Description of Specimens of Rocks collected on the March of a Detachment commanded by Colonel Hardyman, through the District of Bogela in Bigelkund and Gurrah, in the Province of Malwich, by Captain R. Lachlan, of H. M. 17th Regiment of Foot; communicated by Col. Thomas Hardwicke, F.R.S.—Account of a Variety of Limestone found in Connection with the Clay Ironstone of Staffordshire. By the Rev. John Yates, M.G.S.—On the Chemical Analysis of the Clinkstone of Hohentweil. By Professor Gmelin, of Tubingen.—Extract of a Letter from D. Scott, Esq. Commissioner at Cooch Behan, to Dr. Wallich. Communicated by H. T. Colebrooke, Esq. V.P. G.S.

The first part of the 5th volume of the Transactions of the Society has been published. The Geological Map of England, by Mr. Greenough, is now ready for publication; the expenses incident to the undertaking, which, it is supposed, will eventually amount to 1700*l.*, have been defrayed, not from the funds of the Society, but by the voluntary contribution of individuals, who have engaged to advance the entire sum required, upon condition of being repaid out of the first proceeds of the sale; while the loss will be theirs, if the proceeds should fall short of the sum advanced, and the profits, if any, will belong to the Society.

At the General Meeting held on the 4th February, the thanks of the Society were voted to

G. B. Greenough, Esq. .. President, going out by rotation.
Rev. W. Buckland .. .. } Vice-Presidents, also going out
Sir Henry C. Englefield, Bart. } by rotation.
John Bostock, M.D. .. retiring from the office of Secretary.

OFFICERS for 1820.

President.—The Right Hon. Earl Compton.

Vice-Presidents.

The Most Noble The Marquis of Lansdowne, F.R.S.
Hon. W. T. H. F. Strangways.
Henry Thomas Colebrooke, Esq. F.R.S. M. Asiat. Soc.
John MacCulloch, M.D. F.L.S.

Secretaries.

Alexander Henderson, M.D.
Mr. Thomas Webster.

Foreign Secretary.—Henry Heuland, Esq.

Treasurers.

Daniel Moore, Esq. F.R.S. S.A. & L.S.
John Taylor, Esq.

Council.

Council.

W. H. Pepys, Esq. F.R.S.
Hon. Hen. Grey Bennet, M.P. F.R.S.
Hen. James Brooke, Esq. F.R. & L.S.
G. B. Greenough, Esq. F.R. & L.S.
W. Somerville, M.D. F.R. & L.S.
The Rt. Hon. Sir J. Nicholl, Knt. F.R.S.
Richard Bright, M.D.
Thomas Smith, Esq. F.R. & L.S.
Charles Stokes, Esq. F.A. & L.S.
Henry Warburton, Esq. F.R.S.
John Bostock, M.D. F.R. & L.S.
William Hyde Wollaston, M.D. F.R.S.

Keeper of the Museum and Draughtsman.
Mr. Thomas Webster.

BRISTOL LITERARY AND PHILOSOPHICAL INSTITUTION.

On the 29th of February the ceremony of laying the foundation-stone of a new and magnificent building for literary and philosophical purposes in Bristol, was attended by the Right Worshipful the Mayor, Wm. Fripp, jun. Esq., the Sheriffs, and a numerous assemblage of gentlemen, some of the most distinguished for wealth or talent in Bristol. The company met their worthy chief magistrate at the Council House, and thence proceeded in procession, with a band of music and the insignia of the city, to the ground; and afterwards returned in a similar order to the Merchants' Hall to dinner; where they spent the evening in the greatest harmony and unanimity.

The site of this building is at the west end of the bottom of Park-street, one of the finest streets in Bristol. It is intended for the building to "contain a spacious lecture room, with a laboratory adjoining; a room of noble dimensions destined for a library; two apartments which may be appropriated, the one for an exhibition room, the other for a museum; a reading-room for reviews, pamphlets, newspapers, &c.: some other apartments for subsidiary purposes, and for the accommodation of a resident guardian of the building."

It has been for several years in contemplation to form a philosophical society in Bristol, after the example of London, Edinburgh, Liverpool, Dublin, and some other great towns of the empire; but from the intervention of some cause or other, circumstances have continually occurred to delay the execution of so desirable an object. There is now, however, but little doubt, from the zeal which is manifested by the inhabitants of Bristol for adding so useful an institution to the city and so great an ornament to its taste and opulence, that what the friends of this

institution

institution have so long, so sedulously, and so laudably been endeavouring to effect, will be attended with the completest success. It is unnecessary to enter into a detail of the advantages to society, commerce, and the arts, which have uniformly been derived in other places from establishments of this kind; they are too familiar to every well-informed mind to need any comment or observation. Justice, however, requires it should be known, that the patrons of this institution have formed their plans upon the broadest basis of enlightened liberality. Besides the cultivation and diffusion of the nobler sciences, and the prosecution of whatever is likely to be of real benefit or utility to the community and the rising generation, they intend to make this institution a focus, in which to collect and concentrate, not only the scattered rays of the genius and ability of Bristol, but also of all true lovers of scientific pursuits; to confine their patronage to no particular branch or branches of the sciences, but to extend and afford the utmost encouragement for the development of talent in every department of useful knowledge and literature.

XXXV. *Intelligence and Miscellaneous Articles.*

CONICAL BALLS.

To Mr. Tilloch.

Woolwich, March 7, 1820.

SIR, — AT p. 147 and 148 of your last Number, are some useful remarks on conical balls, to which may be added the following correct observations, viz. That about the year 1780, Dr. Pollock, then Professor of Fortification at the Royal Military Academy at this place, invented and proposed to the Board of Ordnance a new kind of cannon shot called pear-shot, being very thick at one end and small at the other, like some kinds of pears, and which might as well have been called conical shot, as they were very nearly in the shape of a cone. These shot, he asserted, would always proceed in their flight with the heavy end foremost. By order of the Board of Ordnance, experiments with these shots were performed at Landguard Fort for several weeks; when the only result was, that they ranged further than the globular or round shot, with the same gun, and equal charges of powder; which was owing to the circumstance of the former being heavier than the latter, and so better overcoming the resistance of the air: a property not then known, or not well understood. It did not then appear whether they flew with the heavy end foremost: but soon after they were further tried in Woolwich Warren, by discharging them against large screens of thin deal boards placed at several distances behind each other; when it was found that the

the shots tumbled over and over in their flight, as appeared by the shapes of the holes made in the screens being all different; some round, others oval, and that with different degrees of obliquity.

Soon afterwards, Mr. Anderson, Professor of Natural Philosophy in the University of Glasgow, came up to Woolwich with other long shot a little varied, being like an egg, with a perforation through from end to end, which by threading the air, as he expressed himself, would contribute to keep the larger end going foremost. But on the trial, the failure was just the same as the former. And no doubt the same effect would attend the Russian shot also, if they were properly tried, as well as all other like conical shot. T. B.

PREPARATION OF PRUSSIATE OF AMMONIA AND OF IRON, WHEN EMPLOYED AS A REAGENT FOR COPPER. BY M. BRANDENBURGH.

The most sensible reagents for copper are without contradiction the prussiates of alkali and of iron, particularly that of ammonia. This salt occasions in the nitrate of ammonia an abundant white precipitate, which is of a very beautiful red when accidentally an atom of copper is found in the solution. To prepare this prussiate, I pour into a phial of the capacity of six ounces, three ounces of caustic ammonia, upon half an ounce of the finest and purest prussian blue reduced to a very fine powder. I stop up the phial well, and leave the mixture to macerate in the cold for several days, taking care to shake it from time to time. If I then find that the deposited matter is become brown, I add a new quantity of blue, and repeat this addition until the colour no longer changes. I filter the matter through paper, and pour by little and little on the residuum an ounce of water, in order to separate all the salt. The filtered liquor is prussiate of ammonia and of iron; it has a beautiful yellow colour and a particular odour.

I have observed that this prussiate is also the most sensible reagent for iron; it is even infinitely preferable to the prussiate of potash and of iron.—*Ann. Gen. des Sciences Physiques.*

ANALYSIS OF RED RAIN WHICH FELL AT BLANKENBERG NOVEMBER 2, 1819. BY MESSRS. MEYER AND STOOP, CHEMISTS AT BRUGES.

[From *Annales Generales des Sciences Physiques.*]

On the 2d of Nov. 1819, at half-past two in the afternoon, the wind being westerly, the heavens clouded, the air calm and humid, there fell at Blankenberg for the space of a quarter of an hour

hour an abundant rain of a deep red, which, insensibly resuming its ordinary colour, continued during the rest of the day.

This extraordinary phænomenon, announced at Bruges by persons deserving of every credit, fixed our attention; we procured a quantity of the water, which we submitted to analysis on the 5th and 6th, four days after it fell, and the following were the results:

One hundred and forty-four ounces of this water, perfectly transparent, of a rose colour slightly approaching to violet, subjected to the action of heat and evaporated to four ounces, became of a brick-red colour, and did not yield on cooling any precipitate.

Experiments in the usual way have showu that before and after evaporation this water was neither acid nor alkaline.

By the addition of sulphuric acid a very sensible disengagement of chloric acid was manifested.

A solution of nitrate of silver produced a white precipitate insoluble in boiling water, which was recognised after being decomposed to be a chloruret of silver.

Mixed with deuto-nitrate of liquid mercury an insoluble white precipitate was obtained, which, by decomposition, we found to be proto-chloruret of mercury.

Mixed with hydro-sulphuret of potash we obtained a black precipitate, which, submitted to the action of heat, became reduced to a metallic state.

The liquor which by the addition of nitrate of silver had precipitated the chloruret of silver, mixed with the hydrate of deutoxide of potash, gave a precipitate of a purple colour, which reduced in the ordinary way furnished three grains of a hard brittle metal of a grayish white, attractible by the loadstone, and which mixed with subborate of soda gave us glass of a beautiful blue.

From the above experiments it is established—1st. That the acid obtained is chloric acid. 2d. That the metal is cobalt.

We were not able to obtain more than about two ounces of pure water, collected from the first shower: it differed from the water on which we experimented in this—that it was much deeper in colour; and that with the help of a microscope living animalcules were discovered in it, which altered nothing of its transparency, and which proceeded doubtless from the vessels in which it had been collected. Some characters which we traced with this water, after ascertaining that it contained muriate of cobalt, and formed thus a sort of sympathetic ink, were faintly visible.

Bruges, Nov. 25, 1819.

TEST FOR OLIVE OIL.

The new process for detecting adulteration o olive oil is founded on the property which the solution of acid per-nitrate of mercury

mercury possesses of congealing and solidifying, in a few hours, pure olive oil shaken with it; while it hardly alters the liquidity of the seed-oils, but gives them an orange hue, and causes them to deposit a quantity of precipitate, which never acquires the hardness of the coagulum produced with the olive oil. The pernitrate of mercury employed in this process, is prepared by dissolving, without heat, six parts by weight of mercury in seven parts and a half of nitric acid, of about 38° of Reaumur's aerometer, during the action of which the saline solution remains fluid, the excess of acid preventing its crystallization.

NORTH-WEST PASSAGE.

[From the Edinburgh Magazine and Review, vol. v. pages 141 and 142, for May 1776, as edited by the celebrated GILBERT STUART, now very scarce, that volume having been mostly destroyed when a printing-office took fire.]

The last undertaking to find out a north-west passage (without the assistance of Government) was above 80 years ago, when Arthur Dobbs, Esq. a gentleman of letters and fortune, and a member of the Irish House of Commons, drew up reasons for a passage to the South Seas by the north-west parts of Hudson's Bay. The reasons that determined him to think that a passage was obtainable about the north-west part of Hudson's Bay, near lat. 64. were 1st. That by all accounts, the coast in those parts was broken land, and islands with large openings between them. 2dly. That there were strong tides from the west and north west. 3dly. That black whales were seen in great plenty, which must come from some western ocean, not being found in any other parts of the bay.

Mr. Dobbs showed his MS. to Sir Charles Wager, then first Lord of the Admiralty, who seemed satisfied of the probability of the discovery, and the prospect of attempting it, and mentioned the Hudson's Bay Company as the properest body to be consulted; but they replied they had lost two sloops they had fitted out in the year 1719, under one Bailon, and seemed quite unconcerned about any discovery that might be made.

Mr. Dobbs however got a view of their charter; where he found their privileges so extensive, that they alone would be the gainers should the attempt succeed, and consequently thought it highly imprudent to go on his own bottom. He therefore waited on their governor, Sir Bibie Lake, and showed him his MS., and assured him that one or two sloops would be sufficient, and to be sent from Churchill to the Welcome, to try the tides; and as the distance was but about 140 leagues, it might be sailed in four or five days, and that, if they met a flood of tide, it would ascertain a passage. The sloops were sent out, and on their return Sir Bibie

Bibic wrote Mr. Dobbs, that they had gone no further than latitude 62. and a half north, and returned without making any discovery. Though the persons who commanded the sloops were no ways qualified for the undertaking, yet it appeared from the report they gave, there that was every reason to believe a passage might be found.

Mr. Dobbs wrote, desiring to see the journals of the sloops; but received only a general answer, whence he concluded that the company were averse to making any discoveries, though their charter was granted for that purpose. He observed that it was very odd that in such a season they had not reached lat. 66. N. since the whale fishers every year go as far as lat. 78 and 80 degrees west, without obstruction. Mr. Dobbs on this, thinking that the Company trifled with him, wrote to Sir Charles Wager, from whom he received the following letter: and thus the matter rested.

Admiralty Office, March, 1738.

Sir,—I received the favour of your letter of the 20th past. I believe you judge very right, that the Hudson's Bay Company do not desire to have any body to interfere with them in the fur trade in those parts. They seem to be content with what they have; and make, I believe, more considerable profit by it, than if it was further extended, which might be the case if a further discovery was made; for, although they should not find a navigable passage through into the south, they might probably find Indian nations, from whom furs might be bought cheaper than they can be bought in Hudson's Bay; which would be a disadvantage to their trade.

The probability of finding a passage, as you propose, seems to be very strong. The flood coming that way is almost a demonstration. If a passage could be found into the South Sea, it would open a very large field, and probably of very profitable commerce.

I think the best way to undertake such a discovery is, to have, as you propose, two proper vessels to go at a proper time of the year, and to winter there, if necessary, and to carry with them a small cargo of goods proper to trade with any Indians they may meet with, and capable honest people to be employed in the expedition, if such are to be found, which I very much doubt. As to vessels being sent at the public expense, though it would not be great, yet the Parliament may think, especially at this time, that we ought not to play with the money they give us for other and particular services. However, if Sir R. Walpole, or other proper persons, should think that the Government should attempt it at the public expense, I shall not be against it.

I am, sir, yours, &c.

(Signed) Charles Wager.

And

And it is added, Unless the Hudson's Bay Company have altered their minds, it is easily to be seen what assistance they will lend to any adventurers who may be inclined to attempt a passage to the Southern Ocean, which, if effected, would certainly open a new mine of wealth to this country, by finding a vent for all sorts of its manufactures.

NEW ISLANDS DISCOVERED.

Extract of a letter from Around S. de Peyster, on a voyage from Valparaiso to Calcutta:—"On the evening of the 17th of May, 1819, one of the people discovered a large fire. We hove-to until day-light, when another small low island appeared about five miles under our lee; we passed it close; it appeared clothed with cocoa-nut trees, and doubtless inhabited. It is singular that Commodore Byron continued in the same parallel of latitude until within a short distance of the easternmost group (for so at day-light they had proved to be) and then steered to the northward; and Capt. Wallis passed about the same distance from the westernmost. To the former I gave the name of Ellice's Group, in honour of my friend Ellice, and to the latter my officers and passengers gave the name of De Peyster's Islands.—Ellice's Group lies in long. 180. 54. W. lat. 8. 29. S.; De Peyster's Islands, 181. 43. W. lat. 8. 5. S.; discovered on the 17th and 18th of May, 1819. The longitude is deduced from lunar observations taken at the time, and two chronometers exactly agreeing. I am thus particular, for the information of any who may follow this track from Chili to India."

EARTHQUAKES IN 1819.

Jan. 8, 1819. At Genoa; the people fled into the country.

Feb. 24, in the night. Canton of Tessin, near Morbio.

——— also in the night. Palermo; several shocks, a number of houses overturned.

26. Rome, Frascati, and Albano; shocks in the direction from S.E. to N.E.

Latter days of February. Syria; shocks, very strong.

Feb. 28, in the night. Teflis in Georgia; shocks preceded by a subterraneous noise; several old houses destroyed.

March 28. Cran and Mazera; shocks in succession for an hour; houses tumbled down, and numbers of the inhabitants buried in the ruins.

April 3, 4, 11. Capiapo in Chili; three awful shocks, which totally destroyed that city; only 3000 persons were able to save themselves in the surrounding plains.

8. Temeswar in Hungary; three shocks.

10. Landshut, Germany; slight shock.

May 26, six P.M. Corneto in Italy; many houses thrown down; a number of persons killed.

27, one

27, one A.M. Sicily; violent shock. Etna, which for eight years has been in a state of profound tranquillity, appeared all in flames, and a considerable eruption commenced.

June 16. Kutch County, East Indies; the town of Booj and the fort of Booj overturned; 2000 inhabitants buried under the ruins. Three days after the first shock oscillatory movements of the ground were felt from hour to hour; a volcano burst out at ten leagues from Booj.

July 10, ¾ past six P.M. Gueraude (Loire Inferieure); slight shock in the direction from the North to South; noise similar to that of distant thunder.

End of July. Olette (Eastern Pyrenees); slight shocks.

Aug. 12, ½-past two A.M. Trinidad; violent shock in the direction of from East to West; duration four or five seconds. A considerable noise preceded the shock.

15. Village of St. Andrew, Lower Canada; shock accompanied with a strong explosion.

— Venice.

29, 31. Sweden and Norway; slight shocks.

Sept. 4, nine o'clock P.M. Corfu; two violent shocks in a direction towards the North; all the clocks of the town were set a-ringing by the effect of the oscillations.

Oct. 16, one o'clock A.M. Martinique; the duration of the shocks more remarkable than their force; no accident.

Nov. 28, ½-past one in the morning. Comrie, Perthshire; strong shock, accompanied with a noise similar to that of distant thunder; duration ten seconds.

EARTHQUAKE IN SCOTLAND.

The following account of an earthquake recently experienced in Scotland is given in a letter dated the 4th March, from Glasgow, at which place it was particularly felt:—" About half-past eight in the morning of the 22d ult. after a sudden thaw had begun to succeed a frost unprecedented for duration and intensity in this country for six years, a rumbling noise, proceeding from a northern direction, was heard, which lasted about three seconds, and was immediately followed by a tremulous heaving of the earth, passing apparently towards the south. Scarcely had this first shock been observed, and while I was still giddy with its stunning effects, when another, and immediately a third, quickly succeeded; the last so smart, that the bell in our town-house steeple was distinctly heard to ring. The inhabitants were so alarmed, that many who were in bed ran into the street, and jostled one another, quite stupefied with the concussions, while the omens of clashing doors and ringing bells terrified those within. The waters of Loch Lomond (north of Port Glasgow) experienced, about the

the same time, a partial rise, or agitation, and some persons crossing in a small boat were terrified by the sudden rippling of the water. Our elegant spire was injured by the same shock which made the bell sound, and it was found that it had been pushed considerably off the perpendicular, which may be detected by a stranger on the most cursory glance."

CROCODILE'S FLESH AN ARTICLE OF FOOD.

At Sennaar crocodiles are often brought to market, and their flesh is publicly sold there. I once tasted some of the meat at Esne, in Upper Egypt; it is of a dirty white colour not unlike young veal, with a slight fishy smell; the animal had been caught by some fishermen in a strong net, and was above twelve feet in length. The Governor of Esne ordered it to be brought into his court-yard, where more than a hundred balls were fired against it without any effect, till it was thrown upon its back, and the contents of a small swivel discharged in its belly, the skin of which is much softer than that of the back.—*Burkhardt's Travels.*

LIST OF PATENTS FOR NEW INVENTIONS.

William Collins, of George-street, Grosvenor-square, for useful additions to and improvements on carriage and other lamps.—10th March, 1820.

William Pritchard, of Castle-street, Borough of Southwark, and Robert Franks, of Red Cross-street, London, for their improved method of manufacturing water-proof hats, to be made of silk, wool, or beaver, or other fur, the brims of which are perfectly water-proof, and will in all weathers and in every climate preserve their original shapes, being stiffened without the use of glue or any other material which would prevent the effect of water-proof mixture.—18th March.

Frederic Mighells Van Heythuysen, of Sidmouth-street, parish of St. Pancras, Middlesex, for a method of making portable machines or instruments to be placed upon a desk or table, and so contrived as to fold or not into a small compass made of wood, brass, or other metal, to support a silken shade for the purpose of protecting the eyes from the strong light; added to which is a green, blue, or other coloured glass, in a frame, and in such a position that when placed opposite a window, lamp, or candle, it will take off the glare of white paper, by shedding a green or blue, or any other tinge dependent upon the colour of the glass. —18th March.

Abraham Henry Chambers, of Bond-street, Middlesex, for his improvement in the preparing or manufacturing substances for the formation of the highways and other roads, which substances when so prepared are applicable to other useful purposes.—18th March.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1820.	Age of the Moon.	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Jan. 15	new	8 A.M. 10 1 P.M. 27·5	29·72	Cloudy
16	1	25·	29·72	Very fine
17	2	35·	29·56	Cloudy
18	3	33·	29·40	Ditto—rain at night
19	4	41·	28·74	Ditto
20	5	30·	29·50	Very fine—stormy night with snow
21	6	34·	29·30	Cloudy
22	7	24·	30·10	Very fine
23	8	34·5	30·	Cloudy
24	9	38·	29·60	Rain
25	10	40·	29·68	Cloudy
26	11	42·	29·43	Ditto
27	12	48·5	29·30	Very fine
28	13	43·	29·57	Cloudy
29	14	40·	30·	Very fine
30	full	46·5	29·94	Cloudy
31	16	46·5	29·74	Fine
Feb. 1	17	41·5	29·80	Ditto
2	18	36.5	29·75	Cloudy
3	19	36·	29·91	Ditto
4	20	36·	29·93	Ditto
5	21	40·	29·77	Rain
6	22	45·	29·77	Cloudy
7	23	51·5	29·84	Ditto
8	24	49·5	29·87	Ditto
9	25	47·	29·77	Fine
10	26	46·	29·77	Ditto
11	27	44·	29·86	Cloudy
12	28	45·5	29·80	Ditto
13	29	43·	29·94	Ditto
14	new	43·5	30·10	Fine

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1820.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Feb. 15	1	37·0	30·20	Cloudy
16	2	31·5	30·20	Foggy
17	3	33·	30·04	Fine
18	4	33·	30·03	Cloudy
19	5	36·5	30·05	Ditto—snow at night
20	6	34·	29·90	Ditto—snow A.M.
21	7	36·	29·80	Ditto
22	8	42.	29·75	Ditto
23	9	50.	29·47	Ditto
24	10	37·	29·30	Rain—stormy night
25	11	42·	29·45	Cloudy—gale of wind
26	12	36·	30·06	Ditto
27	13	36·	30·16	Fine—brisk wind
28	14	38·	29·95	Ditto
29	full	37·5	29·65	Ditto
Mar. 1	16	41·	29·40	Cloudy—stormy night
2	17	31·	29·03	Stormy—snow A.M.
3	18	29·	29·27	Ditto—a considerable fall of snow
4	19	8 A.M. 24 1 P.M. 32	30	Cloudy
5	20	38·5	30·25	Fine
6	21	38·	30·10	Ditto
7	22	40·	30·04	Rain—snow P.M.
8	23	42·5	30·13	Fine
9	24	50·	29·93	Ditto
10	25	39·	29 66	Ditto
11	26	46·5	29·47	Ditto
12	27	43·5	29·40	Ditto
13	28	41·5	29·55	Cloudy
14	new	53·5	29·93	Fine

METEORO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For March 1820.

Days of Month. 1820.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Feb. 23	47	49	44	29·65	0	Cloudy
24	39	41	37	·46	0	Rain
25	37	40	36	·59	0	Cloudy
26	35	35	31	30·10	0	Cloudy
27	33	35	31	·15	0	Fair
28	30	38	30	·05	0	Fair
29	26	42	36	29·92	0	Fair
March 1	40	44	40	·68	0	Fair
2	34	37	32	·10	0	Stormy
3	30	35	30	·89	0	Fair
4	29	36	30	30·19	0	Fair
5	29	36	28	·33	0	Fair
6	29	34	27	·24	0	Cloudy
7	27	35	34	·24	0	Cloudy
8	34	40	32	·35	0	Fair
9	32	46	33	·26	0	Fair
10	34	46	34	29·91	0	Fair
11	35	47	37	·66	0	Fair
12	36	45	39	·50	0	Fair
13	39	49	41	·69	0	Fair
14	40	52	50	30·24	0	Cloud
15	50	59	50	·39	0	Fair
16	45	50	40	·46	0	Cloudy
17	40	43	37	·35	0	Cloudy
18	38	47	38	·37	0	Fair
19	40	43	30	·29	0	Cloudy
20	36	46	40	·22	0	Cloudy
21	41	47	41	·14	0	Cloudy
22	47	50	40	29·93	0	Fair
23	46	50	43	·26	0	Showery
24	44	47	39	·10	0	Showers of hail
25	32	47	35	·45	0	Fair
26	37	46	49	·77	0	Rain

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

XXXVI. *Answer to Mr.* BROWN *on Professor* JAMESON'S *System of Mineralogy. By* A CORRESPONDENT.

To Mr. Tilloch.

Cornwall, Feb. 28, 1820.

SIR, — I OBSERVED in the Number of your Magazine for January, some remarks on the new edition of Professor Jameson's System of Mineralogy, by Mr. P. J. Brown, which appear to me to be written in a very uncandid spirit of criticism.

Mr. Brown begins with confounding a natural history method with what he calls a natural method, which latter, though very indistinctly stated, seems to be neither more nor less than an arrangement according to chemical composition. He thus takes occasion to animadvert severely on deviations from an arrangement with which Mr. Jameson at once disclaims all connexion. Though thus severe upon inconsistences which proceed entirely from his own mistake, Mr. Brown seems quite incapable of observing the beauties, or appreciating the excellencies, of the natural history method. He cannot perceive the high advantage of being able at once to place a mineral either in the earthy or metallic class, from the simple and obvious distinction of specific gravity. He looks only at the grievous, and to him unpardonable, sin against arrangement according to chemical composition, of placing sulphate and carbonate of lead with sulphate and carbonate of barytes.

I need not enlarge on the advantages which would accrue from a method such as that which Mr. Jameson attempts. By enabling any one to recognise a mineral at first sight, and give it its proper place in the system, it removes the great deficiency of mineralogy, and gives it that facility and precision which have conferred such superiority on botany and zoology. To obtain this great desideratum, what mineralogist would hesitate to make some sacrifices, even in what may be considered as the just alliances of minerals, those derived from their composition? And even though he were obliged to place sulphate of lead with sulphate of barytes, would he not think this amply compensated by the advantages gained? At least, while there is a propriety in admitting the one system, there is an equal propriety in admitting the other; each having its peculiar advantages.

As for the discrepancies which he seems so much to exult over, they are so trivial as scarcely to deserve attention, when it is recollected that, in giving the general characters of a class, it is by no means supposed that no mineral is to be included but what agrees exactly with every one of them. It is sufficient if it corresponds in the greater number or the more important.

Another thing which Mr. Brown finds fault with, is, that in the

the observations which are inserted at the close of many of the articles, to enable the student to distinguish particular minerals from those which they more nearly resemble, the discriminating characters given frequently depend on chemical properties, of which Mr. Jameson professes his system to be perfectly independent; yet Mr. B. might easily perceive that these are not delivered as essential characters, but are merely intended to give greater facility, and it would be a very absurd adherence to system to reject them. What he says about the vagueness of the definitions is applicable to mineralogy in general, not merely to Mr. Jameson's system.

The individual and minor faults pointed out by Mr. B. may be readily admitted without any great diminution of the merit of Mr. Jameson's work, when we reflect how arduous the undertaking is which he has attempted, and has so far accomplished. It is not merely precision in distinguishing "fibrous brown zinc blende" from "fibrous brown iron ore," or "antimony ochre" from "bismuth glance," without the possibility of any person of inferior apprehension making a mistake, that is to constitute the excellence of the system; but the laying down such methods, founded on the external characters of minerals, as shall afford the means of immediately placing the mineral in the class, order and species to which it belongs. In such an undertaking some imperfections must no doubt occur; but these, though urged and magnified by wit, good or bad (allowing even to Mr. Brown the indulgence of the alternative), will be little regarded by the candid inquirer.

It gives me pleasure, as an old pupil of Mr. Jameson's, to have the opportunity of vindicating his labours from unfair and groundless objections, and to express the conviction which all must entertain who have studied under him, that mineralogy will always be improved by his exertions.

I remain, sir,

Your most obedient servant,

G. M.

XXXVII. *Further Reply to Mr.* Riddle *on the Lunar Observations. By Mr.* Henry Meikle.

To Mr. Tilloch.

Sir,—In your last Number Mr. Riddle has resumed his favourite theme of making vague remarks and groundless charges on my paper on the Lunar Observations; I shall therefore take this opportunity of replying briefly to some of his very unfair and incoherent assertions.

I formerly

I formerly adduced Professor Playfair's definition of parallax, as of the first authority, and of a more general nature than the limited one given in several books. That Mr. Playfair's not adopting the latter definition was from a conviction of its imperfection, is evident from his rejection of some definitions in Euclid for a like reason. He therefore gave his more general definition. In short, nothing can be more plain than that the limb of the moon is either affected by parallax or it is not. If it is, astronomers are right in applying a correction for parallax to the observed altitude of the limb; but if, as Mr. Riddle would have us believe, it has no parallax, then we obtain the true altitude by using the refraction only. Such is the result of his counterfeit definition.

In the common method of correcting the moon's altitude, the corrections are taken out for the centre in place of the observed limb; the error is therefore the difference of the corrections for parallax and refraction at two altitudes differing by a semidiameter of the moon, or 16′, and this, as I have repeatedly stated, amounts to 18″ at the altitude of 7°. How Mr. R. can take upon him to reduce the semidiameter itself to 18″ is best known to himself. I may likewise observe that the augmentation of the diameter parallel to the horizon is more accurately the difference of the parallaxes of its opposite limbs, than in the case of any other diameter whatever. Of this Mr. Riddle is no doubt sufficiently aware, but he must needs bring himself off in some shape or other.

Mr. R. must also be aware that he has entirely misapplied his inaccurate quotations from my papers; since in most of these passages it is not the altitude of the *centre* I am complaining of. So that, after all, he is still proving his own favourite trifles; and I would further remark, that the method with the excentric point will accurately give the true distance of the centres, whether that is the thing required or not.

It is very extraordinary indeed, that he should still obstinately persist in asserting that there is nothing vague or unsatisfactory in the popular explanation of the quadrant. I would beg to ask him, how much worse an explanation can really be than one that is not true.

The rest of his remarks being much of the same sort as the above, it is useless to multiply words on such an unprofitable subject; since it is plain that "though vanquish'd, he can argue still."

I am, sir, your most obedient servant,

Berner's Street, April 6, 1820. HENRY MEIKLE.

 XXXVIII. *Ca-*

XXXVIII. *Catalogue of Ancient Eclipses, with the Dates of their corresponding Eclipses at one and two Periods Distance.* By Mr. Thomas Yeates.

Mr. Yeates presents his compliments to Mr. Tilloch, and sends for insertion, *if approved*, a collection of corresponding eclipses he has studiously prepared, and presumes them to have an importance towards cultivating more perfectly the lunar astronomy.

Chancery Lane, April 4, 1820.

Catalogue of Ancient Eclipses, &c.*

Before Christ.		Where observed.	Time observed.	D.	H.	M.	
721	☽	Babylon	March	19	10	34	total
	☽	A.D. 192	March	16	4	A.	
	☽	A.D. 1104	March	13	3	A.	
720	☽	Babylon	March	8	11	56	
	☽	Babylon	September	1	10	18	
	☽	A.D. 193. 1105	None				
621	☽	Babylon	April	21	18	22	
	☽	A.D. 292	April	19	1	M.	
	☽	England 1204	April	15	12	39	total
523	☽	Babylon	July	16	12	47	
	☽	A.D. 390	July	13	7	A.	total
	☽	A.D. 1302	July	10	4	A.	
502	☽	Babylon	November	19	12	21	
	☽	A.D. 411	November	16	5	A.	
	☽	A.D. 1323	November	13	2	A.	total
491	☽	Babylon	April	25	12	12	
	☽	A.D. 422	April	22	10	A.	
	☽	Cesena 1334	April	19	10	33	total
431	☉	Athens	August	3	6	35	
	☉	A.D. 482	July	31	4	A.	
	☉	A.D. 1394	July	28	2	A.	
425	☽	Athens	October	9	6	45	total
	☽	A.D. 488	October	6	5	45	total
	☽	A.D. 1400	October	3	2	A.	
424	☉	Athens	March	20	20	17	
	☉	A.D. 489	March	18	5	M.	
	☉	A.D. 1401	March	15	2	M.	
413	☽	Athens	August	27	10	15	total
	☽	A.D. 500	August	25	1	M.	
	☽	A.D. 1412	August	22	6	M.	

* *Vide* Ferguson's Astron. N. B. Eclipses not found in Mr. Ferguson's Tables are collected from *L'Art de vérifier les Dates*, and later ephemerides.

Before Christ.		Where observed.	Time observed.	D.	H.	M.	
406	☽	Athens	April	15	8	50	total
	☽	A.D. 507	April	13	2	M.	
	☽	A.D. 1419	April	10	8	M.	
404	⊙	Athens	September	2	21	12	
	⊙	A.D. 509	August	31	10	M.	
	⊙	A.D. 1421	August	28	9	M.	
403	⊙	Pekin	August	28	5	53	
	☽	A.D. 510	August	5	5	A.	
	☽	A.D. 1422	August	2	11	A.	
394	⊙	Gnide	August	13	22	17	
	⊙	A.D. 519	August	11	7	M.	
	⊙	A.D. 1431	August	8	4	M.	
383	☽	Athens	December	22	19	6	
	☽	A.D. 530	December	20	10	M.	
	☽	Rome 1442	December	17	3	59	total
382	☽	Athens	June	18	8	54	
	☽	Athens	December	12	10	21	total
	☽	A.D. 531	June	15	4	A.	total
	☽	A.D. 531	December	10	2	M.	total
	☽	A.D. 1443	June	12	2	A.	total
	☽	A.D. 1443	December	7	6	M.	total
364	⊙	Thebes	July	12	23	51	
	⊙	A.D. 549	July	10	4	A.	
	⊙	A.D. 1461	July	7	10	M.	
357	⊙	Syracuse	February	28	22		
	⊙	A.D. 556	February	26	5	A.	
357	☽	Zant	August	29	7	29	
	☽	A.D. 556	August	6	11	A.	
	☽	A.D. 1468	August	4	2	M.	
340	⊙	Zant	September	14	18		
	⊙	A.D. 573	September	12	4	M.	
	⊙	A.D. 1485	September	9	2	M.	
331	☽	Arbela	September	20	20	9	total
	☽	Paris 582	September	17	12	41	
	☽	A.D. 1494	September	14	19	45	total
310	⊙	Sicily Isl.	August	14	20	5	
	⊙	Paris 603	August	12	3	3	
	⊙	A.D. 1515	August	9	9	A.	
219	☽	Mysia	March	19	14	5	total
	☽	A.D. 694	March	17	7	M.	total
	☽	A.D. 1606	March	24	11	17	total G.S.
218	☽	Pergamos	September	1	rising		total
	☽	A.D. 695	August	29	9	A.	
	☽	A.D. 1607	September	5	15	40	G.S.

Before Christ.		Where observed.	Time observed.	D.	H.	M.	
217	⊙	Sardinia	February	11	1	57	
	⊙	A.D. 696	None				
	⊙	A.D. 1608	February	15	12	0	
203	⊙	Frusini	May	6	2	52	
	⊙	A.D. 710	May	3	3	A.	
	⊙	A.D. 1622	May	10	2	A.	
202	⊙	Cumis	October	18	22	24	
	⊙	A.D. 711	October	16	5	8	
	☽	A.D. 1623	October	8	0	25	
201	☽	Athens	September	22	7	14	
	☽	A.D. 712	September	19	5	A.	
	☽	A.D. 1624	September	26	8	55	total
200	☽	Athens	March	19	13	9	total
	☽	Athens	September	11	14	48	total
	☽	A.D. 713	March	17	1	M.	
	☽	A.D. 713	September	9	5	M.	
	☽	A.D. 1625	March	23	14	11	G.S.
	☽	A.D. 1625	September	16	11	41	G.S.
198	⊙	Rome	August	6	0	0	
	⊙	A.D. 715	August	4	3	M.	
	⊙	A.D. 1627	August	11	4	M.	
190	⊙	Rome	March	13	18	0	
	⊙	A.D. 723	March	11	1	A.	
	⊙	A.D. 1635	March	18	0	0	
188	⊙	Rome	July	16	20	38	
	⊙	A.D. 725	July	14	0	0	
	⊙	A.D. 1637	July	21	0	0	
174	☽	Athens	April	30	14	33	
	☽	A.D. 739	None				
	☽	A.D. 1651	None				
168	☽	Macedonia	June	21	8	2	total
	☽	A.D. 743	June	18	7	A.	
	☽	A.D. 1657	June	25	9	35	total
141	⊙	Rome	July	18	22	0	
	⊙	A.D. 772	July	5	11	M.	
	⊙	A.D. 1684	July	12	4	26	total
104	⊙	Rome	July	18	12	0	
	⊙	Paris 809	July	15	21	23	
	⊙	A.D. 1721	July	24	0	0	
63	☽	Rome	October	27	6	22	total
	☽	A.D. 850	October	24	8	A.	total
	☽	A.D. 1762	October	23	8	M.	
60	⊙	Gibraltar	March	16	setting		central
	⊙	A.D. 853	March	13	2	A.	

60

Before Christ.		Where observed.	Time observed.	D.	H.	M.	
60	☉	A.D. 1765	March	21	2	A.	
54	☉	Canton	May	9	3	41	total
	☉	A.D. 859	May	6	11	M.	
	☉	A.D. 1771	None				
51	☉	Rome	March	7	2	12	
	☉	A.D. 862	March	4	10	M.	
	☉	A.D. 1774	March	12	10	M.	
48	☽	Rome	January	18	10	0	total
	☽	A.D. 865	January	15	6	A.	total
	☽	A.D. 1777	January	23	4	A.	
45	☽	Rome	November	6	4	0	total
	☽	A.D. 868	November	4	4	M.	
	☽	A.D. 1780	November	12	4	M.	
36	☉	Rome	May	19	3	52	
	☉	A.D. 877	November	9	1	M.	
	☉	A.D. 1789	November	17	3	M.	
31	☉	Rome	August	20	setting, gr. ecl.		
	☉	A.D. 882	August	17	3	A.	
	☉	A.D. 1794	August	25	5	A.	
29	☉	Canton	January	5	4	2	
	☉	A.D. 884	January	2	8	M.	
	☉	A.D. 1796	January	10	6	M.	
28	☉	Pekin	June	18	23	48	total
	☉	A.D. 885	June	16	10	M.	
	☉	A.D. 1797	June	25	8	A.	
26	☉	Canton	October	23	4	16	
	☉	A.D. 887	October	20	0	0	
	☉	A.D. 1799	None				
24	☉	Pekin	April	7	4	11	
	☉	Constantinople 889	April	3	17	52	
	☉	London 1801	April	13	4	M.	

Remarks.

1. The eclipse of the sun observed at Pekin, April 7, $4^h\ 11^m$ in the 24th year before Christ, returned visible at Constantinople A.D. 889 on April 3, $17^h\ 52^m$ after a period of 912 years—less, three days ten hours nineteen minutes. In a period of 912 years three whole days are to be allowed for the anticipation of the moon, at the rate of one day in 304 years. See this explained in Mr. Ferguson's Astronomy, art. 387, p. 377, 12th edit.: therefore this period seems to agree with ancient observations.

2. The same eclipse of the sun observed at Constantinople A.D. 889, returned again invisible in England A.D. 1801 on April 12, at $16^h\ 21^m$ in long. 0· 22° 45′, or about April 13,

4^m

4^{m} civil time, after 912 years eight days twenty-two hours twenty-nine minutes; but by reducing the calendar reckoning to the old style, the same eclipse happened April 1, 16^{h} 21^{m}; which makes the difference two days one hour thirty-one minutes; so that allowing three days in each period for the lunar anticipation, the corresponding eclipses of A. 1801 have been found to anticipate three days in each period, or six whole days in two periods, very nearly.

3. Those eclipses set down in Ricciolus's catalogue are corrected up to the Gregorian account; and therefore the several corrections of the calendar since the Nicene Council must be regarded in making use of the corresponding dates.

4. It seems possible to prove that 912 solar years constitute a complete lunar period, in which the solar and lunar motions coincide in the same point of the ecliptic; and that in this period the whole system of solar and lunar eclipses is performed.

5. That the variations of calendar time may possibly account for the lunar anticipation of the old astronomy, and that such may be accounted for on other principles than any real anticipation of the lunar motions, which principles we shall suggest in a future communication.

[To be continued.]

XXXIX. *On the Expansion and Contraction of Iron Bridges by Changes of Temperature; with a Suggestion for counteracting these Effects.* By A CORRESPONDENT.

To Mr. Tilloch.

SIR, — THE effects of changes in the temperature of the atmosphere, in expanding and contracting metals in exposed situations—for example, iron bridges—does not seem to have excited much attention until lately, when some of the effects became too visible to remain longer unnoticed. Permit me to offer an idea (through your very valuable Magazine) which *may be* of some advantage in the construction of those very elegant and economical structures. As it is impossible to prevent heat and cold from producing their natural effects, instead of the immense abutments, that are at present necessary to resist the increase of thrust or pressure in warm weather with a constant change of the curvature of the bridge, and necessarily a change in the points of contact, in the parts forming the arch, I would place the ribs or segments on blocks or bars of zinc, placed in recesses formed for that purpose in the abutments (and piers if consisting of more than one arch); those blocks or bars in length should be to the extent of the arch in the ratio that zinc expands to the

the metal of which the bridge is composed, taking into consideration their respective situations. Thus in warm weather, when the arch is expanding, the same would take place in the blocks, which would raise the arch to a wider part between the abutments; in cold weather the contrary would be the case. The arch by this means would in all temperatures fit between the abutments, obtaining from them that support which is necessary to its stability, with a perfect uniformity in its thrust, or lateral pressure.

Your most obedient and very humble servant,

Chatham, April 1, 1820. I. K. K.

XL. *Description of an improved Hydro-pneumatic Apparatus. By Mr.* John Cuthbert *.

113, St. Martin's-lane, London, April 13, 1819.

Sir, — I beg leave to offer to the Society for the Encouragement of Arts, Manufactures, and Commerce, a combination of the blow-pipe and pneumatic trough invented by me; the utility and simple arrangement of which I hope will meet with their approbation.

It is a well known fact that in chemical analysis the blow-pipe is indispensably necessary; and the almost constant application of the pneumatic trough need not be enlarged upon. The apparatus I lay before the Society combines these two instruments in one, and when required for either purpose, can at once, without any trouble, be dismounted, and the pieces applied to their separate uses.

To a public lecturer, who often at the time of exhibiting the effects of gas may have occasion for a blast-pipe, this combination is of infinite advantage, by reason of the facility with which oxygen gas from a bladder may be introduced and passed through the platina jet, which cannot be fused by the heat required.

When the apparatus is used as a pneumatic trough, it is only necessary to remove the mouth-pipe and cover; which latter, when taken off, becomes a useful tray for transferring the receivers from the pneumatic trough, the ledge of the cover being sufficiently deep to contain water for that purpose.

The water from the bottom vessel may be occasionally forced up by the mouth, in case of a deficiency when many receivers are in use, and is kept up to the height required by means of a sliding-pipe within the large tube *f f*; and if the contrary effect is

* From the *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, vol. xvii. for 1819. The Society's silver medal was presented to Mr. Cuthbert for this apparatus.

is necessary, by depressing the sliding-pipe any quantity of the water may be let off at once into the lower vessel. By this means a constant accession or diminution of the water is in the operator's power, without delay, or the assistance of a second person.

I am, sir, &c.

A. Aikin, Esq. Secretary, &c.

JOHN CUTHBERT,
Philosophical Instrument-maker.

References to the Engravings, Plates IV. *and* V.

Plate IV. (fig. 1) is a view of the apparatus, as a blow-pipe with the air vessel *b* thrown open to show the tubes within. Through the tube *d d*, which descends within a quarter of an inch of the bottom of the air-vessel, the air is conveyed from the mouth. The tube *e e* is to carry the air from the vessel *b* through the jet to the lamp; this tube contains another tube, which slides air-tight into it, and is shown by fig. 6; to the extremity of this a transverse tube (fig. 7) is connected. To the moveable one is attached the jet *k*, which, by turning in an opposite direction to that required when it is in use, is capable of shutting off the air, as it becomes a stop-cock, by means of the hole *h* in the tube.

The large tube *f* is to let the water down from the broad part of the pneumatic trough, to force the air from the air-vessel through the centre tube *e e*.

The upper part of the tube *d d* is capable of being removed to admit the pipe of the bladder (Pl. V. fig. 6), which fits air-tight into it, by which means oxygen gas may be forced into the air-vessel (the common air being first displaced), which will render the flame particularly effective where great heat may be necessary.

The lamp and tray *l* are placed on the cover of the blow-pipe, so as to allow the wick to stand immediately before the jet. The jet tube *e* can be raised or lowered for the purpose of depressing or elevating the flame, as occasion may require; *m* is a shade to prevent the breath from agitating the flame which is attached to the tube *d d*, and may be removed back at pleasure.

Fig. 3 is a section of the instrument in use, showing the water descending from the upper part through the large tube into the lower vessel *b*, and forcing the air above its surface through the centre tube to the lamp.

Fig. 2 is the apparatus employed as a pneumatic trough for collecting gas, by means of the short sliding tube, shown in fig. 7, which fits into the large tube *f* (fig. 2), and is furnished with the cap *n* (fig. 7) to prevent the water being thrown over the upper edge of the trough when forced up from the air-vessel, by blowing through the tube *d* (fig. 1).

Fig. 8

Fig. 8 is a ground plan of the cap *n* of fig. 7. One of the advantages of this construction is, that in filling a large receiver with water from the trough, the deficiency of the fluid can be supplied by raising it from the lower vessel by the breath, and it is retained in the upper compartment of the trough by the sliding tube *n* (fig. 7) being drawn up above the surface of the fluid.

When the water in the receiver is displaced by the gas, the redundancy is let off into the lower vessel by the same tube.

By this contrivance, the necessity of pouring in or emptying out water through the help of another vessel is obviated, and the operator relieved from that inconvenience.

The broad surface of the upper part of the pneumatic trough is particularly adapted to keep the pressure of water equal on the air, which produces a uniform flame, the altitude of the water being little affected, as well as presenting the most convenient form for pneumatic purposes.

Fig. 4 is the cover of the apparatus inverted, which becomes a tray to which the receivers may be transferred when filled with gas.

The two tubes *o* and *p* are to prevent the water which is in the tray (to keep the receivers air-tight) from passing through the holes.

Fig. 9 is a ground plan of the lamp on a moveable shelf.

Fig. 10 is a side view of the moveable shelf and lamp.

Plate V. fig. 1, is a perspective front view of the apparatus, to show the moveable shelf, and the manner of attaching it to the tubes *e* and *f* by the bayonet catch, as it may be necessary to employ the lamp when the cover is in use as a tray.

Fig. 2 is a perspective side view of the same.

Fig. 6 is the bladder of gas, with a stop-cock and connecting pipe.

Fig. 5 is a gas-holder that fits into the well of the pneumatic trough, with an attached stop-cock *t*.

Fig. 3 is the same vessel inverted, the stop-cock being removed, and the aperture made water-tight by the screw *s* (fig. 4); by the addition of a moveable shelf, it becomes a small pneumatic trough.

Fig. 4 is a section of the above, showing the trough furnished with its shelf and a hemispherical chamber *r*, connected to its under side, to ensure the gas from the retort to pass only through the hole in the shelf, as also effectually to prevent the retort from slipping out of its position.

XLI. *Facts respecting the increased Volatility and Inflammability which Fish Oil and its Vapours acquire by continued or renewed Exposure to certain high Temperatures; elicited by the Examination of Evidence in a late Trial in the Court of Common Pleas* (SEVERN, KING *and* CO. *versus* DREW, *or the Imperial Insurance Office*), *before the Lord Chief Justice* DALLAS *and a Special Jury.*

THIS case, important in every view, as embracing the practice and principles by which fire-insurances are regulated, proved not less so to science, on account of the high attainments of many of the witnesses, and the curious facts that were elicited by their evidence. We have therefore thought that we should render an acceptable service to our readers generally, by giving the proceedings a place in our pages. The trial commenced on the 11th of April, and did not close till the 13th.

Mr. Stephen opened the pleadings.

The Solicitor-general afterwards rose, and stated the plaintiffs' case in an address, embracing a complete view of the whole subject. The present action was brought for a sum of upwards of 8000*l.* against the defendants, who are directors of the Imperial Insurance Company. The plaintiffs were very respectable and opulent sugar-refiners, residing in Whitechapel. This was a part only of a very large sum, amounting to about 70,000*l.* total loss which had been sustained by fire in November 1819. Upon that point no doubt or difficulty existed, nor was the slightest imputation cast upon the character of the plaintiffs. The only question arising between the parties turned either upon points of law, or related to the manner and regularity of effecting the insurance. It would be for the jury, after deliberately weighing the evidence on both sides, to decide whether the plaintiffs were entitled to an indemnification. With reference to this ultimate question he might be allowed to mention, in the first place, that they had paid an enormous premium for their insurance. It was made indeed upon an express provision, and not on any of the general rules by which the office directed its transactions. The rate of insurance in ordinary cases was two shillings per cent., and from that it graduated to five shillings, which was considered, to use the language of the insurers themselves, as the rate applicable to risks of a double hazard. It was the premium required where tallow or any other matter easily combustible was in daily use, or formed a material of manufacture. But the present case was altogether singular, for the premium actually paid was no less than 14 shillings, being more than double that required in all ordinary cases for doubly hazardous insurances. He mentioned this

this circumstance in order to show that the defendants could not have conceived that they were insuring an ordinary risk, but one of a peculiar nature, and from which if they were to sustain loss, they had no right to complain, as they received a premium commensurate to it. He should now have to draw their attention to the description of premises over which the insurance had extended, and which comprehended a grinding-house and stoves, an engine-house, a filling-house, and a warehouse secured by iron doors. (A wooden frame, containing a plan of the entire building, was here produced in court, and was made use of by the learned gentleman to illustrate his description.) This plan was offered to their attention, in order to assist, if possible, in inquiring into the circumstances which attended the origin of the fire. It was not necessary for him to carry the jury through all the pleas upon this record. The material one on the side of the defendants was, that the fire had been occasioned by a cause not included within the risk insured. By stating the case in this manner, he hoped to disembarrass it, and disincumber it of whatever might tend to perplex their inquiry. It was pleaded on the other side, that the plaintiffs had used a process of heating oil which had increased the risk, and that this process had been introduced subsequently to the insurance. Now, he should be able to show that the apparatus in question was separated by a wall and iron doors. It might then become a question, whether, whatever might have been incumbent on the plaintiffs to state to the insurers at the outset, they ought, upon the adoption of this process, to have subsequently apprized them of it. He should contend that his clients were not bound to make any communication on this subject, unless the defendants themselves had thought proper to inquire in the first instance. Supposing, therefore, the risk to be increased by this new process commenced upon contiguous premises, the plaintiffs were under no obligation to disclose the circumstance unless the insurers thought proper either to inspect the premises, or seek information; in which case, his clients were undoubtedly bound to make a full and unreserved disclosure. His clients were engaged in the business of sugar-refining to a great extent, and the processes by which that operation was carried on were almost as various as the establishments engaged in it. Those, therefore, who insured such manufactories were bound to examine the mode in which they were conducted, if they had in contemplation to limit their insurance to any given risk; and if they did make such an inquiry, it was the duty of the other party to leave no circumstance concealed. When he talked of a variety of processes, he would endeavour to make himself intelligible. The original and simple mode of refining sugar was, to place it in a pan, with a fire under it. This, however,

ever, was found very inconvenient, inasmuch as that part of the sugar which was nearest the fire was burnt before the rest could be brought to a proper temperature. Ingenious and scientific men contrived to produce the requisite degree of heat by pipes of boiling water coiled and immersed into the syrup, or sugar. But this difficulty arose, that the sugar would not boil at the same heat as the water, and therefore a high pressure, attended with much danger, was required. At Liverpool tallow was in common use for this purpose; but this was found extremely offensive, and, to obviate its inconveniencies, Mr. Wilson had invented a process for using fixed oil. This oil was put into a large retort, and from it a coil of pipes passed into the sugar which it was intended to bring into a boiling state. The oil required a temperature of 600 degrees of Fahrenheit for boiling, and sugar 350. This was a degree of temperature below any heat that could create danger. The first question therefore would be, whether by means of this communication there was any increase of risk; and, secondly, whether the loss was occasioned thereby —as he understood, instead of being increased, the risk was diminished, upon a comparison with the former mode. One of the risks attending the former mode was that of the sugar boiling over, in which case the premises must be infallibly destroyed. By the new process, a thermometer was fixed in the retort, and, as soon as the oil was heated to the degree of 350, it passed into pipes which conveyed it through the sugar, till, like the circulation of the blood, it reflowed into the retort. It would be idle to suppose that, upon a question of this nature, he was not aware of some of those grounds which would be taken on the other side. It would be said, as he apprehended, that the pipes might burst; but this was guarded against by the valves being smaller than the diameter of the pipe. But then it might be urged, that gas must be created; and to this he would reply, that gas was not created by fixed oil until it arrived at a temperature of between 600 and 700 degrees, long before which the thermometer would have exploded. To be brought to such a temperature, the heat must be continued for a long time in a state of almost incredible intensity, for the accession of heat became very slow after a certain point. From the retort a small pipe ascended, for the purpose of carrying off such air as might remain in the pipes, and in a retort not always full. If any gas was created, it must escape through this pipe. Again, it might be urged, that the air might be driven downwards by a sudden draught—a result, certainly, to be looked for in an ordinary room, but, if admitted into these premises, would have defeated the whole process which was carrying on. Every draught was carefully prevented, and the fire fed by external air admitted from behind

behind it. He understood also, that if any gas should escape and mix with the air, it never could so escape in any proportion, that by the contact of fire could produce either danger or explosion. All this he stated on the authority of persons long familiar with experiments, and deeply versed in subjects of this nature. It would perhaps be said that the nature of oil might be changed by the renewed application of heat, and that when so changed, it would emit gas at 300 degrees. This, however, he had reason to believe was utterly false, and although the oil might be rendered thicker, the reapplication of heat would restore it to its former state. The fact also was, that it had no communication with the external air. The retort had been recently taken off and repaired; and he might undertake to demonstrate that the risk was less than by the former method. With regard to a pan that was still in use, and to which his learned friend had just drawn his attention, it was enough to remark, that it was a mere substitution for one used previously to the insurance. As to the circumstances attending the fire, he should but briefly advert to them, as they would appear in evidence, clearly and satisfactorily, before them. After it was extinguished, as it was natural to imagine that some explosion might have been the cause, the apparatus was inspected, and the retort found without any rent or fissure in it, and with no other mark than that impressed by the superincumbent weight which had fallen upon it. A copper boiler was also found melted; but it would be proved that the coals under it, and in a state of ignition, gave a satisfactory explanation of that circumstance. This was the case which he should now proceed to substantiate by evidence; and, having so done, he was sure the jury would feel satisfaction in securing to a gentleman who had paid so large a premium, a full indemnification for the loss which had been incurred.

The Chief Justice asked, whether the process in question was peculiar to the plaintiffs' sugar-house?

The Solicitor-general replied in the negative, and added, that the same insurance-office had underwritten several warehouses of the same description.

The policy having been put in,

The Chief Justice remarked that, as he now understood the case, five questions would arise:—1. Did this process fall within the risk contemplated by the insurance?—2. Was the fire occasioned by it?—3. Were the plaintiffs bound to give notice when they introduced it?—4. Was the insurance void?—And, 5. Did the circumstance of this process being carried on closely adjoining to, but not actually upon, the premises that were insured, create any difference in the preceding questions?

On

On the suggestion of Mr. Scarlett, the conditions of the policy were read at length, and the Solicitor-general undertook to show that there was a special agreement in this case.

George Luke was then called, and stated that he was a millwright, and had been three years in the employment of the plaintiffs. He left the premises on the 9th of November, the day preceding the fire, about half-past eight o'clock in the evening. The only person whom he left behind him was, to the best of his knowledge, the engineer. The fill-house, where the retort was placed, was in the lowest part of the premises, and the warehouse on the next floor immediately above. When he returned at seven o'clock the next day, the fire had broken out, and had been raging for some time. He was acquainted with the new oil apparatus. That part of it which consisted of a pan had been used before, and the whole had been at work about three months. The fire kept under the oil was less than it used to be by the former method. When the oil became heated to a degree of between 320 and 350, it was considered to be in a fit state for working. The pump was then set on, and, by means of its suction, the heated liquid was conveyed through pipes. As the sugar was cold, the heat of the oil decreased by its communication, and became just sufficient to cause the sugar to boil. In the warehouse there was always a quantity of sugar lying about on the floor: and, being the place where the packing was performed, there was generally a considerable quantity of empty hogsheads, paper, and straw likewise. The retort-pipe entered a steam-vent, and the air escaped by an opening in the side of the chimney. He had examined the retort a few days before the fire; and, though a drop or two of oil might leak out, it worked very well, and without danger.

This witness, on his cross-examination by Mr. Scarlett, said it was not his business to attend to the fire: that was the express employment of another man. The pipe from the retort did not go to the top of the steam-vent, which was 70 feet high. The pipe in question rose about 16 feet from the vessel. All the fires were out when he left the place on the evening of the 9th. There was no fire-place, nor any fire generally kept, in the warehouse. He did not know whether the same oil was put into the vessel after its repair as had been used before; but he admitted that it still continued to drop.

In answer to a question from the Bench, witness added that no fire had taken place in the premises during the three years before the new process was adopted, and that this new process had been at work only three months.

Henry May deposed that he was engine-keeper at the time of the fire, and had been 24 years in that situation. He was on the

the premises on the evening of the 9th, till about nine o'clock. The last witness was with him some time in the engine-house, when he was engaged in packing the piston. When he went away, no one, to his knowledge, was left behind but the watchman; and all appeared to him to be safe and right. He returned a few minutes after three o'clock the next morning, and went to the engine-house to get the engine ready. The fire had been lighted as usual, before he came. He, however, proceeded to make it up, and found nothing wrong at that moment. In about twenty minutes after he went through the mill-room to the long-house, which was on the warehouse floor, and, standing on the steps which led down to the fill-house, he called to know if they were ready below. He received for answer, that they were not quite ready; and in consequence of this he went away, but returned in the space of ten minutes and inquired again. Understanding now that they would be ready immediately, he prepared to start his engine, which stood in a yard by itself, and set it to work. In a very few minutes he heard an alarm of fire, and he went again through the mill-room, and perceived, by a door opening to the left, that the fire was on the warehouse floor. There was a good deal of smoke, and he saw no hole in the floor through which the fire could have found its way. There were various hogsheads, as well as straw and paper, lying about. The iron doors were fast, and the premises were burnt down in about three hours.

Cross-examined.—There was no fire remaining when he left the premises the night before. The steam-engine was used for the mill as well as for the oil-pump. The engine had been put on several times before he heard the alarm of fire. There was no great body of fire when he first perceived it in the warehouse.

Christian Lampe was employed in the warehouse. It was there that hogsheads were emptied on the floor, for the purpose of being refined. On the 8th of November several hogsheads were packed, and on the following day six or seven were emptied. This had perhaps increased the quantity of straw and paper usually lying about. One pan had been charged by him with the sugar thus emptied from the hogsheads. The candles sometimes used in the warehouse were blown out, and left either on the floor or on the hogsheads till the next morning; there was no particular place for depositing them. The hogsheads were sometimes left with sugar adhering to their insides, but were finally scraped and steamed. A gas-light was used in the warehouse.

Henry Wylling, another workman on the same premises, confirmed the evidence of the preceding witnesses in all its essential points. He came about four o'clock on the day of the fire; and on examining the iron doors of the warehouse, found them to be fast. The smoke was so bad that he could not at first see whence the fire proceeded; but, by crawling on his hands and knees, he reached the place of communication between the long-house and the warehouse, and perceived light as well as smoke in the long-house. He could also see into the fill-house, by means of the gas and fire above. The fire appeared to have seized on the sides of the fill-house, but he saw none in the body of it. He had likewise a distinct view of the oil apparatus at this time, and must have seen the mischief, if any were taking place; but he saw none.

George Wick, a sugar-refiner, who lived near the premises, deposed, that he rose in consequence of an alarm of fire at the time already specified. He approached within a yard and a half of the fill-house in Union-street; and at twenty minutes past four he perceived neither smoke nor fire within it.

Samuel Aubur stated, that he also resided in the same neighbourhood; and being awakened by the watchman's rattle at an early hour on the 10th of November, he proceeded to a spot where he could see through a window of the warehouse. The lower part of this window was on a level with his breast, the fill-house being considerably below ground. He then perceived a quantity of fire and smoke opposite, part of which appeared to him to descend into the fill-house.

Mr. Henry Wilson was then called, and stated that he was well acquainted with the various processes of refining sugar. He had for some time been conversant with this subject, and with the application of oil for that purpose. He had put up apparatus for the same purpose in two houses at Liverpool, three years ago, as well as in one or two others in London. In the present case it had been put up almost exclusively under his direction. He conceived that the ordinary mode of refining sugar was attended with both inconvenience and danger. One great danger was that of the sugar boiling over; another, of its emitting very inflammable gases, arising from the combustibility of sugar when in a desiccated state. At the degree of 344, inflammable gases were created. The boiling point was 245. He visited the premises in question almost every day, and observed that there was some leaking, but none that could produce the slightest mischief. It was impossible that the leaking of oil into the fire below could cause the fire to communicate with the oil in the vessel. When the oil was thus converted into combustible gas, it passed off up the chimney. The temperature of the oil in the vessel never exceeded 360 for the working point. If it were to go beyond 440, the thermometer would burst. The boiling point of oil was about 600 degrees; and until it attained that degree of heat, it it would send out no permanently inflammable gases. To bring about such a degree of heat would require several hours of hard firing, and could never take place from negligence alone. If a similar fire were placed under sugar, inflammable gases would be produced in a much shorter time. If produced in this retort, they would go up the steam-pipe, a vent which was rendered necessary for discharging the air within the vessel; and in oil there was also a quantity of aqueous matter, which it was necessary to carry off in the same manner: conducted through the pipe to which he alluded, it all passed out at a lateral aperture in the chimney. All draught down the chimney was carefully guarded against, and the brick-work of the chimney secured against heat by cast-iron pipes. He thought there was no danger from the gas that might escape mixing with the atmospheric air, because the proportion necessary to cause explosion must be one to six or seven, and it was impossible that such a proportion should be formed even in the fill-house. This danger was much greater with the ordinary process. The vent was quite sufficient to carry out ten times the quantity of gas which could be produced by this apparatus. The retort had been repaired a few days before. With regard to the oil changing its quality, because it became thicker by use, he did not think that that increased its tendency to become inflammable. He was present at an examination of the apparatus with several surveyors and engineers after the fire had happened; there was not the slightest rent or fissure in the retort, and a four-inch wall on one side of it was as perpendicular as when originally built. There was a great quantity of rubbish upon it, and a piece of fused brass which had come from above; but he could not discover, from the appearances around, the slightest indication of an explosion having taken place. The screws of the aperture were perfect, and the thread bright and sharp. The pump was much fused, and the copper vessel melted—a circumstance to be accounted for by the coals kept under it for the purpose of cutting off the draught. The pipes were of copper, and could not burst from the pumping of the oil, because the valves were smaller than their diameter: in the inside of the retort there was a quantity of carbonaceous matter, naturally resulting from the gradual distillation of the oil, and explosion would have caused a different appearance.

Cross-examined.—He had taken out three patents for the process which he had been describing—one in 1816, one in 1817, and the third in 1818. Previous to this invention he had been engaged in chemical manufactures. His patent had been applied to four cases; two of them occurred at Liverpool, but in both of these tallow was used as well as oil. He had put up his apparatus for another house in London about six months ago, but it was by way of an experiment only, and had not been applied generally to the manufacture. There were various modes of carrying on this manufacture, and one or two patents had been obtained for carrying it on through the means of steam. After the retort had been repaired, the oil which had been already used was put back into it, with the addition of some fresh. He saw no vestige of the thermometer on his inspection of the apparatus after the fire.

Aaron Manby deposed that he was an iron manufacturer, and had constructed the vessel in which the oil was boiled. He attended the formation of the vessel, which was quite perfect. It had no flaws or defects in it. The leakage which had been spoken of occurred in consequence of the common effect of wear and tear. All engines were subject to the same deterioration. Such wear and tear, and their effects, could not be avoided.

Mr. James Harris stated, that he was a sugar-refiner, at Liverpool, and had carried on the business for 14 years. His process was, at present, the common one. He formerly was in the habit of refining sugar by means of oil and tallow; not in a close vessel, like that described in the plan, but in an open one. He found that that method could not succeed, on account of the offensive smell of the oil and tallow. The process pursued by the plaintiffs was greatly superior to the old-fashioned process. By the former a sugar-refiner could heat the article without incurring the danger of the sugar boiling over. Another danger was also avoided, namely, that of heating the sugar to excess, and totally spoiling it. Witness had seen the model now exhibited, and, supposing it to be correct, he thought the hazard infinitely less than that which was to be apprehended from the ordinary course. The plan was less objectionable, and altogether less dangerous, than that which was more generally adopted. Witness, when he formerly made use of oil and tallow, had caused an alteration to be effected in his policy of insurance. He applied to the office to allow him to boil sugar in that way, and it was admitted. It was an existing policy, which, in the first instance, he applied to have altered, and it was in consequence endorsed. By the endorsement, the company allowed him to boil sugar in this way. The date of the endorsed policy was the 31st of May 1814. An alteration was made in the body of another policy, bearing date the 22d of July last, by which the new process of boiling sugar by means of heated oil was allowed. He paid no additional premium when he got the policy endorsed.

Cross-examined.—Witness did not assure the company when he applied for the alteration that the risk would be less under the new system. His first policy was for tallow; the second for tallow and oil, which would be less offensive. The latter process

cess he had abandoned, and returned to the old practice. He had no process like that now under consideration in operation at present. At one time he had three pans, in which the sugar was boiled by heated oil, but he gave the system up.

Re-examined.—He abandoned that principle because the smell of the oil was unpleasant. That defect was now, however, completely cured; and he had no doubt but that he should abandon the old plan, and adopt the improved new one.

—— Robinson stated, that he had been a sugar-refiner for 25 years, and was well acquainted with the old mode of refining sugar, which was attended with considerable danger. It required infinite vigilance to prevent the recurrence of danger. The danger was likely to arise from the boiling over of the sugar. If it boiled over, and came in contact with the fire, or if the sugar fell into the ashes, which were exceedingly hot, the danger was most imminent. He conceived that there was much less risk with Mr. Wilson's method. While the vessel which contained the oil remained entire, there could be no danger. The present plan required much less vigilance and attention to prevent accident than the old one. Witness did not boil his sugar according to Mr. Howard's plan, but according to the ancient method. He knew of sugar-houses having been burned down, but had never once ascertained the exact cause. The floors of sugar-houses were saturated with sugar, and the timber was necessarily extremely dry, in consequence of the intense heat that was obliged to be used.

William Domford was present, after the fire, when the ruins were examined. The vessel in which the oil had been contained appeared to be scarcely injured. Witness was acquainted with the business of sugar-refining; and, on looking to the model exhibited in court, had no hesitation in stating it as his opinion that the new plan for refining sugar was much safer than the old; first, because much less fire was used; and, next, because the flame was not suffered to come in contact with the vessel. Another reason was, because, when mischief was apprehended, the supply of heated oil might be immediately stopped. In the old course there were no such advantages. One of the great dangers of the old plan was, that an inflammable vapour was produced, which was liable to take fire. Sugar boiled at about 344 degrees of heat would, he believed, produce a permanent inflammable gas. He did not know what heat would produce a permanent inflammable gas from oil. It would, however, require a much greater degree of heat to produce vapour, or inflammable gas, from oil than from sugar. In his opinion it would certainly require 582 degrees of heat to form gas from oil. Witness did not know whether the nature of oil was rendered more inflammable by being

ing often heated. In all new oil there was a certain portion of water, and of course it did not inflame quite so soon as old oil which had been frequently heated, and had lost some of those aqueous particles. The difference in that respect could not, however, be considerable. It was quite clear, from the model then before them, that if any inflammable gas had been generated in the machine, it would have escaped through the leaden pipe which led into the steam-vent.

Cross-examined.—Experiments had been tried for the purpose of ascertaining whether oil changed its properties in consequence of constant use. A quantity of old oil was put into one retort, and an equal quantity of new into another. Very little difference was found in heating them to the pitch at which gas was produced. In heating oil for refining sugar, he saw no danger whatever. The process of boiling sugar in the old way was extremely dangerous.

Mr. Samuel Parkes stated that he was an experimental and practical chemist. He had attended to the model of the machine, and to the manner in which the process of refining sugar was carried on; and he considered that the old mode of boiling sugar, for the purpose of refining, was more dangerous than the present. He had no hesitation in saying, that it was attended with much less danger. The danger arose from the chance of the fire coming in contact with the sugar-pan. Witness attended throughout to Mr. Wilson's examination, and, with certain exceptions, agreed in the correctness of what he stated. He did not, however, agree with him in what he said on the subject of the gas of old oil. He had mixed five ounces of sugar with a proper proportion of water; and when he had increased the heat to 230 degrees, the sugar and water boiled rapidly. The thermometer then remained stationary for a time, but it afterwards moved slowly upwards to 340 degrees. A species of gas was then produced; but, when the heat was raised to 370 degrees, the gas burned with a strong and permanent flame. When a heat of 590 degrees was applied to old oil, it produced an inflammable, but not a permanent inflammable gas. The lowest temperature at which inflammable gas was produced from oil was 586; but it was not permanent inflammable gas. What he meant by a non-permanent inflammable gas was, a gas which would light if a match were applied to it, but which would go out when the match was removed. When the heat was raised to 600 degrees, a gas was produced from oil, which, if touched with a match, lighted up, and continued to burn. That was the lowest temperature at which permanent inflammable gas was produced from oil. When a chemist went on applying powerful heat, the difficulty of raising it it was much increased after it had arrived at a certain point. It would not be an easy task to raise the heat of oil to 600 degrees in an instrument like that alluded to. It would require a great effort for several hours. If the fire were left to work by itself, considering its size, it was impossible that so great a heat could be produced. Even if gas had been formed, looking to the formation of the room from which it might have escaped, he saw no danger of its mixing with the atmospheric air, and producing explosion.

Cross-examined.—There was a vapour from new oil at 586 degrees, and from old oil there was a slight inflammable vapour at 580. At 370 degrees sugar produced a strong gas. New oil produced a powerful gas, or vapour, at 608 degrees of heat; old oil produced it at 590. He had not an opportunity of minutely examining whether, in proportion to the time during which oil was used, it gave out gas or vapour with greater readiness. He had made no experiment with respect to the weight of gases emitted by oil at different degrees of temperature. All experience told them that new oil would burn sooner than old. He made no experiment to ascertain whether a large mass of oil, heated from time to time, would become more inflammable than oil which had not been used. The old oil he used was furnished by Mr. Wilson.

Re-examined.—If the oil in the retort were heated to any thing like 590 or 600 degrees, it would carbonize the sugar. Even 400 degrees of heat would produce that effect. One great advantage of the new apparatus was, that in a moment the sugar could be prevented from boiling over.

Mr. Wilson re-examined.—The oil he sent was whale oil, and had been heated in a vessel which he had for the purpose, every day, for a period of about two years. It was heated for the purpose of making experiments.

Mr. W. Brande stated that he had looked at the apparatus now before the court, and had maturely considered the subject with the view of giving evidence here to-day. As he was necessitated to come there, he had made a few experiments. As far as his experiments, which were on a small scale, went, he must say that the apparatus appeared to him to be less dangerous than the old one. He had placed a vessel of oil over the fire, and put a pan of sugar in it. He then put in a thermometer to mark the degree of heat. When the heat rose to 300 or 400 degrees the sugar burned. He then applied some lighted paper to the oil, which it immediately put out. He therefore concluded, that sugar gave out gas much sooner than oil. He could not produce inflammable gas from oil under a less temperature than 600 degrees. If gas were generated in that retort, he did not imagine that it would produce danger. The gas which he had seen produced

duced from oil was lighter than atmospheric air. The gas which could have been formed in that apparatus, if it had been let into the fill-house, would not have set it on fire. The gas would have escaped by the steam-vent. In those parts of Mr. Wilson's evidence on which he could give an opinion, he entirely concurred. From examining the model, and viewing the premises, the impression on his mind was, that it was much more likely that the fire broke out in another place, and was not occasioned by the oil in the retort.

Cross-examined.—Supposing the fire, when first observed, seemed to have broken out in that steam-vent, which appeared all in a blaze, what would you then conceive to be the probable cause of the fire?

Witness.—I do not think, if gas escaped from the retort, that it could set the place on fire, unless the aperture or pipe which communicated with the steam-vent were shut.

Mr. F. Accum stated, that he had made experiments to ascertain at what temperature oil would emit inflammable gas. He could distinguish between new and old oil, having made his experiments with both. He found that new oil emitted permanent inflammable gas at a temperature of 600 degrees. It never emitted gas of that kind at a lower temperature. He had seen the model, and was of opinion that, with the fire which it appeared was placed under the boiler, it would take the man in attendance eight or ten hours of constant exertion to produce so great a degree of heat as would generate inflammable gas, if he were called on to do so. It was impossible to produce such an effect in half an hour or twenty minutes. Old oil, used again and again, would give an inflammable, but not a permanent gas, at 580. If gas had been formed in a retort, it could not have been confined there, but would have escaped through the pipe. If gas were formed in that court, it would immediately mount to the cupola, where it would remain, unless there happened to be an opening there. In that case it would escape, and the heavier atmospheric air would take its place. All inflammable gases were lighter than common air. There was not, he believed, any gas of an inflammable nature that was not lighter than common air. Looking at the new plan and the old, he had no doubt that the former was less hazardous. He had placed two vessels, one containing a solution of sugar, and the other of oil, in a similar degree of heat; and when the temperature arrived at 350, the sugar produced a species of inflammable gas, which ignited when a lighted body approached it. This could not be done with oil. One great advantage in the new plan was, that the director had a complete control over the heat, which was not so in the other method. The danger attending the boiling over of sugar was immense;

immense; for, of all bodies, sugar produced the greatest quantity of inflammable gas. The oil in the retort was so secured, that it could not have itself taken fire; and if gas had been formed, it must have escaped through the pipe. If a certain quantity of gas could be collected, and suffered to remain in a particular situation, without any means of escape, until it was approached by a lighted body, it would undoubtedly explode, and destroy the whole building. But this could not be the case here. As the house was not air-tight—as there was a regular way by which the gas could escape, if any had been generated—it was impossible that the fire could be attributable to gas. If any explosion had taken place, its effects must have been seen. The oil-vessel, and every thing in and about the retort, would have been destroyed.

Cross-examined.—The oil with which he made his experiments was sent to him by Mr. Wilson. He hoped that some *unadulterated* oil was still to be had. He could distinguish new oil from oil that had been used. The latter was thick and gelatinous. He repeated his experiments several times, and found no difference. Sugar generated gas at 350 degrees.

Mr. W. Allen stated that he was a chemist, and had studied that science for several years. He had looked at the model, and his decided opinion was, that the method employed by Mr. Wilson was by far the safer plan. The reason of the preference must be obvious to all who knew what an inflammable body sugar was, and how easily it became ignited. If fire were placed under it, there was a great risk that it would boil over, and in that case a conflagration was inevitable. By the new method, train oil was heated and conveyed through pipes, by which means the sugar was melted in the most commodious as well as the safest way. One principal reason which induced him to consider this method the most safe was, that the sugar might be brought to boil by the application of heat, from which no danger need be apprehended. He had made experiments with oil, and observed that whale-oil would not give out any permanent or elastic inflammable gas, unless the heat nearly approached to the temperature of 600 degrees of Fahrenheit. It would before that produce a little vapour, but that was often nothing else but aqueous particles which were in the oil. With respect to the difference between new and old oil, he observed that the latter began to generate gas at a little lower temperature than the former. The difference was, however, very trifling, but a few degrees of Fahrenheit's scale. Looking to the construction of that model, and particularly to the gas-pipe, he was convinced that if any person had left the whole apparatus to its fate, and paid no attention to it, danger would not have been occasioned, since

 the

the gas, if any were formed, would be gradually carried off by the tube. It would require a good deal of time, and much labour, to raise the heat to 6 or 700 degrees of Fahrenheit. Such heat would decompose the sugar; and if, in consequence, gas were produced, it would find its way through the steam-vent, and out of the house. If the heat were increased even to 400 degrees, it would be at the risk of decomposing the sugar, and converting that article into gas. Looking to the construction of the machine and of the house, he did not think that any explosion could have taken place by any gas which one apparatus of that kind could have produced. He conceived that the gas from the lamps in the manufactory was infinitely more dangerous than any that could have been generated in the retort. He visited and examined the ruins some time ago, to discover whether there was any trace of an explosion; but he could not perceive a fissure in any part of the premises, or the least appearance of any concussion having taken place. Fixed oils, train-oil, linseed-oil, &c., were so called to distinguished them from the essential or volatile oils which were produced by distillation. The former required a great degree of heat before they could be converted into vapour, while the latter went off in the most rapid manner. There were no two things in nature more different than fixed and volatile oils.

J. Barry, Esq. stated, that he was a practical chemist, and had examined the model before the court. The new mode of sugar-refining was much safer than that formerly adopted. In the first place, it avoided that temperature of heat at which sugar was liable to boil over or to catch fire in the pan. Sugar would boil over at 250 or 260 degrees, whereas oil required 650 or 700 degrees to accomplish that object. He had seen a very inflammable gas formed from oil at the temperature of 565, but not in any great quantity. It required more than 600 degrees to produce any large portion of gas. Mr. Wilson's method of refining sugar was by far the best, because there was no fire under the pans, and the heat could be so easily turned off. If, in consequence of any negligence, gas were generated in the retort, it would not be sufficient in quantity to do any mischief. It would be discharged into the steam-vent, whence it would immediately escape. He knew of no instance of inflammable gas being heavier than common air. Sugar would emit a light gas at about 330 degrees. He found oil, after being much boiled, produce gas at rather a lower temperature than it did when new. New oil produced gas at 585 degrees, old oil at 568.

Cross-examined.—Permanent inflammable gas was not a scientific term. It had not been used by him.

Mr.

Mr. R. Walker, a civil engineer, stated, that he had viewed the premises after the fire, and gave the same account of the state of the retort as Mr. Wilson had previously done.

Mr. C. Sylvester stated, that he had directed his attention to the apparatus exhibited in court, with a view to give evidence that day. According to the old mode of boiling sugar much mischief was risked. The new mode he considered to be much less dangerous. The preventing the fire from coming in contact with the pan must be extremely beneficial. He heard Mr. Wilson's evidence, and did not differ from him in any thing. He was present at certain experiments that were made on sugar and oil by Mr. Cooper. The oil was procured by Mr. Wilson. He observed the alteration of the temperature every five minutes. The temperature increased much quicker in the early part of the operation than towards the end. The heat was 575 or 580 degrees before inflammable gas was generated. The oil that had been boiled produced gas at a temperature of about 20 degrees less than that which was new. He thought it would be exceedingly difficult to produce such a heat by this apparatus. It would require two or three hours, with the utmost care, to raise the temperature so high. If gas were formed in the retort, it would ascend; and there being several apertures, it must escape. At first, it could only accumulate in very small quantities. Constructed as it was, there could be no combustion in the retort itself. Such an event could not be effected by any leaking of oil into the fire.

Mr. J. T. Cooper stated, that he was acquainted with this machine and work, and also with the old mode of refining sugar. He conceived that the new one was much preferable. Elastic gas could be produced from old oil at the temperature of 570 or 580 in small quantities. That was the lowest temperature at which it could be generated: if it were wished to procure it in a large quantity, the heat must be raised much higher. He had never procured permanent inflammable gas under 610 degrees. If gas had been formed in the retort, it would have escaped by different apertures. It would ascend, and could not have accumulated to any dangerous extent. Considering the apparatus, there was no likelihood that gas would have been generated; but if it had, it would have gone off by the pipe. There was a difference of betwixt 10 and 15 degrees of temperature between the heat at which old and new oil would produce gas. After the heat arrived at a certain temperature, it was most difficult to add to it. In 40 minutes he had raised the heat of oil from 20 to 350 degrees, and to raise it from 350 to 600 or 610 degrees occupied nearly two hours and a half more. In the latter minutes he could hardly get it up at all.

 Cross-

Cross-examined.—He did not think that old oil would give gas at 460. He did not know whether the oftener oil was heated the more inflammable it became.

Mr. R. Henley agreed with the last witness as to his experiments with oil and sugar, and was of opinion that the new process was superior to the old.

H. Coxwell, Esq., secretary to the committee of chemistry in the Society of Arts, had made experiments on the relative combustibility of oil and sugar. He was present at those made by Mr. Cooper, and agreed altogether in the account the other scientific gentlemen had given. The new process was, he thought, infinitely less hazardous than the old one. He thought there was no sort of danger in boiling the oil.

Thomas Gill, Esq., chairman to the committee of mechanism in the Society of Arts, also witnessed the experiments of Mr. Cooper, and entirely coincided in opinion with him.

The Chief Justice here observed, that it was useless to call any more witnesses to those facts. They had heard the evidence of Mr. Accum, Mr. Allen, Mr. Cooper, and several other gentlemen well versed in science and mechanics; and to call others to the same points, though it might swell the number of the witnesses, would not add to the weight of testimony.

Mr. Scarlett said, he would show, by a series of experiments made by men of the first eminence, that his clients had a good defence.

Mr. J. Deville and Mr. Blamer were then called: they both agreed in the superiority of the new over the old plan, and deposed to the difficulty with which gas can be produced from oil, and the comparative ease with which it can be formed from sugar.

Henry May was again examined by Mr. Scarlett.—When he called to Muller, he did not know whether the gas-lamps were lighted or not. He heard Muller speak, but he did not see him. He had worked 24 years with his master. Muller had been there about a year and a half. He did not know whether there was any other man in the fill-room with Muller.

This closed the case for the plaintiffs.

Mr. Scarlett proceeded to address the jury for the defendants. It was with unfeigned reluctance that he was obliged to occupy a considerable portion of their time, after the great attention which they had yesterday paid to this cause. He should not, however, trespass longer on their time than was absolutely necessary; for, independently of his desire to consult the convenience of his lordship and the jury, he was under the necessity of being as brief as possible, in consequence of his own indisposition, occasioned

casioned by the great professional labour to which he had lately been subjected in other places. This was in itself an important cause, and its importance was increased by its being one in considering which it was extremely difficult to divest one's self of that natural wish which every well-constituted mind was likely to cherish for the success of a suffering party. He agreed with his learned friend the Solicitor-general, that the plaintiffs were gentlemen of great respectability; and it was far from his intention to impute to them any fraud, or even blame. But that circumstance gave additional importance to a question such as the present, because, when a decision was made which was founded on the principles of law and justice, in opposition to the natural wishes of the heart, it exhibited the triumph of justice over partiality. The defendants also were gentlemen of great respectability, and had no wish to be exempted from any responsibility to which in justice they were liable; there were claims on them, however, as a society, which they were bound by the ties of honour rigorously to discharge. It would evidently be futile for societies to establish rules respecting insurance against fire, if whenever a case occurred these rules were to be opposed as contrary to the claims of justice. He should follow the example of his learned friend in endeavouring to disembarrass the question of technical forms. The points for the consideration of the jury resolved themselves substantially into three questions; and, however widely the matter might be scattered over the record, they would find that on one or all of these grounds the defendants were entitled to their verdict. The first question related to the description of the premises—a point which had hardly been touched upon as yet; and he affirmed that the premises insured had not been properly described. The second question was, whether the fire had been caused by the introduction of a new process, which required the boiling of oil. And the third was, whether this new process increased the risk; which question, though it might be illustrated by the preceding one, was not necessarily connected with it. On the first of these points, he presumed that in this place, and before his evidence, he need not state that a description of premises incorporated their connexion with other buildings. The Solicitor-general had referred to the Building Act; but by that act no communication was allowed between a building of this description and a dwelling-house; and to a warehouse a communication was allowed only by an iron door. In every policy effected on buildings of this kind, it was unquestionably the duty of the persons executing it to state in what manner premises insured communicated with other buildings. The jury would bear in mind that the premises of the plaintiffs comprised three houses—the long-house, the grinding-house,

house, and the new house. The insurance in this case was effected on the grinding-house; and though the fire did not originate in it, the question was, whether they did not all form one building: they were all surrounded by a common wall; they all belonged to one person, carrying on one trade; a pump was worked in this, every species of work was done in that, and there was a mill in another; they all communicated by a door with the house insured. But the buildings were improperly described, because it appeared they communicated with others which were not specified in the policy. He was instructed to say, that the society would not have made this objection to the validity of the policy, if it had only omitted to state the communication between the grinding-house and the long-house; for they had formerly insured the long-house by a separate policy, and therefore knew beforehand that it was separated from the other by an iron door. They therefore would have waved this objection, though in point of law they were not obliged to carry in their memory the descriptions contained in every policy that they had ever underwritten. There had, however, been no former policy describing the connexion between the grinding-house and the new house. The learned counsel then read part of the policy executed on the grinding-house on the 21st of October last, which was merely, he said, a transcript of a former policy, and made no mention of a communication either with the long-house or the new house. To the omission of the former they would not have objected, for the reason which he had already assigned; but he contended that the policy was defective in omitting to state the connexion between the grinding-house and the new house, though it appeared that they were connected by iron doors, and that every story of the one communicated with the corresponding story of the other. His learned friend had omitted noticing this circumstance in his opening speech, reserving it, no doubt, for the conclusion, when the attention of the court and the jury should be fatigued by the length of the trial, and when it might perhaps have some chance of being glossed over. In the printed notices of the society, it was laid down as a regulation, that "extraordinary risks must be insured by a special agreement." Now, this notice was given to the public for the express purpose of inducing them to give correct descriptions of the premises to be insured; and then the society were to be left to judge of the risk. It was evident that, as the risk was increased by the number of stoves on the premises, it was necessary to state the number of these stoves, in order to enable the party insuring to judge of the risk. Whether, if in this case a correct description had been given, competition would have induced the defendants to take the insurance with the additional risk, was a consideration with which the

the jury had nothing to do; the only question for them to decide was, whether a correct description of the premises had or had not been given. In his opinion, the communication between these buildings was most important: and, if they were of that opinion also, they could have no hesitation in coming to a decision on the validity of this policy. With these remarks he should dismiss the first of the questions. He came then to the other two points, which had already occupied so much time. Notwithstanding any impression that might have been made on the minds of the jury by the evidence which they had heard yesterday, he was convinced that the fire was occasioned by the process of boiling oil; and the plaintiffs knew it: they knew it well. Did the jury observe with what great caution his learned friend had laid down the doctrine, that parties were not obliged to account for the origin of fires? He had said, that this fire might have arisen from the lighting of a candle, or from various causes of which no chemist ever dreamed. But why should the jury, when called on to give a verdict, be left to their own conjectures, when positive and important evidence, which it was in his learned friend's power to produce, had not been brought forward? Where was the man who lighted the fire in the morning? Where was the watchman? His learned friend had first examined Duke, a millwright, who was to give the whole description and history of the general mode of conducting the process; and it turned out on his (Mr. Scarlett's) cross-examination of him, that he had never attended for five minutes together, and that he could not tell whether the machine leaked or not. Then the engine-keeper had been called, who at three o'clock found the watchman by whom the fire had been lighted. Now surely, when a fire like this had occurred, the plaintiffs were bound to give all the information on the subject which their servants could communicate, and the jury were bound to derive evidence from a witness not called, as well as from those brought forward. The evidence of May, the engine-keeper, was of great importance. He found that the watchman at three o'clock had lighted the fire. He asked Muller whether he was ready, and whether the fire was lighted under the oil; to which Muller replied, that he was not quite ready. He saw no fire, no light, at a quarter after three o'clock; but he said there was a lamp lighted in the grinding-house, as usual, all the people having to pass through it. In a quarter of an hour afterwards he came back, and Muller said he would be ready in a few minutes; and then there was no token of fire or light in the warehouse. But it did not rest here. At nine o'clock on the preceding night he was the last person who left the premises without any light; at three o'clock in the morning there was no light; at a quarter past three there was no light;

light; and ten minutes after he heard an alarm of fire. He went back, and saw to his left an appearance of flame or fire on the floor. Now the jury, on examining the plan, would see whether a flame from the steam-bin might not easily have been mistaken for a fire on the floor, especially as the witness said he saw only a glimpse of it. But where was Muller? His learned friend in not bringing him forward had exercised a sound discretion, and had taken the only course that could give his clients any chance of success; but he (Mr. Scarlett) had had too much experience of juries in that place, to fear that this circumstance would not have its due weight with the jury whom he now had the honour to address. It was clear that the alarm had been given by Muller, and, by his not being produced, they were called on to believe that the fire did not originate in a place where it was proved by all the witnesses that a fire had existed for an hour before the alarm was given. In further considering this part of the case, he must, in some degree, mix it up with the other question; but he would first observe, that Mr. Wicks had a good deal perplexed the case of the plaintiffs. He had been roused by the rattle of the watchman when the alarm was given; and this watchman would be examined, and the jury from his evidence would be able to judge of the probable cause of the fire. He would suppose that a man in an opposite street saw an immense body of flame bursting through a window on the back part of the premises, and rising in a column to the opposite houses; was it not probable that the fire originated in that part of the building where it was seen? But, whatever might have been the cause of the fire, the jury must be convinced that the plaintiffs had not laid before them all the evidence which they might have produced. He should now endeavour to show that the fire did not originate in the warehouse, but must have proceeded from the apparatus used in the new process; and this brought him to the consideration of the third question—the increase of risk. He wished to speak with respect of all the scientific gentlemen who had been examined yesterday. He wished, however, that Mr. Brande and Mr. Parkes had conducted the same train of experiments as his witnesses had done, and then he had no doubt they would have stated very different opinions from those which the jury had heard yesterday. It was not by one or two experiments that questions in chemistry could be decided, for that science underwent continual fluctuation, and its principles required more patient investigation than those of any other science. In his younger days, what was called the phlogistic theory was very prevalent, in consequence of Dr. Priestley's writings; and the late celebrated Dr. Milner certainly gave very brilliant lectures on the subject, which he (Mr. Scarlett) attended. He probably should have been also led away

away by this theory, had he not happened to form an intimacy with that eminent man, the late Mr. Tennant, of whom he would say, if he might be allowed to go out of his way to pay a compliment to departed merit, that he united in himself more diversity of talent and comprehensiveness of genius than any man of this age. Mr. Tennant told him not to be led away by this theory, for the French were changing the whole nomenclature of the science, and all this phlogistic theory would in consequence be soon overturned. On hearing this, he had desisted from the study, and the result had verified the prediction of Mr. Tennant. Since that time, Sir Humphry Davy had worked another revolution in the names of chemical substances, and perhaps the present generation might be fated to witness another change. This variety of classification and frequent substitution of one defective nomenclature for another, arose from the propensity of men to generalize from particular appearances. It appeared to him that Mr. Wilson was meddling with a subject which he did not understand, when he spoke of the properties of oil. It turned out, on his evidence, that he was the only person who ever applied this principle to the boiling of sugar. In London the plaintiffs were the only persons who used it. Wilson, indeed, wished to insinuate that another house had adopted it; but all they did was, to allow him to make an experiment, and then they rejected the plan merely because it was too dangerous. It was worthy of notice, that Mr. Harris of Liverpool, who had adopted this process, thought it necessary to communicate the circumstance to the insurance-office, although the risk had been proved to be less than from tallow, which he had formerly used in boiling sugar. Now, with regard to the risk of this process, it was said to be less than that of the ordinary process, for two reasons: first, that sugar at 350 degrees of heat emitted gas highly inflammable; and, secondly, that there was danger of the sugar boiling over. On that part of the case the plaintiffs had called only Mr. Robinson, and Mr. Harris from Liverpool; but he would defy his learned friend to produce any sugar-refiner who would say that he had ever seen a sugar-boiler overflowing. Sugar boiled for their purpose at 240, and, if raised to a greater heat, it would defeat their object. Whenever, therefore, it rose higher, they threw water on the fire to damp it; and besides, while the personal security of those attending the process required caution, there would be little danger of the boiler being allowed to overflow. The real risk of a sugar-house arose from the multiplicity of stoves and fires, by which a greater mass of combustibles than was collected in almost any other manufactory was exposed to danger. He defied his learned friend to produce any instance of sugar ever having been boiled to a heat of 350 degrees, unless

 it

in the experiments of chemists. It was alleged that sugar was liable to boil over in an inflammable state, and that, as there was no fire into which it could overflow, the danger was diminished by this new process. This statement had at first view an appearance of plausibility; but on reflection it would be found perfectly puerile, for it reduced the whole danger to the overboiling of the sugar-pan—an accident which no sugar-boiler ever dreaded. On the danger arising from the boiling of oil he was completely at issue with his learned friend. It was important to consider the variations in the evidence on this subject. Mr. Wilson proved roundly that oil of whatever age, and however long used, exhibited no difference in heat. Mr. Accum stated, that when he tried old oil it produced the same effects at 460 as new oil at 600; and Mr. Parkes found the same effects produced at 586 that Mr. Wilson found at 600. He thought the jury would be satisfied from the evidence of Mr. Accum, which was clear and distinct, that Mr. Wilson's theory of oil was not so perfect as he supposed. All those who were accustomed to work in oil knew, that the longer it had been used, and the more frequently it had been heated, the more inflammable it became. The variety was endless; at 310 old oil would produce effects of combustion, which new oil at 600 would not produce. Those who extracted oil from blubber never attained a greater heat than 210; because, beyond that degree it became uncontrollable. It was a substance which was very reluctant to part with its heat, and in a certain state, even some minutes after it had been taken off the fire, it produced destructive effects. The learned counsel then exhibited a phial containing a liquid which had been distilled from new oil. If he were to take out the cork, and apply a light at a small distance, the vapour would flame; but if the light were applied nearer, it would explode, though without noise. He refrained from making the experiment at present, from the disagreeable smell which it would produce in the court; but the results which he had stated would be mentioned in evidence. He particularly directed the attention of the jury to the fact that the explosion was not attended with noise. This distillation had been made from new oil at the heat of 600; but if the oil had been used for twenty-four days, the same effect would have been produced at 410. Those who were engaged in extracting gas from oil were aware of the extreme danger attending the heating of that substance; and that it required a greater degree of skill and caution than any other combustible matter. The company, understanding that this was the case, employed scientific men to make experiments on the subject; and the results of these experiments would be submitted to the jury. They would hear the details of a series of experiments, continued for twelve successive days, on

twenty-

twenty-four gallons of oil, heated to from 300 to 400 degrees. From the evidence of Mr. Faraday, Dr. Bostock, Mr. Children, Mr. Garden, and Mr. Martineau, the jury would be informed of some effects arising from the heating of oil, of which they had not heard before. Besides the aqueous vapour of which they had heard, the oil produced another vapour, highly inflammable and of greater specific gravity than atmospheric air. They would find that in one of the experiments to which he had referred, the oil, at the heat of 460, had boiled up and rushed to the roof, to the height of seven feet. Now, in the apparatus used by the plaintiffs, the vapour arising from the oil was carried off by the vent, which went to the chimney-top; but if that vapour was heavier than the atmosphere, it must descend, and the steam-bin being considerably lower than the vent, it might fall into it; and, being highly inflammable, it might set on fire any combustible substance on the floor. The learned gentleman illustrated this supposition by the familiar case of a smoky chimney, and contended that the same causes which produced the one effect might produce the other. He did not say it had been the case, but it was possible that, on the morning in which the fire took place, the wind might have been in such a direction as by an eddy to have forced the gas down the steam-vent into the bin. His learned friend said the steam-vent had a draught upwards, by the working of the steam-engine; but it was to be remembered that the engine was not at work at that time, and that was a most important fact in the case; Muller had not got the engine to work when the fire took place, and that, no doubt, was the reason why he had not been called. The boiler at first must, on account of the coldness of the sugar, be heated to a greater degree than the average heat necessary during the day; and this being done while the pump was not at work, the mischief he had described was likely to take place. Now, since the fire had taken place at the time Muller was heating the oil—since the pump was not then work—and since Muller, who was the workman employed on the occasion, had not been called—he would leave it to the jury to judge whether this was not the most likely cause of the fire. What he had now said he should substantiate by as great a body of evidence as ever was submitted to a jury; and if the jury should be of opinion either that the fire was occasioned by the cause which he had assigned, or that the danger and risk were increased by the introduction of this new process, or that the description of the premises was defective—on either of these grounds his clients would be entitled to a verdict.

Samuel Willoughby, a fellowship-porter, recollected the morning in which the fire took place. He lived at No. 10, Charles-street, about 100 yards from the premises. A few minutes before

fore four o'clock he came to the corner of the premises, and in a moment he saw a large body of fire, and the window from which it issued flew out. (The witness here identified the window in the model.) If he had been opposite to the window he should never have told any thing about the fire, for the body of fire that burst out would have knocked a horse down. He saw no smoke; he gave an alarm immediately. The flame then ascended the wall of the building. He saw no light on the floor of the house. It was a wet morning when he went out of his house. The first person who came to him was Clayton the watchman, who at his desire sprung his rattle. It was in the first long window in the central part of the building that he saw the fire.

John Clayton, a watchman, confirmed the testimony of the preceding witness. The only person he saw after springing his rattle was Wilson, the private watchman belonging to the house. When he was springing his rattle, he saw Wilson at the door of the Mulberry-tree public-house, taking down a kind of covering he had for shelter. He could not tell what became of Wilson after that. When he looked in at the window, the inside seemed to be all a body of fire. The flame burnt continually, and had nothing particular in its appearance. He heard no explosion.

Cross-examined.—The window from top to bottom was all in a blaze.

Examined by the Court.—Part of the premises lay in his beat. He was up at the building at half-past three, and saw no fire or smoke at that time issuing from any part of the building.

John Young, clerk to Mr. Lockey, a surveyor, deposed, that his master was employed to make a perspective drawing of the premises. Witness was on the spot within three or four weeks of the time at which the fire happened, and was acquainted with the premises. He did not at that time know the connexion between the several parts of the building, but Mr. Lockey did. The grinding-house is near the centre. He understood that the fire broke out in the long sugar-house. He believed there was a communication between the long sugar-house and the grinding-house, but he really did not know whether there was any between the long sugar- and the new sugar-house. He could not say, from his own knowledge, whether there was any connexion between the grinding-house and the new sugar-house. (As it appeared that the witness had no precise knowledge of the premises, his examination was broken off.)

Samuel Lockey, a surveyor, was the next witness called. He stated that he had been employed by the plaintiffs to make a drawing of their premises some months ago. In so doing he had inspected every part of them, and, whilst engaged in this business, had a communication with Mr. Bishop, with regard to a projected

projected alteration. It was a considerable time, certainly above twelve months previous to this, that he had examined the premises on behalf of the Phœnix Company. When Mr. Bishop first mentioned that he was about to introduce a boiler, witness did not conceive that any increased hazard would be incurred. He did not, in fact, exercise any opinion upon the subject, nor had he pointed out any particular spot as best adapted to the purpose. Being employed by the plaintiffs in a private capacity, he was in the habit of frequently visiting their premises. It was at some time during the last spring that Mr. Bishop took him to a door in the building which was not in the plan, when he observed, "I have something to mention between ourselves—I want to put something up here for a new process." Witness was then led to an upper floor, where he was told that it was in contemplation to erect a boiler, and was asked whether in his opinion it could stand there. At first oil was not mentioned, but he understood, before he went away, that the projected boiler was for the heating of oil. He did not form at that time any judgement with regard to the probable risk that might attend it. The only observation which he then made was, that a brick trimmer would be necessary under the boiler. He should have thought that equally requisite, had the boiler been for water. To him the subject presented itself more as a question of curiosity, than of reflection or calculation as to the success of the measure. Had he been employed by the Insurance Company to inspect the premises, and found that an oil-boiler had been newly introduced, he should doubtless have thought it his duty to communicate that circumstance to the office. He had no recollection of making a report at the instance of the Phœnix Office in September 1818. This might be owing to the frequency of his visits to the premises in question. On the occasion to which he had before alluded he was privately consulted by Mr. Bishop, and undertook not to reveal what had been communicated to him. The morning after the fire witness met him, and said, he was sorry to find what had happened, but glad that one part of the building was safe. On being asked for what reason, he replied, "Why, because there you have been trying your new experiments." Mr. Bishop expressed himself in the negative; and in answer to a question whether he had concealed the new process from his partners as well as the office, he said they all knew it as well as himself.

The Lord Chief Justice here remarked, that it was now obvious why the defendant's counsel had preferred calling Mr. Lockey's clerk rather than himself. It arose from a motive of delicacy, and a reluctance to draw forth a disclosure of what had been mentioned in confidence.

In

In answer to a question by a juryman, the witness added, that he was sure as to the words he addressed to Mr. Bishop, "Have you kept this a secret from your partners as well as from the offices?" He asked this, from a fear that he might be blamed by his partners had he been making experiments without their knowledge, and because he concluded, from what had before passed between them, that Mr. Bishop was desirous of keeping the matter secret from the insurance office. The object of this secrecy was, he (witness) believed, to conceal the new process from other sugar-refiners, and that a thought of the increased risk never entered his mind.

William Christopher, a bricklayer in the employment of Messrs. Craven and Beaumont, recollected setting up, under Mr. Wilson's directions, an apparatus for boiling and circulating oil, upon their premises. It was set up on a detached place, and, as he understood, merely as a matter of experiment. It was tried fourteen different times; and Mr. Wilson was absent not above once or twice upon these occasions. The boiler stood upon an arch in a separate building, about thirty feet from the sugar-house. It was twice heated, according to Mr. Wilson's directions, to a temperature of 210, before Mr. Wilson arrived. He himself used to raise it to 300 or 400, in order to communicate the proper degree of heat to the whole mass. The pumping was then begun, and on one occasion the pump was stopped, and the heat rose to 500. The door was then thrown open, and the fire damped. The pipes were of pewter, and broke once or twice when the oil was mixed with sugar. One day, toward the finish, whilst the oil was at 210, the pan was discharged, and the pump being stopped, it rose suddenly to 500. It had not stopped above five minutes before the pipes burst. Mr. Wilson began again to pump, and afterwards proceeded to rake out the fire. The heat, however, increased till it got as high as 600. Witness threw several pails of water upon the fire, but without producing any diminution of the heat. Mr. Wilson and two gentlemen with him then retired, and witness also, when he found the oil continue to come out of the boiler.

Cross-examined.—Mr. Wilson was at this time making experiments. The pipes were not of copper, but pewter. Nothing like a fire was caused by the accident he had described. The thermometer was graduated only to 600, and the temperature of the oil rose to that.

Samuel Wilkinson stated that he was foreman to Messrs. Taylor and Martineau, chemists and engineers, and had been directed by the former in February last to make some experiments in oil. Mr. Martineau, one of the partners, tried a boiler on the 13th with a small quantity of whale-oil. Witness was directed to add a large a large quantity, or about two-thirds more than was at first put into the vessel. The whole quantity amounted to about twenty or thirty gallons. The vessel resembled that exhibited in the model; it was about three feet long, fifteen inches wide, and fifteen in depth. It was made of wrought iron connected with rivets. This experiment was continued only for a few hours. Mr. Taylor wished to ascertain whether common oil mixed with oil previously boiled, would produce inflammable vapour at a low temperature. They could not ascertain the temperature till it came to 80 degrees. A vapour, with considerable noise, began to rise at about 100 degrees; and at 280 degrees a light was several times applied to the top of the tube. The vapour then took fire with sudden gusts, as with an explosion in the boiler. By explosion he meant a noise like that of fat boiling: it burnt only when a light was applied to the tube. When a candle was held to the vapour it assumed an appearance like that of lightning. The thermometer broke at a temperature of 348 degrees. He believed he broke the stem with his hand, but the ball was also broken, and this could not have happened in that way. Witness afterwards received directions to empty the boiler, to clean it well out, and charge it with 33 gallons of new whale-oil, which had never before been subjected to any experiment. This was on the 16th February, and the experiment was continued during twelve days. The fire was generally made about six o'clock, and kept up eleven hours each day. The boiler was always suffered to cool during the night-time. The highest degree to which the oil was ever heated was 507. On the first day the highest was 380, and no inflammable vapour was produced. The tube employed on this occasion was about four feet in height. On the second day the temperature was at 375, and at that degree the air appeared slightly inflammable. On the third day, at 395, there were no symptoms of inflammability; but on the fourth the lowest temperature at which it was produced was 348. At 360 the vapour took fire on the application of a light, and witness then remarked that a difference was created by the slow or brisk burning of the fire. At 380, with a slow fire, the vapour was scarcely inflammable, but with a sharp one was highly so at 360. On the fifth day inflammability appeared at 365, 360, and 345. On the sixth day, the vapour was highly inflammable at 345. The seventh day arrived, and twelve gallons of oil were taken out of the boiler, (the oil in their boiler being deeper than in that indicated by the model,) in order to make the experiment as fair as possible. The highest degree to which it was that day heated was 390, but at 335 it threw out a slightly inflammable vapour. This was the lowest point at which it had been hitherto observed. He was at the commencement desired not to raise the tempera-

ture

ture above 400; but, after the quantity was reduced, he received directions to raise it as high as 500. On the eighth day he accordingly carried it to that point. Inflammability took place at 360, but at 500 he took a rule and measured the vapour, which extended seven inches above the tube. When a light was applied to it, it had the appearance of lightning. The vapour took fire on the ninth day at 497, and the heat rose 140 degrees in fifteen minutes. The fire-place was twenty inches long, ten inches wide, and there was a space of ten inches from the bottom of the boiler to the fire-bars. On the tenth day, the vapour became inflammable at 345 degrees, and spread as before like lightning. At one time on the same day it appeared as inflammable at 360 degrees as at another at 500 degrees. On the eleventh day it became inflammable at 310 degrees. On the succeeding day slightly so at the same point. A similar experiment was continued for twenty-three days, at twelve hours every day.

Cross-examined.—The notes to which he had referred were written by himself at the time of making the experiment. The same oil was used during the whole twelve days, and he did not observe that much was wasted at the end. He could not account for the inflammability taking place at a much lower temperature at one day than at another. He did not make use of an hydrometer, nor remark the atmosphere.

Michael Faraday, esq. chemical operator at the Royal Institution, stated, that he had lately made various experiments on oil; he found that it emitted a vapour denser than the atmosphere. It also threw out gas at a temperature of 340, and caused various combustions before it arrived at that degree. The result of his experiments was, that the use of it, for the purposes of refining sugar, or the circulation of it, when greatly heated, through any medium, must be attended with considerable danger. He had found, too, that sugar would throw out gases at a temperature of 240 degrees. Oil, after distillation, was rendered more volatile and inflammable than before, and the vapour would explode frequently without any noise.

Mr. Richard Phillips deposed that he had had occasion to make chemical experiments on the subject of this inquiry, and he found that a volatile oil was created from fixed oil by the application of heat. Aqueous matter was formed during the decomposition by the union of hydrogen and oxygen. His experiments were made on a retort that contained about twenty gallons. He too was of opinion, that there was great danger of an inflammable gas communicating with the external air.

Dr. Bostock, physician, and lecturer on chemistry at Guy's Hospital, described an experiment which he had seen in Whitecross-street, and which had lasted for twenty-three days. The temperature

temperature of the oil was raised in twenty minutes from 360 to 460, at which point it threw out some highly inflammable vapours, as also jets of flame. He considered the practice of employing it in a boiler to be extremely hazardous; and thought that a conflagration produced by it would exhibit appearances similar to those which were described to have actually taken place at the fire in question. This judgement he formed on the result of his inquiries on a small scale.

This evidence was further confirmed by that of Mr. Children.

Mr. John Taylor, chemist and engineer, was next called, and stated, that he had been in the habit of producing gas from oil. With regard to the possibility of a current of air descending through a pipe or tube, through which the draught generally ascended, it might depend greatly on the state of the wind. A vapour created in the manner referred to would rather coruscate like lightning than explode with any noise. He was acquainted with the ordinary process of boiling and refining sugar, and did not apprehend that there was the slightest danger attending it. He was however of opinion that the new process was extremely hazardous.

Mr. John Martineau and Mr. W. Daniel, both sugar-refiners, also described the mode in which that operation was carried on in ordinary cases, and expressed their conviction that it was not attended with any risk of conflagration.

Mr. Arthur Aikin stated, that he had long made chemical pursuits his study, and about four or five years ago was led to make some experiments on oil. The oil he chiefly used was whale-oil, which was of a viscid and gelatinous nature, and contained a great deal of animal jelly. This, when exposed to a temperature that blackened it, was decomposed, and produced a very volatile and inflammable oil. By distillation this inflammable quality was greatly increased, and, if poured into the hand, it would evaporate like spirits of wine. When a viscid fluid like whale-oil was exposed to the action of naked fire, its carbonaceous substance was decomposed, and formed a stratum at the bottom. There was an accumulation of heat in this stratum, much greater than the degree indicated by the thermometer placed in the fluid above. He thought there must be great danger attending its use, for purposes such as those in question; and the result of his own inquiries perfectly corresponded with the experiments made by preceding witnesses. There was always a risk that the boiler would give way under the pressure of the volatile oil. It was a dangerous and unmanageable fluid; and the more frequently it was subjected to the action of fire, the more volatile and inflammatory it became; insomuch that combustion would be produced at last with a very small degree of heat, and an accident might

 easily

easily happen. He should conceive, that the moment when an explosion was most likely to take place would be that, when, after the volatile oil had been detained by the mass standing to cool all night, the fire was applied on the following morning, before the pump was set to work.

Willoughby and Clayton were then again called, and, in answer to a question from the bench, stated, that when they first perceived the fire breaking out from the premises of the plaintiffs, there was no particular smell, nor did the flame appear of a different colour from that of ordinary fire.

Here the evidence for the defendants closed.

The jury afterwards intimated a wish to have some of the witnesses recalled, in order to put certain questions to them on the same subject.

Mr. Faraday was then recalled, and examined by the foreman of the jury. In his former evidence, he had spoken of an inflammable vapour, not of a volatile oil, as arising from the pipe. That vapour would have burned more or less quietly in proportion to the quantity of atmospheric air mixed with it. It required a proportion of a fourteenth of gas to be explosive. This was the vapour of oil at the temperature of 410. The explosion produced by it would not resemble the explosion of gunpowder. The explosion of gunpowder was violent in proportion to the volume of gas produced by the volume of gunpowder, combined with the rapidity of the inflammation. The explosion of gases might be generally considered in the same way—that was, the proportion of the heated gas to the cold gas before it was fired. The explosion would of course cause a rapid expansion. When heated to a white heat, it occupied three or four times more space than before, and it would expand to that extent in every direction. All the vapour exploded would have its smell destroyed. The smell belonged to the vapour, and not to its result or product. The product of the exploded vapour would have other properties, would new substances, and enter into new combinations with the air into which it exploded. From the rough experiments he had made, he thought that inflammable gas would have the same effect as the oil. The smell, after the explosion, would depend on the quantity of unconsumed vapour which might get about. If the proportion of vapour were small, the flame would be blue; if there was a great quantity of vapour, and more confined, it would burn brighter. The common gas-light was an instance of continuous explosion with the greatest portion of vapour.

Mr. Parkes was next recalled, and asked, by the same juror, whether he differed from Mr. Faraday, and in what? He said he did not concur in all that Mr. Faraday had stated. He did not recollect

recollect to have witnessed such explosions as those alluded to. There might be a smell after an explosion. The smell caused by a thunder-storm he considered to proceed from a mixture of nitrous gas. There generally was a smell in explosive mixtures. He thought the smell would be occasioned by part of the gas or vapour being driven about unconsumed, which would retain its original smell. He did not believe that any inflammable vapour would arise from fixed oil at the temperature mentioned. He thought it impossible that any inflammable gases could have remained in the flues at such a heat as was kept up. They must have gone into the air as soon as generated.

If an accumulation of inflammable gas had taken place in the flues, or in the premises below, where the light was, would it have exploded like gunpowder, or in what degree?—I do not know what experiments others may have made, but I think it impossible that an explosion, as such, could have taken place without a great noise, which must have been heard in the neighbourhood all round.

Mr. Brande was recalled, and asked, whether he differed, and in what, from Mr. Parkes; but it appeared from his answers, that he concurred in Mr. Parkes's opinion.

Mr. Phillips was next recalled.—In case of an explosion of vapour from oil in a high state of temperature, he thought there would be a smell. He would, by way of illustration, say, that the common gas, where it escaped without burning, produced a strong smell, but where it burned there was little or none. If there was any accumulation of vapour, it would emit a strong smell before it exploded. In explosion it would make a noise, but that would depend on the quantity of atmospheric air; there might be no smell after explosion, but he thought there would be a strong one before it.

Henry May recalled, and examined by the foreman. He had been engine-keeper for twenty-four years. It was the watchman who lighted the fire on the morning when the place was burnt. That man was not here. Witness was usually in the engine-house. It was about half-past three when the alarm of fire was given. He had started his own engine before he heard the alarm given. It might be three-quarters past three when he heard the alarm given. When he heard it, he went to pull up the iron door. The fire, when he entered, was on his left. There was not any strong smell, nor anything in the appearance of the fire different from others he had seen; he heard no explosion before the fire, nor after.

Mr. Lockey was next recalled, but nothing material was elicited from his re-examination.

 The

The Solicitor-general now rose to reply. The present case he considered as one of vast importance, not only to his clients, but to the public at large. It must indeed be a most grievous disappointment to them, after all the pains they had taken to preserve their property, after having paid a premium for insurance three times more than was usually given for doubly hazardous insurance, to have now to pay for that which they imagined they had so well secured. But, however grievous the disappointment and loss might be, if there was any point of law against them, if there was any just ground why they should not recover the amount claimed, the jury should give a verdict against them. This, however, must not be founded on any vague speculation, nor on conflicting testimony. The grounds of their verdict should be distinct, and wholly removed from any thing like doubt. Let the jury now look at the case. It appeared that before the plaintiffs had adopted the new process, they had three fires in the same room, where afterwards they used only one. They very naturally conceived (and that such was the fact he would show) that that diminished the hazard. Their conduct subsequently proved that such was their belief; for was it to be supposed that in an establishment where a capital exceeding 100,000*l.* had been invested, and only a part of it insured, that they would have risked an uninsured property of 30,000*l.* or 40,000*l.* by any plan which they knew to be more hazardous? His learned friend had said that the plaintiffs had not given in the policy a proper description of the place insured. To this he answered, that it was well known to the defendants that only a part of the premises were insured, and that such part had a communication with other parts of the building. But before he came to the other points, he would say a word as to the necessity of giving a description of the place near to that insured. He contended, that even if this process was carried on in the part insured, it would not be necessary to give a description of it, unless the risk was thereby increased. But how had the defendants themselves thought of this increased risk? Let the jury look to what occurred with respect to Mr. Harris of Liverpool. He had applied to the defendants for leave to use oil and tallow in his process, and they endorsed their compliance on the back of the policy without charging any additional premium. Was that the conduct of men who conceived the hazard increased by the deviation from the old process? So much for what had been thrown out respecting the policy. But it was maintained that this new process increased the risk, and caused the fire. But in order to get a verdict on this ground, the fact should be distinctly proved, so as not to admit of a doubt. The *onus* of proof lay upon the defendants,

fendants. Now, how did they attempt to prove it? What was the evidence of Mr. Garden—a most intelligent man, and whom every body knew to be so? He considered it a matter of conjecture (the difference between the two modes); but he added, that, if he were to form an opinion, he should think the new process the more dangerous of the two. Was this the kind of evidence which should satisfy the minds of the jury that the risk had been increased, and that the fire had been caused by it? It was not necessary for him to go in detail through all the parts of the evidence. He would look at the plaintiffs' case only in its principal points. The first men in Europe and the world in their profession had stated that they considered the process adopted by the plaintiffs as less dangerous than the former one. It was true that this was met and contradicted on the other side by witnesses highly respectable; but what had the most respectable and intelligent of them said? That, only for the experiments made, they would have conceived the new process as less hazardous than the other. The learned gentleman then proceeded to contend that the old plan was attended with more danger than the other, and cited as a proof the means which were resorted to by several to refine sugar without running the risk of its boiling over—a risk to which the old plan was constantly exposed. Mr. Martineau, for this purpose, had recourse to steam, and, in order to raise the sugar to the required temperature, was obliged to use the high-pressure steam-engine—an engine which was confessedly attended with very considerable risk. It had been also attempted to prevent the boiling over, by one of the greatest chemists of this country, Mr. Howard, who had suggested the boiling *in vacuo*; and at the present moment the defendants were proprietors of a patent for preventing this danger.

Mr. Sergeant Blosset here interrupted the Solicitor-general, and begged to state that he was mistaken; the defendants had nothing of the kind; they were not concerned in any sugar-manufactory.

The Solicitor-general continued. He had made a mistake; but it was one for which the jury would forgive him. He had fallen into it from confounding the present defendants with the defendants in another case. Now, in order to infer the greater risk from the use of oil, what had been said? That oil produced an inflammable gas which was extremely dangerous. What had his witnesses said on this point? That they used common whale-oil, and that it produced no gas under a temperature of 500 or 600. It was said that he had got gas from oil at a temperature of 400, but then that was from oil which had for nineteen months been at several times exposed to heat. Others of the witnesses had

had deposed, that they had got gas from oil at a temperature not much above 300; but, then, how was this done? The oil was got from Mr. Taylor's, and the evidence of Mr. Faraday showed, that in refining oil some sulphuric acid was used, and this would cause gas at a lower temperature than if the oil was unmixed with any foreign ingredients. Would not this account for the low temperature which was produced? and would it be asking too much, after having heard Mr. Faraday's evidence, to suppose that some foreign ingredient was mixed with the oil on which the experiments had been made, and from which gas had been produced? If this were the case, it would reconcile the apparently conflicting testimony on both sides. According to the evidence for the plaintiffs, the oil was at a temperature of 341; and, when the engine was set in motion, it went down 100, because it was cooled by passing through the pipes between the sugar. Was it probable that it would take fire at such a temperature? But suppose the oil had gone through several processes, and been used many times before, what would happen? Why, it would have been thick and black, and would have been almost solid in the cold air; but on examining this oil—and it was taken out for the purpose six or seven days before—it was found to be good, only that it was a little blacker. Was it probable that the plaintiffs would have used oil of so unmanageable a quality as this would be in such a case? Why, it would cool in the pipes at the extremity, and could not be worked. What, then, was the deduction from this? Was it not that the oil used by the plaintiffs had not gone through such processes as to make it give out gas at 350; and that in the oil used in making the experiments there must have been some foreign ingredients? It was stated for the defendants, that the oil used gave out gas at a temperature of 350, and in some instances as low as 310. Why, this was less than the temperature at which the oil used by the plaintiffs was generally kept; and if, as was meant to be inferred on the other side, the gas was coming down and accumulating day after day, it should have exploded in August or September, and not in November. The defendants then, in this instance, proved too much; for, if there was any foundation in the analogy they wished to show between the oil used in the experiments and that used by the plaintiffs, it must have had the effect of causing an explosion long before the time at which the fire took place. The only fair inference that could be drawn from this part of the evidence was that which he had mentioned—that some foreign ingredients must have been in the oil which was used in making the experiments. It was not necessary for him to meet every part of the defendants' case. It would be sufficient to throw

throw a doubt upon it; for, in order to obtain a verdict, there should no doubt remain as to the sufficiency of the defence. He would not, however, rest here: he would show that they really had no case at all to bear them out. It was said that a great quantity of gas escaped daily; but, if this were the case, the whole of the oil must have gone off. The evidence of Mr. Aikin was by no means conclusive; for he could not say at what temperature the volatile oil would be raised. But it was said, that the gas went up and came down again. Why, if this were the case, it must have caused a most noxious vapour, attended with a most unpleasant smell. Yet neither May nor any of the other witnesses found any such smell. Indeed, if it had existed, it would have discovered itself long before the time the fire happened. Mr. Martineau had said, that a current of cold air might have come down the chimney. This could not have been the case; for the fire was fed by external air, and not from the interior of the building. This, therefore, was only a speculation. It was true, if a fire was placed in a vent, fed from within, a current might come down if the air in the room had become much heated; but this would not be the case if the fire were fed with external air. Let the jury see how the case stood with respect to the fire. There was no appearance, no smell which indicated the combustion of inflammable vapour; and, if any such existed, some of it must have scattered about unconsumed, which would emit an unpleasant smell. If it were consumed all at once, there must have been an explosion, which would have been heard; but no explosion was heard, nor was any one symptom noticed which could indicate the presence of inflammable vapour. If the fire had been caused by the vapour, it would have exploded, and set fire to the parts about it; but those who first saw the fire stated that it was not in that part of the building where it would have first appeared if it had been caused by the combustion of inflammable vapour. Independently, then, of all speculation on each side, and supposing that to be equal, let the jury look at the facts sworn to. The place where the fire was seen, the colour, the smell—all of these were circumstances against the case attempted to be made out on the other side. He would now leave the case in the hands of the jury, quite satisfied that their verdict would be a just one.

The Lord Chief-Justice immediately proceeded to address the jury, and, after stating to them the various pleas which had been put upon this record, recapitulated the whole of the evidence which had been adduced from the commencement of the trial. Having finished the reading of his notes, he drew their attention to those points on which, disentangled from the technicality of pleadings, their judgement was to be exercised. The first question

tion would be, whether or not the premises had been accurately and sufficiently described? It appeared that subsequently to the original insurance, and previous to its renewal, a building called the New House, and communicating with that already insured, had been erected. The true way of viewing this question was perhaps to consider whether, had the alteration been made known to the Insurance Company, they would have deemed an increase of the premium necessary. It was, however, for them to determine, under all the circumstances, whether a more minute and extended description was required by any positive agreement, or by the printed terms of the insurance. The next issue which they would have to try related to the manner in which the fire had broken out, and to its probable cause. With regard to this subject, it was not extraordinary to find it involved in mystery and doubt. Calamities of this nature could seldom be traced to their actual origin. They usually took place in the darkness of night, and a hidden spark might produce a conflagration. The fire had taken place within three months after the introduction of a process materially different from that which had been used before, and no fire had ever taken place before this new process was introduced. But then, again, it was for them to recollect—indeed they could not be ignorant—that fires had taken place in many other sugar-houses where the old process was still in use. He now came to the last and most important question on which they would have to deliberate. It was, Did the new process produce an increased risk of fire? On this important point they had heard the evidence, he would not say of the most intelligent, but of as intelligent men in chemical and scientific pursuits as were to be found in this country or in Europe. He had himself read the works of some of them, had derived pleasure from their labours, and entertained the greatest respect for their talents and information. But they had, nevertheless, left the Court in a state of utter uncertainty; and the two days during which the results of their experiments had been brought into comparison, were days not of triumph but of humiliation to science. The constellation of brightness which had shone upon them left them in a state of half-knowledge more full of doubt than a state of perfect ignorance. Those who walked in the twilight ought to proceed with caution; and, speaking for himself, he never would apply the contradictory results of experiment to the real and momentous interests of mankind. It must be matter of general regret to find the respectable witnesses to whom he was alluding drawn up, not on one side, and for the maintenance of the same truths, but, as it were, in martial and hostile array against each other. Volumes had already been spoken; but volumes more must be written before this subject was likely to be elucidated.

For

For the present, he was himself left in a state of the utmost doubt; but in his situation it was permitted to him to continue in doubt. It was the province of the jury to decide, and he begged them to form their own opinion without reference to any that he might be supposed to entertain.

The jury then retired, and, after being absent about half an hour, returned, and delivered, through their foreman, a verdict for the plaintiffs, by which they found, in the first place, that the premises had been correctly and sufficiently described; in the second, that the fire did not originate in the place where the new process was carried on; and in the third, that no increased risk was caused by the introduction of that process.—Damages, 7,200*l.*

XLII. *New Vegetable Alkalies**.

The discovery of morphium having stimulated chemists to search for other alkalies among vegetable substances, two new ones, *brucine* and *delphine*, have been added to their number—the first by MM. Pelletier and Caventon, the latter by MM. Lassaigne and Feneulle.

Brucine.

This alkali was obtained from what the authors call the false Angustura bark (*Brucea anti-dysenterica*) by the following process: A kilogram (32 ounces troy) of the bark was reduced to powder, and treated in the first place with ether, to separate a fatty matter which it contains: the ether being withdrawn, it was then treated with alcohol in successive portions, the infusions were put together, and evaporated to dryness. The dry residue was then dissolved in water, subacetate of lead was added, which precipitated the greater part of the colouring matter, and the excess of lead was separated from the solution by passing through it a current of sulphuretted hydrogen gas. The experimenters being in quest of strychnine, the solution thus purified was acted on by magnesia: the presence of an alkali was rendered evident; but on washing the magnesia, it passed off in solution:—had strychnine been present, it would have remained insoluble. On evaporating the washings, a solid mass, very alkaline, was obtained: it was a new alkali—brucine. To purify it, it was combined with oxalic acid, which formed with it a salt but little soluble in alcohol; it was washed in alcohol till the salt was colourless. The oxalate thus obtained in a fine white powder being decomposed by lime or by magnesia, the brucine is disengaged, which, being dissolved in boiling alcohol, becomes crystallized by spontaneous evaporation.

* From *Annales de Chimie*, vol. xii. p. 113 and 358.

The crystals of brucine are in the form of oblique quadrangular prisms, colourless and transparent; and some of them several lines in length. When quickly crystallized from a saturated solution in boiling water by cooling, it is in bulky plates, somewhat like boracic acid.

Brucine dissolves in 500 parts of boiling water, and in 850 of cold water; when impure, by some of the colouring matter not having been perfectly separated, it is much more soluble. It has an exceeding bitter acrid taste, which remains long in the mouth. When administered in doses of a few grains it proves poisonous, but less so than strychnine. It does not alter in the air, melts at a temperature a little above that of boiling water, without decomposition, and when again cooled appears like wax. When decomposed by oxide of copper it yields only carbonic acid and water, and a small trace of nitrogen, which is thought perhaps accidental. It seems then to be composed of carbon, hydrogen, and perhaps oxygen, but the proportions of its constituents have not yet been ascertained.

With acids it forms neutral and bisalts, which crystallize with facility, especially the latter.

Sulphate of Brucine crystallizes in long slender needles, which appear to be four-sided prisms terminated by pyramids. It is very soluble in water, and slightly so in alcohol. It is very bitter, and is decomposable by potash, soda, ammonia, barytes, strontian, lime, and magnesia; and also by morphium and strychnine, which dissolve in it very readily by uniting with the acid. None of the acids dissolve this salt except strong nitric acid, which alters the brucine itself, and produces, as with strychnine and with morphium, a fine red colour. *A supersulphate* is formed by adding sulphuric acid to a neutral solution of sulphate of brucine. The supersulphate is less soluble in water, and crystallizes more readily than the neutral sulphate. The neutral sulphate yielded on analysis by Pelletier and Caventon,

Sulphuric acid	8·84	9·697
Brucine	91·16	100·000

[The sulphate of strychnine is composed of

Sulphuric acid	9·5	10·486
Strychnine	90·5	100·000

The sulphate of morphium is composed of

Sulphuric acid	11·084	12·465
Morphium	88·916	100·000]

Muriate of Brucine.—Brucine dissolves readily in muriatic acid, and yields quadrangular prisms terminated at both ends by oblique faces. These crystals remain unaltered in the air, and are very soluble in water. When heated to the temperature that affects vegetable bodies it is decomposed, the muriatic acid going off

off in white vapour. It is decomposed by sulphuric acid, and by those bases which decompose the sulphate. Nitric acid has the same effect on it as on the sulphate. Its constituents on analysis are:

Muriatic acid	5·9533	6·331
Brucine	94·0467	100·000

agreeing pretty accurately with the calculations made of its composition.

[Muriate of morphium gave by analysis

Muriatic acid	8·2885	9·0375
Morphium	91·7115	100·0000

Muriate of strychnine gave

Muriatic acid	7·0723	7·6102
Strychnine	92·9227	100·0000

These also agree pretty well with the calculations.]

Phosphate of Brucine.—Brucine dissolves with facility in phosphoric acid. The neutral phosphate does not crystallize. When more acid is added the bisulphate crystallizes readily, yielding large rectangular tables with bevelled edges. This salt is very soluble in water, effloresces slightly in the air, and is soluble in alcohol at common temperatures.

Nitrate of Brucine does not crystallize, but by evaporation forms a gummy mass. The binitrate crystallizes readily in acicular quadrangular prisms, terminated by dihedral summits. These crystals when sufficiently heated inflame and burn, as does binitrate of strychnine. They are readily distinguishable from the nitrate of strychnine, which is a neutral salt. If the nitric acid be added in excess a fine red colour is produced, as with strychnine, occasioned probably by a peroxygenation of the alkali. When either of these liquids is heated it becomes yellow. Protomuriate of tin dropped into the yellow liquid of strychnine causes a dirty-brown precipitate, but dropped into the yellow liquid of brucine produces an intense beautiful purple.

Acetate of Brucine is very soluble, and appears not capable of being crystallized.

Oxalate of Brucine crystallizes very readily in long needles, especially if the acid be in excess.

Brucine, when administered internally, produces tetanus, and acts on the nerves without attacking the brain or injuring the intellectual faculties. Its intensity as a poison is to that of strychnine as one to twelve. It required four grains to kill a rabbit; and a dog having taken three grains suffered severely, but recovered. This alkali appears to be combined in the bark with gallic acid: the bark contains also a fatty matter, gum, yellow colouring matter, sugar in small quantity, and ligneous fibre.

 Delphine.

Delphine.

This alkali is obtained from the seeds of staves-acre (*Delphinum staphysagria*). The seeds, deprived of their husks and rinds, are boiled in a small quantity of distilled water, then pressed in a cloth, and the decoction filtered, and then boiled for a few minutes with pure magnesia; it must then be re-filtered, and the residuum left on the filter: when well washed it is boiled with highly rectified alcohol, which dissolves the alkali, and, by evaporation, it is obtained as a white pulverulent substance presenting a few crystalline points.

It may also be obtained by making sulphuric acid to act on the seeds, unshelled, but well bruised; precipitating the solution by subcarbonate of potash; and acting on the precipitate by alcohol; but when obtained in this way it is very impure.

Pure delphine is crystalline while wet; but being dried by exposure to the air rapidly becomes opaque. It has a bitter acrid taste, melts when heated, and becomes hard and brittle, like resin, on cooling: when more highly heated, it blackens and is decomposed. It dissolves in small quantity in water, but very readily in alcohol: the latter solution renders syrup of violets green, and restores the blue tint of litmus reddened by an acid. With the acids it forms neutral salts, which are very soluble. The alkalies precipitate the delphine in a white gelatinous state, like alumine.

Sulphate of Delphine, when evaporated in the air, does not crystallize, but becomes a transparent mass, like gum. It has a bitter acrid taste, and dissolves in alcohol and in water. It is decomposed in the Voltaic current, parting with its alkali at the negative pole.

Nitrate of Delphine, evaporated to dryness, becomes a yellow crystalline mass; treated with excess of acid it is converted into a yellow matter, little soluble in water, but soluble in boiling alcohol. This solution is bitter, not precipitable by potash, ammonia, or lime-water; and, though not alkaline, appears not to contain any nitric acid. Further quantities of acid do not destroy it, nor does it form oxalic acid. It does not, like strychnine and morphium, take a red colour with nitric acid.

Acetate of Delphine is not crystallizable, but forms a transparent hard mass, of a bitter and acrid taste, and is readily decomposed by cold sulphuric acid.

The *oxalate* forms small white plates, similar in taste to the above salts.

Calcined with oxide of copper, delphine gives no other gas but carbonic acid.

Delphine is, in the seeds of staves-acre, found combined with malic acid, and the following principles: a brown coloured bitter

ter principle, precipitable by acetate of lead; a volatile oil; a fixed oil; albumen; animalized matter; mucus; saccharine mucus; a yellow bitter principle, not precipitable by the acetate of lead; and mineral salts.

XLIII. *On the Sirene, a new Acoustic Instrument designed to measure the Vibrations of Air which constitute Sound. By Baron* Cagniard de la Tour*.

If the sound produced by instruments is owing principally, as philosophers believe, to the regular succession of multiplied shocks which they give to the atmospheric air by their vibrations, it seems natural to think, that by means of a mechanism so combined as to strike the air with the same swiftness and the same regularity, the production of sound may be effected.

Such, in fact, is the result which I have obtained by means of an apparatus constructed by me, of which the following is the mode of operation: The wind of a pair of bellows is made to issue through a small orifice, covered by a circular plate, moveable on a centre placed at a little distance from the aperture. The circular plate has a number of oblique equidistant holes made through it, in a circle round the axis, which passes over the orifice of the bellows: when this plate is made to revolve, (which, by the obliquity of the holes, may be effected by the current of the air, or otherwise by proper mechanism,) the aperture is alternately open and shut to the passage of the air; and thus a regular series of blows are given to the external air, and sounds analogous to the human voice are produced, and more or less acute according to the velocity with which the plate revolves. In place of one aperture many are used, which are opened and shut simultaneously, by which means, without interfering with the height of the sound, its strength is increased.

The instrument is a circular copper box four inches in diameter. Its upper surface is pierced by 100 oblique apertures, each a quarter of a line in width and two lines long: on the centre of this surface is an axle upon which the circular plate turns: this plate has also 100 apertures corresponding to those below, and with an equal obliquity, but in an opposite direction. The obliquity is not necessary to the production of the sounds, but it serves to give motion to the plate by the currents of air. The box is, by a tube, connected with the bellows that supply the air.

In the experiments to ascertain the vibrations for each sound, the plate was made to revolve by wheel-work moved by a weight. The bellows were then used only for the purpose of judging whe-

* From *Annales de Chimie*, tom. xii. p. 167.

ther

ther the sounds of the machine accorded with the notes of a standard instrument, namely, the Harmonica, consisting of an arrangement of steel bars made to vibrate by a bow. Thus arranged, the machine was made to produce the diatonic notes of the gamut, and some beyond them: the revolutions of the plate were ascertained by the revolutions of a wheel, which made one revolution while the plate made thirteen and a half.

The vibrations indicated in the table which follows are the results of preliminary experiments as exact as the imperfection of the instrument employed would admit of: it deserves to be remarked, however, that they approximate very nearly to the theory proposed by Sauveur in his works on Acoustics. I have strong hopes, nevertheless, that I shall be able to push these experiments much further when I shall have completed the apparatus I am now engaged in making, and which, being wholly appropriated to this object, must give results much more exact.

TABLE.

Notes produced by the Sirene.	Number of revolutions made by the wheel during one minute.	Number of vibrations produced in one second by the movement of the plate.	Number of revolutions of the plate in one second.
La	19	427	$4\frac{27}{100}$
Si	$21\frac{1}{4}$	477	$4\frac{77}{100}$
Ut	$22\frac{3}{4}$	511	$5\frac{11}{100}$
Re	25	567	$5\frac{67}{100}$
Fa	30	675	$6\frac{75}{100}$
Sol	34	765	$7\frac{65}{100}$
La	38	855	$8\frac{55}{100}$
Si	$42\frac{1}{2}$	955	$9\frac{55}{100}$
Ut	$45\frac{1}{2}$	1023	$10\frac{23}{100}$
Re	50	1125	$11\frac{25}{100}$

If water is passed through the Sirene instead of air, sound is equally produced, even when it is entirely immersed in the fluid; and the same number of shocks produce the same notes as are done by the air. It is on account of this property of being sonorous in water that I have given it the name of Sirene.

It may not perhaps be improper to mention, that when the Sirene is moved with a certain quickness it produces octave sounds higher than the last *fa* of pianos with six octaves, and much better characterized.

XLIV. *Re-*

XLIV. *Remarks to show that the Nature of the Atmosphere contradicts the Notion that the Air explains the Phænomena of the Sky.*

To Mr. Tilloch.

April 1820.

Sir,—It having occurred to me that some appearances attributed to the atmosphere are contradictory to the known properties of this environing fluid, and my objections having received some sanction from private friends; in the hope that they may receive a timely correction or stronger corroboration from your able correspondents, I have drawn them up for your perusal and most candid criticism. See fig. A. (Pl. IV.)

E the earth, *a a a* the atmosphere.

Were the atmosphere of a like density throughout, its appearance would be the same in every direction, except where *local causes* might effect a partial change in its phænomena; but *these* excepted, the form and colour of the great body of the air would necessarily impose upon the sight equality of distance and of tint on every hand, unless the greater quantity of air from its exterior through the horizon to the eye might lessen the apparent atmospheric distance there, and give its colour strength.

The *greater* density of the air upon the general surface of the earth, compared with its tenuity on mountains, has been most fully proved; and, if it be not transparent in an absolute sense, then much less quantity of atmosphere must be contained in the same measure of distance vertically from the eye than horizontally; and consequently, the lower elevations of the sight could not but discern a nearer and a stronger coloured indication of the atmosphere than could be viewed above.

Sir, yours respectfully,

W. W.

XLV. *On the Alteration which Sulphuric Acid experiences in acting upon Alcohol*.*

In Number 289 of the *Annales de Chimie*, there is a Memoir by M. Dabit on Sulphuric Ether, in which he endeavours to show that the action of sulphuric acid upon alcohol is not confined, as Fourcroy and Vauquelin have supposed, to determining the formation of water, on account of its great affinity for that liquid; but that the sulphuric acid is really decomposed, that it yields a portion of oxygen to the alcohol, without however passing into the state of sulphurous acid; and that it forms a new acid intermediary between the sulphurous and sulphuric,

* From *Ann. de Chimie*, Jan. 1820.

In

In a note printed in the same volume, p. 318, Fourcroy and Vauquelin have endeavoured to refute the theory of M. Dabit on the formation of ether, and they have justly remarked that this theory, were it true, is incompletely demonstrated. M. Dabit had not, in fact, given any proof of the existence of his acid, and his theory might therefore be considered as a play of the imagination. However, two years after he published in the 43d volume of the *Annales de Chimie*, p. 101, a Sequel to his first Memoir, in which he allows that the objection which Fourcroy and Vauquelin had made was well founded; and he replies to it by a series of experiments which do not leave any doubt as to the formation of a particular acid during the conversion of the alcohol into ether by means of sulphuric acid. It is astonishing that these experiments, really very interesting, should remain so long forgotten, and that they should have fixed for the first time the attention of M. Sertuerner, who speaks of them, besides, as if they had never been known. The justice which is due to M. Dabit, as much as the importance of the subject, induces us to give an extract from his memoir: we shall afterwards make known what MM. Sertuerner and Vogel have added to it, subjoining to this notice some observations of our own.

"Having saturated (says M. Dabit) with carbonate of lime some residue of ether, diluted with water, I filtered it and set it to evaporate; I obtained a yellowish salt not crystallized. Having dissolved this salt in a sufficient quantity of water to purify it, and to separate from it the sulphate of lime, I filtered it and set it anew to evaporate. I obtained a salt partly crystallized in parallelipipedal crystals, without much taste, and which dissolved in about a hundred parts of cold water;—hot water dissolves it a little more;—exposed to the air it did not experience any alteration.

"The salt which attached itself to the sides of the vessel during the progress of the evaporation, when it became dry and had acquired a certain degree of heat, carbonized and became acid. When the solution itself was allowed to settle, it presented similar phænomena. Whence could this carbon and acid, which I recognised to be sulphuric acid, arise? The spirit of wine which was found in the solution appeared to me to have been alone the cause; and the following experiment proved that I had conjectured rightly. Some grains of this crystallized salt, with some drops of rectified spirit of wine, steeped in a little water and set to evaporate, yielded the same results. I conceive that the new acid which constitutes the salt of which I speak decomposes, at this degree of heat, the spirit of wine, by abstracting from it the oxygen to pass into the state of sulphuric acid, while the carbon of the portion of the spirit of wine decomposed

composed deposits itself. As there is then no longer enough of lime to saturate the whole of the acid completely, a part remains untouched.

"Some sulphuric acid poured into a solution of new salt produced a precipitate. The acid which is formed during the course of its evaporation causes the same effect. It is clear that the acid of this salt is not sulphuric acid, since it is disengaged from its base by this latter acid; but is this another acid, or is it only a modification, as I have advanced? This is the point to be determined. In order to ascertain the fact, I mixed this salt reduced to powder with the half of its weight of pounded charcoal; and having calcined it, I obtained sulphuret of lime.

"If the first experiment has demonstrated that this acid is not sulphuric acid, the latter proves at least that it is a modification of it. It now only remains for me to prove that the difference of this acid from sulphuric acid is owing to its containing less oxygen.

"Having accordingly passed a current of oxygen gas through a solution of new salt, and having left these two bodies in contact for some time, a precipitate was formed, which I found to be sulphate of lime. In another experiment, having boiled a solution of salt with nitric acid, the nitrous gas was disengaged, and a salt deposited in parallelipipedal crystals, which I ascertained to be sulphate of lime. It appears thus to me well established by these two experiments, that the new acid differs from the sulphuric acid only by its containing less oxygen than the latter.

"The residue of the ether treated with carbonate of barytes, in the same manner as it had been with the carbonate of lime, presented me with the same phænomena; only the salt which is deposited when a solution to which nitric acid has been added is heated, does not crystallize, and the deposit takes place when the liquor begins to boil.

"The salt which I obtained with the barytes was irregularly crystallized; it had a sharp and somewhat styptic taste, dissolving in nearly fifteen times its weight of cold water; it dissolves in about an eighth of hot water.

"In boiling the salt of lime with carbonate of potash, I obtained a bitter salt of a micaceous crystallization, which requires six parts of cold water to be held in solution.

"Salt of soda, prepared in the same manner as the preceding, crystallizes in parallelipipeds, two sides of which are straighter than the others; it dissolves in double its weight of cold water, and with the carbonate of ammonia a very soluble salt is obtained, which crystallizes partly in cubes and partly in octahedrons.

"From this series of experiments it results very clearly; 1st, That the sulphuric acid may lose a portion of its oxygen, without passing on that account to the state of sulphurous acid. 2. That a portion of the sulphuric acid which is employed in the preparation of ether is reduced in that operation; that consequently, the phænomena which have taken place in the course of the preparation of the ether arise as I have described them, and that the theory which I propose is the true one."

These experiments, although they leave much to be desired, are evidently so remarkable, that it was not without reason that we have said, it was astonishing they should have remained so long unknown. We shall now report the additions made by M. Sertuerner.

[To be continued.]

XLVI. *Anatomical Preparations.*

It has been usual to employ for this purpose spirit of wine somewhat above proof, and which costs 18*s*. or 20*s*. per gallon. It has been ascertained by Mr. W. Cooke, of Great Prescot-street, that a saturated solution of muriate of soda (common salt) answers the purpose equally well; and this solution (about three pounds of salt to the gallon) does not cost above 10*d*. per gallon. The process, as the author remarks *, is exceedingly simple.

"It consists (says Mr. Cooke) in putting animal substances (deprived of their blood by maceration in water) into a saturated solution of muriate of soda. Yet, as I believe the transparency of the fluid and the permanency of the preservation depend upon some trivial parts of the management, I shall give the particulars.

"I keep a saturated solution of muriate of soda in good spring water. Every gallon of water dissolves about three pounds of salt.

"The specimens intended for preservation are macerated in water, frequently changed, to deprive them of the colouring part of the blood, which usually occupies three or four days, or more, if the substances are large. I then place them in a solution of salt, kept in a common receptacle, to saturate them with the salt; and, as the water suspended in the animal structure would reduce the strength of the solution, this is counteracted by placing in it a linen bag filled with the muriate. It is kept in a linen bag to prevent the foreign matters of the salt from soiling the preparations. At the expiration of a few days they may be transferred from this vessel, into that bottle of solution which is to contain them permanently, or they may remain in the inter-

* Transactions of the Society of Arts, &c. for 1819. The Society's Silver Medal was voted to Mr. Cooke for this communication.

mediate

mediate vessel as long as conveniency may require. The solution in which they are finally to be kept should be a very little below saturation. Evaporation is much less liable to happen than if spirit is employed; yet should it occur, from any accident, crystallization would result, unless this precaution were adopted. I have added about half an ounce of very clear water to a quart of the saturated solution.

"All kinds of animal structure do not exhibit the same relative gravity when saturated with the solution; some will float whilst others sink. In the final operation of preservation, the latter undergo the same treatment as is adopted with spirit, viz., by supporting them with delicate threads of silk; but the former require some *body* to give them an increase of gravity.

"In almost all preparations it is requisite to keep certain parts separated by means of quill, bristle, or other insoluble substance; instead of these, I use delicate pieces of glass, which may be obtained from the glass-blowers of every degree of fineness. These answer fully; they look neater than any thing else; can be applied with great facility; render sufficient weight, and are of very little cost; for a few pence I purchased enough to last some years. In the specimen of ulcerated intestine, the glasses applied [as stated in the former letter] were not intended to be permanent; I had not then procured the glass from the manufacturer. Membranous parts it is customary to spread on talc, as two of the specimens are applied now with you. I think, however, by suspending a fine cylinder of glass at the bottom, the part retains a more natural appearance; and, therefore, of late, I have entirely relinquished the talc.

"When the specimens are put into a solution of proper strength, and perfectly transparent, and the parts properly separated, to exhibit the different objects it is designed to show, the next point of importance is to close the bottles with accuracy and neatness.

"For the reasons already stated, glass, with a medium of resin, was chosen. I make the rim of the bottle perfectly dry, and spread on it some resin, with a common spatula. The glass (previously fitted) is then applied, and by holding the warm spatula pretty close to it, the resin softens, and the top is fixed with the utmost accuracy. I have thought it best to conduct this process in a moderately warm room, and the glass should be rather less than the circumference of the rim, or on lifting the bottles by the top, as is very generally done, the glass might be raised. With small bottles it is sufficient to make the resin smooth, and then varnish it; but on bottles of a larger size, additional security is obtained by attaching a slip of bladder around the edge, by means of glue.

"When the diameter of the bottle is large, the glass top is liable to break after two or three weeks; by using the *eye* of the glass, I have found them perfectly safe, with a diameter of four inches; and for larger I have not had occasion.

"In addition to the specimens mentioned in my former letter, I might have named fœtuses at the full period of gestation.

"Having been accustomed to put up anatomical preparations, I do not hesitate to affirm that the method I have now detailed, is as easy of application as any other in popular use; and I believe 10*d.* per gallon would not only pay for the salt, but all the other little expenses, with the exception of the bottle.

"Many parts of morbid structure are preserved much better than by spirit."

XLVII. *Notices respecting New Books.*

A new System of Shoeing Horses, with an Account of the various Modes practised by different Nations, more particularly a Comparison between the English and French Methods, and Observations on the Diseases of the Feet connected with Shoeing. By JOSEPH GOODWIN, Veterinary Surgeon to His Majesty, &c. 8vo. pp. 309.

MR. GOODWIN rests his claim to attention on the important part of the veterinary art, of which he treats in this work, on the best of all foundations, long experience. For twenty years he has been extensively occupied in attending to the management and diseases of horses of all denominations.

The system of shoeing horses which Mr. G. recommends is in direct opposition to the common method employed in England. The circumstance which led him to adopt it, he thus relates:

"Several years ago, when cavalry-officers and others, who had been on the continent, were going through Carlton-House stables, I repeatedly heard them observe, that the proportion of lame horses in France, compared to the number in England, was quite trifling. This induced me to neglect no opportunity of inquiring into the cause of so important a fact: I frequently asked if there was any difference in their method of shoeing, or any other circumstance, which contributed to this desirable object; but I could not acquire any distinct information. However, when the communication between France and this country was opened in 1814, an opportunity presented itself. Two horses were then brought from the king of France's stables, both of which were aged, and had previously undergone several years of hard work. Their feet were very perfect in form, and in good order, and they

they are so at this time, and have continued so ever since they came to this country, notwithstanding they have done much work. I do not wish to make it appear that a correct notion was to be formed of the French principles of shoeing by two examples; but they afforded me an opportunity of observing their method in a general manner. I soon perceived that there was not any similarity whatever in the form of their shoes, or in their principles of shoeing, with the mode practised by the English; I therefore began to suspect that the plan of shoeing horses in France would sufficiently explain, and would eventually prove, the truth of the general observation, that I had continually heard repeated for years, viz. that lame horses were seldom to be seen in that country.

"I was most desirous to make a trial of the French system; and having obtained permission for that purpose, I selected some post-horses belonging to the private stables at Carlton House, that could not be kept at work when shod upon the English principle; it soon appeared that they improved in their feet, and went to work. Others were shod in the same manner with the same success.

"Two years elapsed, the new system of shoeing being attended with continued success; and I anxiously availed myself, during this period, of every opportunity to become acquainted with the reasoning of the French veterinarians on this important subject.

"I found it consistent with every view I had entertained; and I have now great satisfaction in making it publicly known, that after a trial of the system for several years in this large establishment (the royal), consisting of horses used for the different purposes of the road, posting, town-work, and hunting, its beneficial effects are so manifest, that I have no hesitation in pronouncing it to be the best possible practice as yet discovered."

The work is clearly written, and contains a great deal of curious and important information. We extract the following as no unfavourable specimen. The author is speaking of the nerve operation.

"It may, I think, fairly be considered as an era in the Veterinary Art. In cases of lameness, hitherto considered as admitting of no relief, viz. all those permanent diseases of the feet which have been so frequently adverted to, it has been proposed by Mr. Sewell of the Veterinary College, to divide, or rather to amputate a part of the nerves which go to the foot. This operation is, on some occasions, performed below the fetlock joint on both sides, and on other occasions above the joint. The success that has attended this novel and great discovery, most decidedly

dedly claims not only the praise of every professional man, but of the public at large.

"After a number of successful cases resulting from this operation, there were persons that attempted to take away the merit of the discovery from Mr. Sewell, by stating that Mr. Moorcroft had performed the operation twenty years ago. Admitting that as fact, what does it show? That no beneficial result originated from Mr. Moorcroft's attempt; for if there had, we should have heard of it either from him or his successor. It therefore appears, that if Mr. Sewell had not shown its utility, we should never have heard of Mr. Moorcroft having performed it. I have also heard Professor Coleman say that he performed the operation more than twenty years back; but when he said so, he by no means intended to lessen the credit due to Mr. Sewell; on the contrary, he imputed blame to himself for not persevering in it. I have heard there are other persons who have attempted to insinuate a sort of claim as being the suggestors of this operation. They are, however, in my opinion, all without any foundation in truth. I have considered it necessary to make these remarks, to oppose any insidious attempts to rob Mr. Sewell of the fame he has so justly acquired. Some evils certainly attend the performance of this operation; but I look at them as I should on the explosion of a powder-mill, a steam engine, a gas apparatus, or any serious evil which may occasionally attend any useful and important invention; and however such accidents may be lamented, powder and gas will be made, and steam applied as a mechanical power.

"The fair way of balancing the account is, to put the evils in one scale and the advantages in the other, when it will appear which preponderates; and as far as my observation goes, and from all the information I have, it is greatly in favour of the latter. The evils I allude to are the loss of hoofs, consequently a loss of the animal. Two cases which occurred at the Royal stud may serve to illustrate others: a mare seven years lame, and another five, were operated on for experiment. They both became sound; but about nine months after the operation, there appeared in one case a trifling separation between the horny and sensible sole at the toe. This increased daily, notwithstanding all my endeavours to subdue inflammation; it then extended to the coronet, and at this there was considerable inflammation and swelling of the leg: the separation increased round the coronet, and in a short time there was a separation between the horny and sensible laminæ. I should have rather said insensible laminæ, as there was not the least sensation in the foot at this time. In this state the coffin bone was let through the hoof, and was fractured in many pieces. The other case was very similar to this, and I have heard of other cases, where

where the animals, as it were, walked or stepped out of their hoofs' The progress and result of these cases are very distressing; but on any future occasion, and where a similar fate appears inevitable, the slaughter-house is the only humane course for adoption. I have heard of many similar cases in different parts of the country; but, on the other hand, I see many fine and valuable horses at work, that were of no use to the owner before the operation, and have continued at severe work now two years without any return of lameness; and I see no reason why they may not be as effective as they now are many years to come. It therefore becomes a question with the owners of lame horses, where no other remedy offers to be of any advantage, whether they will work them lame, sell them for little or nothing, or risk the chances of the nerve operation. A determination of these alternatives will much depend on the kind of work a horse under these circumstances may be required to perform. If a lameness is not severe, a horse may do slow draught without having recourse to the operation; but, on the other hand, if a horse is required for quick draught or for riding, I conceive there are few persons that would not risk the chances of the operation. With stallions and brood-mares there can be no necessity to risk it, except in extreme cases of lameness. The two instances mentioned of failure in brood-mares that had been some years previously lame, but on becoming sound again never performed any sort of work, proves the experiment to have been made under every favourable circumstance; it however shows, that success much depends on the state of the feet previous to the operation.

"In lameness of very long standing, where a change of structure has taken place to any considerable extent, and more particularly on the state of the joints within the foot, if the disease does not amount to a destruction of a joint, but a secretion of the synovia or joint oil has been stopped, and the cartilaginous covering of the ends of the bones is in a state of erosion, the animal is enabled, by the loss of sensation in the foot, to bear a greater degree of weight on these parts than they have been for a long time accustomed to; by which means are brought on all the distressing symptoms previously described. It therefore appears that the chance of success depends much on the length of time the disease may have existed previously to the operation. With horses with thin flat feet and weak laminæ, the operation is not so likely to succeed as with those where horn abounds. The structure of the horse's foot is particularly favourable to the success of this operation, as there are no muscles within the horny box; therefore, as the powers which put the foot in motion are situated at a distance, a division of the nerve going to the foot has no effect in paralyzing any parts which are necessary

sary to its motion; which might have been the case had it been otherwise constructed. It has been imagined, that the foot, after this operation, would become a mere block, and that the animal would be rendered dangerous to ride or to drive. On making close observations on this point, I am glad to find that it exists only in the imagination, as horses that we have now at work, who have undergone the operation nearly two years, are considered quite as effective, in every respect, as those which have not required it. It has also been stated, that the hoof and foot waste after the operation; but I am glad to know that this observation is imaginary also. I have observed in hoofs where the operation has been performed a more plentiful production of horn than before, and a greater disposition to grow in the natural form; and I think this is a consequence to be expected, as the irritable state of the foot being destroyed by the division of the nerve, the secretion is more likely to go on freely. When the operation has been performed before any considerable change of structure has taken place, I have no doubt that the original cause is in time removed altogether, by the animal being enabled to bear the full proportion of weight on the lame foot; as I have shown, that if the foot, from any cause, is long kept in a state of relaxation, the horn soon contracts, and tightens on the internal contents, and hence arises the utility of the patten shoe being applied on the sound foot in cases of lameness.

"It is useful to know that where the nervous influence has been restored, and lameness in some cases has returned, a repetition of the operation has been attended with the same advantages as in the first instance.

"The operation itself is very simple. Having first ascertained the course of the artery by the pulsation, an incision is made through the skin, about an inch and a half or two inches in length; and on removing the cellular substance, the artery, vein, and nerve are all shown running together, the nerve on the inside of the artery and close to it. A needle may be readily passed under it, leaving a thread, which, on pulling a little, enables the operator to separate the nerve from the artery with ease, and to take out as much as is considered necessary; after which the skin should be closed with a stitch, which sometimes heals by the first intention; after which bleeding and physic are useful."

A New Geological Map of England and Wales, with the Inland Navigations; exhibiting the Districts of Coal, and other Sites of Mineral Tonnage. By WILLIAM SMITH, Engineer, 1820; on one large sheet, neatly coloured and shaded. Cary, St. James's-street.

The map before us is of unquestionable utility, and should have a place a place in the office or counting-house of every professional man, merchant, or considerable trader in the kingdom; to say nothing of the study of the curious in geological researches, to whom its facts regarding the *stratification* of our country, are more particularly addressed. Here, for the small sum of 14*s.* will be found embodied all that is most generally useful and interesting, as the results of Mr. Smith's unwearied labours, commenced in the year 1792, regarding the localities of the favoured districts of our Island, producing its *mineral treasures*, as coal, limestone, ironstone, lead and copper ores, freestone, pottery clay, &c. &c.; and a delineation of the unparalleled means which British enterprize, capital, and skill have provided, for conveying of these treasures from the places of their production to their places of consumption, viz. rivers rendered navigable, canals, railways, and turnpike roads.

Smith's Geological Atlas, No. 3, published by Cary, St. James's-street.—This number of a work unparalleled in its objects, viz. that of exhibiting on separate maps the Geology of the several counties of England and Wales, contains the counties of *Oxford*, *Buckingham*, *Bedford*, and *Essex*, each on a scale sufficiently large, and so well and neatly filled up by the engraver, as to contain most of the smaller villages, roads, streams, &c. and thereby identify the localities of all the principal strata, and their tortuous lines of parting on the surface, in a far more correct and satisfactory manner, than has heretofore been done in any of Mr. Smith's geological publications; and such as may challenge comparison with the imitative productions of any other individual, however powerfully his pretensions may be supported. Any of the maps in this Atlas being sold separately, every one has now the opportunity, at a very trifling cost, of making himself conversant with the geology of his own particular county, or of any district of the kingdom wherein he may occasionally reside, or feel an interest.

An Introduction to Mineralogy; comprising the Natural History and Character of Minerals, and a Description of Rocks, both simple and aggregate. By Robert Bakewell, Author of an Introduction to Geology. Plates. 8vo. 21*s.*

Notices, illustrative of the Drawings and Sketches of some of the most distinguished Masters in all the principal Schools of Design. By the late Henry Reveley, Esq. 8vo. 12*s.*

A New System of Cultivation, without Lime, Dung, or Summer Fallows, as practised at Knowle Farm, in the County of Sussex, by Major-General Alexander Beatson, late Governor of the

 Island

Island of St. Helena, and Hon. Member of the Board of Agriculture. 8vo. 9*s*.

An Account of Timbuctoo and Housa, Territories in the Interior of Africa, by El Hage Abd Salam Shabeenie, a Native of Marocco, who personally visited and resided as a Merchant in those interesting Countries. With Notes critical and explanatory. Edited by James Grey Jackson, late British Consul at Santa Cruz, South Barbary, &c. &c. &c. To which are added, Letters descriptive of several Journeys through West and South Barbary, and across the Mountains of Atlas; personally performed by Mr. Jackson, between the Years 1790 and 1805. Also, his Translations of several very interesting Letters in the Original Arabic, from Muhamedan Potentates to Christian Kings, exemplifying the peculiar Phraseology of that Oriental Language; African Anecdotes, Fragments, &c.

Preparing for the Press.

A Grammar of the Arabic Language. By James Grey Jackson, Professor of Arabic; late British Consul at Santa Cruz, in South Barbary, &c.

XLVIII. *Proceedings of Learned Societies.*

ASTRONOMICAL SOCIETY.

April 14. The continuation of Dr. Pearson's paper on a new *Micrometer* was read; and illustrated by some specimens, which were laid before the Society. It is principally adapted to the measurement of very small angular distances, such as the diameters of the planets, the distances of double stars, &c.; and promises to be a valuable addition to the apparatus of every practical astronomer. There is no instrument, at present, which will measure such small objects so accurately.

XLIX. *Intelligence and Miscellaneous Articles.*

CONITE.

Blackheath, April 2.

Sir,—In my account of the Western Isles of Scotland I have described a new mineral by the name of Conite; that name having been conferred on it from the powdery form in which it occurs. As it is there mentiuned as having been found as yet, only in Mull and in Glen Farg, it will not be uninteresting to your mineralogical readers to know that I have since discovered it in the Kilpatrick hills, in the trap of which that district consists, and also in Sky.

I have

I have little doubt that it will be found in similar circumstances elsewhere, now that it has once been pointed out to the attention of mineralogists. I am, sir, your obedient servant,

J. MacCulloch.

P.S. I may add that the aptitude of the term to the most striking feature of this mineral, has induced me to retain it; since, although the same name has been applied by Prof. Schumacher to a very different substance, I do not consider that this is likely to maintain its place in our catalogues of mineral species.

ANTIQUITIES.—EMERALD MINES.

M. Caillaud's account of his discoveries in Egypt will shortly be published in Paris. Some time ago he discovered near Mount Zabarah, the famous emerald mines which were previously known only by the writings of the ancient authors, and the stories of the Arabs. They had been almost forgotten for a long lapse of time, and were totally unproductive to the government of the country. They were discovered by M. Caillaud nearly in the same state in which they had been left by the engineers of the Ptolemies. He penetrated into a vast number of excavations and subterraneous canals, some of which are so deep that 400 men may work in them at once. In the mines were found cords, levers, tools of various kinds, vases, and lamps; and the arrangement of the works afforded every facility for studying the ancient process of mining. M. Caillaud himself set about working the mines, and he has presented six pounds of emeralds to Mahommed Ali Pashaw. In the vicinity of the mines, the ruins of a little town have been discovered, which in ancient times was probably inhabited by the miners: among the ruins are the remains of several Græco-Egyptian Temples with inscriptions. M. Caillaud has twice visited Zabarah; during his second journey he was accompanied by a considerable number of armed men, miners and workmen, whom the Pashaw had placed under his directions. On his way to the emerald mines, the French traveller crossed one of the ancient routes for the trade of India, by the way of Egypt. He observed stations, enclosures for the union and protection of caravans, cisterns, &c. M. Caillaud learnt from the Arabs of the tribes of Ababdeh and Bycharyn, that this road led to the ruins of a very extensive town, on the banks of the Red Sea, situated about the 24th degree of latitude, near the mountain of Elbé. This town has since been visited by MM. Belzoni and Bitche, and will probably be better described by them than by M. Caillaud. On the banks of the Red Sea, the traveller discovered a mountain of sulphur on which some diggings had been made; in the neighbourhood of this mountain, traces of volcanic eruptions were observable, and a quantity of puzzolane and other igneous substances were found. M.

Cailliand carefully observed the mountains which separate the Nile from the Arabian Gulf, as well as the calcareous tracts of ground, and chains of mountains between the Nile and the Oasis, which all belong to the primitive soil. Here he examined several ancient Egyptian structures, and others of more modern date; he discovered several very ancient vaults, thermal springs, &c. Among the Greek and Latin inscriptions which he met with in his excursions, was one containing 70 lines, and about 9000 letters; it is more copious by at least one-fifth than the Greek inscription on the Rosetta stone. By dint of vast patience and labour, M. Caillaud succeeded in copying this incription in three days. Though it is of recent date compared with the Rosetta monument, since it belongs to the age of the Emperor Galba, it presents some new and curious facts relative to the internal administration of Egypt. M. Caillaud returned last year to Paris, bringing along with him a vast number of drawings, notes, and antiques, found principally in the hypogeá of Thebes, &c. These treasures have been purchased by the French government. The antiques are deposited in the cabinet of medals and antiques of the king's library, and the drawings will be engraved and published with descriptions in two vols. folio. M. Caillaud has again set out for Egypt. In November last he was at Bony-Souey, 25 leagues from Cairo. He was about to depart for the Fayoum, and to proceed towards the Oasis of Sivah. He must ere this have made many new and interesting observations. At a quarter of a league from one of the pyramids of Sakkarah, he descended into a hypogeum sacred to the deity Apis, where he found, in a kind of labyrinth, several bulls embalmed and preserved like mummies.

It should be remarked, that M. Belzoni had performed the same journey not long before; and perhaps had discovered this same sepulchre of Apis, in company with Mr. Beechey (son of Sir William, the painter), whose name the French writers most unpardonably mangle, by writing it *Bitche!*

DISCOVERY OF CICERO'S TREATISE DE REPUBLICA.

The following letter, dated December 23, 1819, from the principal Librarian of the Vatican to the Pope, giving an account of the discovery of Cicero's Treatise *de Republica*, has excited great expectation; and though the writer may be too sanguine as to the possibility of deciphering the whole, there can be little doubt that what is actually gained will be a valuable addition to classical literature. (Copy.)

"Most blessed Father, first kissing your sacred foot, I have the honour and satisfaction to inform your beatitude that my studies

studies in the Vatican library, in which I preside through your sovereign clemency, have been encouraged by signal success.—In two re-written codices of the Vatican, I have lately found some lost works of the first Latin classics. In the first of these MSS. I have discovered the lost books *De Republica* of Cicero, written in excellent letters of the best time, in three hundred pages, each in two columns, and all fortunately legible. The titles of the above noble subject, and of the books, appear in the margin: and the name of Cicero, as author of the work, is distinctly legible. A composition of the middle ages having been again written upon this MS. the original pages have been misplaced, and even mutilated; notwithstanding this, a great part remains. The moral and political philosopher, the legislator, the historian, the antiquary, and the lover of pure latinity, will naturally expect, with impatience, the publication of this important work of Cicero, so long lamented as lost. I shall lose no time in preparing it for press, and in submitting it to your holiness's inspection. The other re-written codex presents various and almost equally precious works. It is singular that this MS. contains some of the same works which I discovered and published at Milan, and I have here found what there was wanting. I perceived this at first sight, not only from comparing the subject, but also from the hand-writing, which is precisely the same as that of the Milan MS.

"The contents are—1. The correspondence between Fronto and Marcus Aurelius, before and after he was Emperor. This is an instructive, affectionate, and very interesting collection; the first and second books, containing epistles to M. Aurelius, were published from the Milan MS.; that now found in the Vatican contains the third, and fourth, and fifth books, as well as the supplement to the second, and some other works by Fronto, in Latin and Greek.—2. The fine commentary of the inedited scholiast on Cicero, begun to be published by me at Milan, and now to be increased by five other orations, with the supplements to those already printed at Milan.—3. A fragment of an oration by Q. Aurelius Symmachus, with the supplement of two by the same author, already published by me.—4. The supplements to the Homily, or Gothico-Ulphilan Commentary, a portion of which was also found at Milan, together with an essay of Ulphilas. These valuable works mixed into two volumes, which were taken for writing parchment in the middle ages, were sent partly to Rome and partly to Milan, from the convent of St. Columbanus at Bobbio. They will now be again united in a Roman edition of them, which I shall lose no time in publishing. I will not now request your attention, most blessed father, to some other fragments

fragments of those same codices, although they are worthy of publication. May I be permitted to express my joy, &c.

(Signed) "ANGELO MAI,
"First Librarian to the Vatican."

Cicero composed his Republic (to which the above letter refers) in imitation of Plato. It is alluded to and quoted by St. Augustin, Lactantius, and others. The fragments that have come down to us were published by M. Bernardi in two volumes 12mo. 1807, with a dissertation on the progress of the arts and luxury of the Romans.

THE INTERIOR OF AFRICA.

The Marquis d'Etourville, who is at present in Africa on matters of private business, intends, on his return to France, to publish some interesting notices relative to natural history, a science wherein he has made numberless discoveries, and such as well deserve the attention of the learned. He has recently forwarded certain memoranda which he made during his long captivity, of which the following is a very brief analysis.

M. d'Etourville emigrated from France to Spain in 1790; he there commenced a course of medical studies, and afterwards resided some time in Lisbon; taking lessons in that science. From Lisbon he repaired to the isle of St. Thomas, situated under the Equator, at the extremity of the gulph of Guinea. He remained some years in this island, whence occasionally he made excursions into the western regions of Africa. In one of these, he fortunately cured some dangerous wound under which the Manicongo, a prince of the country, was suffering. Having thereby gained the favour of the prince, he attended him in an expedition or journey more than four hundred leagues in the interior of the continent.

In the course of this peregrination, M. d'Etourville traced on a map the western lines of the lake Aqualinda, respecting which, till then, no certain information had been obtained. He likewise ascertained with precision the geographical route of the Zaire, with its sources, and the lakes it forms in its progress.

In a journey which he undertook in 1800, M. d'Etourville was taken prisoner by a wandering tribe of Gijas, who are cannibals. Whatever common fame has reported of their ferocity, is no exaggeration. They make war to devour their prisoners; and it is certain, as Dopper relates, that human flesh is sold in their markets. The blood which they draw from the veins of their living victims, is to them a delicious beverage. M. d'Etourville remained fifteen months among these barbarians. All his companions were devoured; and he must have shared the same fate,

fate, had he not been so fortunate as to cure a broken arm of the favourite mistress of the chief of the horde.

Compelled to be in the train of this troop of Gijas, he ranged through an extent of continent from the country of the Auriscans to Hulla, when he escaped from their hands. He then proceeded to a province south of the western Mountains of the Moon, at a small distance from what he considers as the real sources of the Nile. Hereabout he fixes the empire of Droglodo, unknown at present, but far more civilized than the circumjacent regions. The politics of the government, according to M. d'Etourville, bear a strong resemblance to the Chinese, and the civilization of the Droglodians must be traced to a very remote source. The merchants of Droglodo go, once a year, authorized by their government, to meet the Abyssinian merchants in a narrow passage of the mountain Narcar. They convey thither gold dust, musk, pearls, precious stones, ivory, gums, and Ethiopian slaves, in exchange for which they receive shawls, Indian stuffs, Turkey carpets, and salt.

In this country M. d'Etourville remained about ten years; and though in a state of slavery, he had many opportunities of noticing the manners of the people and their antiquities. His different observations have led him to conclude that the Abyssinians, the Nubians, and the ancient Egyptians, who built the pyramids, were all originally from *Droglodo*, which he conceives to have been the country inhabited in ancient times by the *Troglodites.*

M. d'Etourville returned to France about the time of the re-establishment of the Bourbons; but set out again, in 1814, to realize and secure some goods and property in Africa, whence he is expected shortly to return, and when the full account of his travels may be expected in the Journal of New Voyages and Travels.

NEW VOYAGE OF DISCOVERY.

Advices from St. Petersburgh, dated March 22, state that a new voyage of discovery will be undertaken this summer in the North. The expedition will sail from the mouth of the Lena for the Frozen Ocean, in order to examine the coast of Siberia and the islands which were discovered to the north of it some years ago. As it is not yet ascertained whether these supposed islands may in reality be one main land or not, and as hitherto they have only been visited in winter, it will be interesting to know how far the ice will permit vessels to advance during summer, and to determine its extent.

ANCIENT NAVIGATION.

A discovery was recently made in the environs of the Cape of Good Hope, which is highly interesting to history. While digging a cave, the workmen found the hull of an ancient vessel construct-

ed

ed of cedar, which is believed to be the remains of a Phœnician galley. If this appropriation be just, there is no longer room to doubt that the bold navigators of Tyre had reached the south point of Africa: and if they actually gained that point, we may infer that they navigated also the eastern oeean.

EARTHQUAKE AT CORK.

Between two and three o'clock on Tuesday morning, April 6, a shock of an earthquake was sensibly felt at Cove, Ahada, Middleton, and the neighbourhood of the harbour's mouth. At about half after two o'clock a noise was heard like the rumbling of a heavy carriage, accompanying which was a very sensible shaking of the house, bed, and furniture, which lasted about eight or ten seconds: during the time of its continuance, some of the servants of the house state that they with difficulty preserved themselves from being shaken out of the bed; in particular a nurse, whose attention to an infant rendered her more liable to such an accident. Immediately after the shock, water was dashed against the bed-room windows, in such quantity as to convey the notion that it was thrown at the glass out of vessels. To some persons, the noise accompanying the shock resembled the report of a great gun.

At Haulbowline island the sensation was very terrific. The watch-house there is built with immense masses of limestone, many of them weighing some hundreds; these appeared to the persons in the house to be shaken in such a manner as to create fears of their tumbling down, and made them run into the open air and fall prostrate. At Ahada, opposite Cove, the noise heard was such as resembled the firing of cannon; and many there, but for the early hour, were inclined to think it proceeded from the morning gun in the harbour.

In the town of Middleton the shock was not less severe than in Cork harbour. In one house a bed was shaken so violently as to cause the person who lay in it to jump up, under an impression that the floor was giving way; the water-jug was thrown on its side, and the cover shaken off a teapot which stood on a shelf in the bed-room. Many persons thought it must have proceeded from an explosion of gunpowder at Cove or Spike Island, and for a great part of the day were most anxious to hear from their friends there.

The more probable conjecture is, that some more violent shock has taken place elsewhere, and that details of some dreadful calamity may be daily expected. Previous to the account of the awful and destructive earthquake which laid Lisbon in ruins in the year 1755, like sensations were felt in Cove, and the entire coast at that side, and we await with no ordinary anxiety to learn if any parts of the Continent have suffered under a like visitation at present.—(*Cork Mercan. Chron. April* 7.)

SHAKING MINARETS.

The city of Ahmedabad, in Bombay, is justly celebrated for its beautiful buildings of stone and other materials, and was not less so for the famous Shaking Minarets, which were admired by every stranger. The awful earthquake which visited this city on the 18th of June last has, we regret to learn, levelled nearly the whole of these fine ornaments with the dust. The proud spires of the great mosque, the Juma Muzjid, erected by Sultan Ahmud, the king of Guzerat, and the founder of the city of Ahmedabad, which had stood nearly four hundred and fifty years, have tumbled to the ground within a few yards of the place where they once reared their heads. The mosque itself has sustained less injury than could have been expected, and the handsome arch which divided the minarets has escaped unhurt. Another Muzjid, of elegant structure, which lies to the left of the road leading to the Shahee Bagh, denominated the Beebees, or Unchunt Koonkee ke Muzjid, has shared the same fate. A gentleman, while riding out, saw the minarets come down; the tops were thrown to a distance, and immediately afterwards the stones came tumbling down one after another.—The only remainining shaking minarets which are at all worthy of notice, and are much inferior to the others, have, it is said, been sadly fractured; they are situated in the Goompteepoora, to the east of the city, outside the walls.

INDIAN WILD ASS.

General Sir D. Ochterlony lately dispatched, as a present from the Nawab of Bhawulpoor to Governor-general the Marquis of Hastings, a beautiful wild ass, of that species called by the natives *Gor Khur*. This elegant creature is described as being eleven or twelve hands high, of a beautiful light fawn or cream colour, with long ears, and large black eyes. In disposition it is untractable, and in this as well as in every other respect excepting the colour, resembles the zebra. It is said to be a complete model of strength, beauty, and agility.—*Asiatic Mirror*.

LIST OF PATENTS FOR NEW INVENTIONS.

To Francis Lambert, of Coventry-street, in the parish of St. James, Westminster, for an invention communicated to him by a foreigner residing abroad, being a new method of mounting and producing, and also of removing, preserving, and replacing the figure in weaving gold lace, silver lace, silk lace, worsted lace, cotton lace, thread lace, and other laces, whether made or composed of the aforesaid articles, any, or either of them, or a mixture thereof.—11th April 1820.—4 months allowed to inroll specification.

To Henry Constantine Jennings, of Carburton-street, Fitzroy-square, for a lock or fastening for general use.—11th April.—6 months.

To William Hall, and William Rostill, of Birmingham, for a certain improvement in the manufacture of hafts, handles or hilts for knives, forks, swords, or any other instruments to which hafts, handles or hilts are necessary and can be applied, whether made of turtle- or tortoise-shell or other suitable material, which invention or improvements they believe will be of general benefit and advantage.—11th April.—6 months.

To Thomas Burr, of Shrewsbury, for certain improvements in machinery for manufacturing lead and other metal into pipe and sheets.—11th April.—6 months.

To Edward Coleman, Professor of the Veterinary College, St. Pancras, Middlesex, for a new and improved form of construction of shoes for horses, which invention he believes will be of general benefit and advantage.—15th April.—2 months.

PROPOSAL FOR ESTABLISHING A MORE CORRECT ACCOUNT OF CIVIL TIME, NO LESS SIMPLE THAN THE GREGORIAN COMPUTATION.

Were every fourth year, excepting the 500th, reckoned to consist of 366 days, the average tropical year would be estimated at days 365, hours 5, min. 57, sec. $7\frac{1}{5}$; an approximation close indeed to the year's true value. It is therefore suggested that the papal year be amended by the addition of the differential 2000th part. W. W.

ASTRONOMY OF THE ORIENTALS.

[Abridged from the Calcutta Journal.]

The following are some of the astronomical measures of time relating to the sun and moon, according to the calculations of the Hindoo astronomers, and by which the Bramins, Moguls, and other Mohammedans in India chiefly go, in the reckoning of time*.

The lunar year they reckon 354 days, 22 gurris, 1 pull. The solar year they reckon 365 days, 15 gurris, 30 pulls, $22\frac{1}{2}$ peels, Indian time: 60 peels making 1 pull, 60 pulls 1 gurri, and 60 gurris 1 day. According to which the following table is constructed.

From the table it appears that the Indian year of 365 days 15 gurris 30 pulls and $22\frac{1}{2}$ peels is equal to 365 days 6 hours 12 minutes and 9 seconds of our time; and accords with our sidereal year nearly, which is stated at 365 days 6 hours 9 minutes and $14\frac{1}{2}$ seconds. The Indian lunar year, reckoned at 354 days 22 gurris 1 pull, measures 354 days 8 hours 48 minutes

* See Fraser's History of Nadir Shaw, passim.

24 seconds,

24 seconds, English time; which very nearly corresponds with that settled in our tables at 354 days 8 hours 48 minutes 36 seconds *.

TABLE.

Peels.	Pulls.	Gurris.	English Time.
2½	....	....	1 second
12½	....	....	5 seconds
25	....	....	10 seconds
37½	....	....	15 seconds
50	....	....	20 seconds
75	1¼	....	30 seconds
150	2½	....	1 minute
750	12½	....	5 minutes
1500	25	....	10 minutes
2250	37½	....	15 minutes
3600	50	....	20 minutes
4500	75	1¼	30 minutes
9000	150	2½	1 hour
18000	300	5	2 hours
27000	450	7½	3 hours
36000	600	10	4 hours
45000	750	12½	5 hours
54000	900	15	6 hours
81000	1350	22½	9 hours
108000	1800	30	12 hours
216000	3600	60	1 day

The lunar cycle, or period of 19 years, as also that called the Chaldean or ecliptic period, confessedly originated with the Eastern astronomers: and that we may see the agreement of the Oriental astronomers with our European calculators, we here insert the measure of 19 sidereal and lunar years after both accounts, thus:

Indian Time reduced.

	Days.	Hrs.	Min.	Sec.
19 × 365 days =	6935	0	0	0
19 × 6 hours =	4	18	0	0
19 × 12 min. =	0	3	48	0
19 × 9 sec. =	0	0	2	51
Indian time ..	6939	21	50	51
Ferguson's Tables, p. 190,	6939	20	55	35½
Difference	..	..	54	15¼

* See Ferguson's Astronomy.

Hence

Hence the difference between the Indian and European is 54 min. 15½ sec. in 19 sidereal years.

Indian Lunar Years reduced.

	Days.	Hrs.	Min.	Sec.
19 × 354 days =	6726	0	0	0
19 × 8 hours =	6	8	0	0
19 × 48 min. =	0	15	12	0
19 × 34 sec. =	0	0	7	36
19 × 12 lunations =	6732	23	19	36
6 lunations =	177	4	24	12
1 lunation =	29	12	44	2
235 lunations =	6939	16	27	50
Ditto by English tables =	6939	16	26	51
Difference	..	..	..	59

The difference between 235 lunations, composing the lunar cycle of 19 years, by both reckonings less than one minute!

METEOROLOGY.

George Town, Columbia (U. S.) Feb. 22, 1820.

"We are in hopes that our winter is over; in fact, we had last week some as hot weather as I ever experienced in England, but the following statement of the height of the thermometer will give you some idea of its actual state: 10th. Very cold wind, with sharp sleety snow. 11th and 12th, warmer. 13th, too hot at 4 o'clock P.M. to walk comfortably, even thinly clad. 14th, hotter still. 15th. Thermometer at 3 P.M. 76°, at 6, 66°, at 11, 62°. 16th. Thermometer at 3 P.M. 66°, at 7 P.M. 73°. This day we had no fire in our room, our windows wide open, coat off, &c. &c. The ladies all resumed their summer thin apparel. 17. Thermometer at 8 A.M. 62°, at 3 P.M. 66°. Vegetation has come on during these last three days with a rapidity I never before beheld. 18th. Weather much cooler. 19th. Thermometer 2 P.M. 54°. 20th. Cold wind. Thermometer at 3 P.M. 49°. 21st. Colder still, with a N.W. wind. Thermometer 44° at 3 P.M. We expect it colder still to-morrow, and then probably we shall have it as hot again as it was last week."

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1820.	Age of the Moon.	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Mar. 15	1	57·	30·05	Very fine
16	2	50·	30·15	Cloudy
17	3	51·	30·10	Ditto
18	4	47·5	30·23	Fine
19	5	43·5	30·14	Cloudy
20	6	47·	30·10	Very fine
21	7	51·5	29·86	Ditto
22	8	49·	29·60	Cloudy—rain at night
23	9	48·	29·95	Stormy
24	10	41·	28·85	Rain and hail
25	11	44·	29·20	Cloudy—snow P.M.
26	12	39·	29·60	Rain
27	13	55·5	29·53	Cloudy—heavy rain at night
28	14	54·	29·74	Ditto
29	full	59·5	29·85	Fine
30	16	57·	29·77	Ditto
31	17	57·	29·77	Ditto
April 1	18	53·5	29·65	Cloudy
2	19	58·	29·87	Ditto—brisk wind
3	20	62·5	30·04	Fine
4	21	58·5	29·80	Ditto
5	22	62·5	29·53	Ditto
6	23	53·5	29·16	Cloudy—rain in the morning
7	24	44·	29·24	Fine—snow A.M.
8	25	50·	29·20	Cloudy
9	26	47·5	29·20	Ditto—rain A.M.
10	27	51·5	29·29	Ditto
11	28	55·	29·29	Ditto—rain and hail with thunder and lightning P.M.
12	new	55·	29·60	Ditto—heavy rain in the evening
13	1	52·	29·70	Ditto
14	2	48·5	29·40	Ditto—heavy rain all the morning, and rain again in the afternoon.

Thermo-

Thermometrical Report from 1*st January to* 31*st December* 1809, *Fort Frederick, Trincomalie—Ceylon.*

Months.	Medium Temperature Morning.	Medium Temperature Midday.	Medium Temperature Night.	Highest Temperature.	Lowest Temperature.	Great Variation in 24 hours.	Average Monthly Temperature.
1809.							
January	75½	77½	76	81	73	4	76½
February ..	75⅔	79½	77	82	74	5	78
March	77	83	79⅔	86	75	8	80
April	79½	85⅔	82	90	78	9	82
May	79⅔	88	82	90	77	10	83⅓
June	79½	89⅓	82⅔	90½	78	10	84
July	79	88	81	90	75	10	82⅔
August	78½	87	81	90	76	11	82
September ..	78	87⅓	80	90	76	11	83
October	76½	83	78⅓	90	74	12	79⅓
November ..	75⅔	80½	78	84	73	9	78
December ..	75⅔	79⅓	77	81	73	7	77¼

Thermometrical Report from 1*st January to* 30*th November* 1810, *Fort Frederick, Trincomalie—Ceylon.*

Months.	Medium Temperature Morning.	Medium Temperature Midday.	Medium Temperature Night.	Highest Temperature.	Lowest Temperature.	Great Variation in 24 hours.	Mean Monthly Temperature.
1810.							
January ...	75½	79⅔	76⅓	81	72	6	77
February ...	74	87	77	82	72	7	79⅔
March	78	83½	79	86	75	6	80½
April	80⅔	86⅓	83	89	79	8	83⅓
May	82	86⅓	82½	92	75	10	83
June.... ..	79	88	81⅔	93	76	12	83
July	79	84⅓	82	91	78	10	82
August	78	85	79½	90	76	10	81
September..	79	87½	82	90	77	11	83
October....	77½	83½	79⅓	86	75	8	79⅓
November..	77⅔	82	79	85	74	6	79⅓

Thermo-

Thermometrical Report from 1st *January to* 31st *December* 1817, *at Kandy, Island of Ceylon.*

Months.	Medium Temperature Morning.	Medium Temperature Midday.	Medium Temperature Night.	Highest Temperature.	Lowest Temperature.	Greatest Variation in 24 hours.	Mean Monthly Temperature.
1817.							
January	70	76⅔	75½	80°	67	9°	72⅔
February ...	70½	76⅓	73⅔	80	68	9	73⅔
March	71	77⅔	74	80	68	11	74
April	73	80	76½	83½	69	10	76⅔
May	73⅓	79	76	84	70	11	76⅓
June	71	76	75	79	67	11	74½
July	72	75	73	78	70	7	73⅓
August	70½	75	70	80	67	9	72
September ..	70½	75⅔	72	81	67	10	72
October	70	75	72⅓	78	68	9	72⅔
November ..	70	75⅓	72⅔	78	67	11	72⅔
December ..	71½	77	73⅓	80	68	10	73½

Thermometrical Report from 1st *January to* 31st *December* 1818, *at Kandy, Island of Ceylon.*

Months.	Medium Temperature Morning.	Medium Temperature Midday.	Medium Temperature Night.	Highest Temperature.	Lowest Temperature.	Greatest Variation in 24 hours.	Mean Monthly Temperature.	RAIN. Inches and tenths.
1818.								
January	67⅔	75⅚	71⅓	80	60	15	72	2·5
February ...	70	79	73⅔	81	62	15	74	1·0
March	67	79	70⅔	82	61	19	72⅔	4·4
April	67	81½	70⅔	84	64	20	71	5·2
May	69	82⅓	72	84	63	21	74⅓	0·5
June	71⅓	78⅔	72½	83	70	11	74	6·2
July	70⅓	78⅕	72	82	66	12	73½	9·7
August	70	77	70⅓	81	68	12	72	6·1
September ..	70⅓	75⅓	72⅔	80	69	10	73	7·7
October	70	75½	73	78	68	9	72	15·4
November ..	68	75⅔	71⅓	80	63	11	71⅔	9·8
December ..	67¼	76	70⅓	80	61	17	71⅔	6·0

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For April 1820.

Days of Month. 1820.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
March 27	50	54	50	29·89	0	Cloudy
28	47	57	51	30·08	0	Fair
29	52	58	50	·20	0	Fair
30	51	58	45	·05	0	Cloudy
31	46	58	47	·11	0	Fair
April 1	45	57	50	·07	0	Fair
2	52	63	49	·15	0	Fair
3	52	60	46	·25	0	Cloudy
4	47	62	46	29·97	0	Fair
5	47	66	50	·80	0	Fair
6	49	54	40	·47	0	Showery
7	40	48	41	·57	0	Hail showers
8	41	52	45	·35	0	Sho wery
9	46	51	40	·45	0	Showery
10	38	47	47	·50	0	Rain
11	48	57	49	·60	0	Showery
12	47	55	49	·85	0	Cloudy
13	47	50	40	30·11	0	Rain
14	47	52	48	29·77	0	Cloudy
15	47	57	48	·91	0	Fair
16	46	58	53	30·19	0	Fair
17	52	64	57	·31	0	Fair
18	57	67	55	·25	0	Fair
19	55	67	56	·25	0	Fair
20	54	63	52	·32	0	Fair
21	53	62	51	·41	0	Fair
22	54	63	46	·45	0	Fair
23	47	62	45	·57	0	Fair
24	45	62	44	·59	0	Fair
25	46	61	45	·50	0	Fair
26	50	67	47	29·89	0	Cloudy

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

L. *On the alleged Plagiarisms of Frenchmen: on the Originality of the musical Instrument called the* Sirene; *and on the Pitch and Scale, of the Notes of a French Standard* Harmonica, *which are said to have been ascertained thereby. By Mr.* JOHN FAREY *Sen.*

To Mr. Tilloch.

SIR, — IT has been to me a subject of poignant regret to learn, from several intelligent Englishmen who have visited Paris or travelled in France in search of *useful* information, that an exceedingly small proportion only, of any of the ranks or classes in society in France, understand our Language, sufficiently to converse therein; and that in consequence, those of our countrymen who cannot speak French, and are unaccompanied by a friend who does so, fluently, do little more than *lose their time and spend their money*, when they cross the English Channel, with the view of acquiring the information to which I have alluded.

It is to this general distaste, which, if I mistake not, Frenchmen in general have, to the study *of any modern language but their own*, except perhaps the Italian and German, amongst the superior and scientific classes, and not more so to our language, than those of others of the nations who surround them, that I wish, in candour, to attribute, the many instances of apparent plagiarism and of literary injustice, as to the discoveries and writings of our countrymen, in the sciences and useful arts, which are complained of in our journals, against Frenchmen *: and this remark I wish in particular to apply to the Baron Cagniard de la Tour's account, translated into pages 293 and 294 of your last Number, of the Instrument which he denominates the *Sirene*; being willing to suppose, that the Baron had never read, or perhaps heard of, Dr. Robison's account in the Supplement to the third edition

* A memoir of the late James Watt, esq. was, as it since has appeared, written by Mr. *William Playfair*, for the *New Monthly* Magazine, and has thence been transferred into your last volume; at page 445 Mr. P. has therein, without regard to truth or propriety of language, charged M. de Prony with gross and wilful injustice to Mr. Watt, as to his improvements of the Steam-Engine, as my Son has (although by mistake you omitted to place *Junior* after his Name) in p. 110 of your present volume, shown, by quoting the passages from M. de Prony's Work, wherein *due acknowledgements are made* of Mr. Watt's claims: notwithstanding which, and without noticing these extracts, Mr. P. has, in another Work, viz. the original *Monthly Magazine*, for the present month, p. 322, in language not less unjustifiable, repeated *his attacks* on M. de Prony, grounding the same now, principally on a conversation which he alleges to have held with Mr. W. in 1810 or 1811:—in answer to which I beg, in the absence of my Son to state,

edition of the *Encyclopædia Britannica*, vol. ii. p. 650, of *his discovery*, of the mode *of producing perfect musical Sounds, by the mere opening and shutting of a stop-cock, through which, slightly compressed air, thus interruptedly passed, at very quick intervals*, to which discovery the Baron appears now to lay claim, in his account referred to.

Those of your Readers who are conversant with *Harmonics*, can scarcely fail to have regretted with me *, the want of precise measures of the intervals of the Scale, which are *actually produced*, by the most expert and approved of the *professional Tuners* of our common keyed Instruments. Such an instrument as the *Sirene*, when properly constructed, and used with the requisite care and precautions, seems capable of affording these measures; it is however, abundantly apparent to me, that some or all of these requisites have been wanting, in the experiments which the Baron has recorded, as to the Pitch and Scale of the Standard French Instrument which he calls an *Harmonica*.

In the first place I would observe, as to *the pitch*, that if the Key Note *ut*, or c of this Instrument make 511 vibrations in 1″, and the English concert pitch be 480 vibrations for the same note, as I believe to be usually the case, this *Harmonica* was pitched 55Σ, or nearly a major Semitone, higher than our English Instruments are usually pitched or tuned †.

The numbers given in the 2d and 3d columns of the Baron's Table, are not in the ratio 1 to 13·5, as the previous description might have led one to suppose, but in the ratio 1 to 22·5 or very nearly so; and the whole description is so concise, and withall so confused, as to furnish no clue, for determining the accuracy of the

state, that about two or three years ago, while he was engaged in preparing the elaborate article *Steam-Engine*, which is printed in Dr. Rees's *Cyclopædia*, Mr. Watt did us the honour to call here several times, and in my presence, held various conversations with my Son, relating very fully to Mr. W's inventions and improvements of the Steam-Engine, and relating to the rival claims, which various persons have at different times set up; into the particulars of which rival claims, my Son had previously, spared no pains to inquire, through every practicable channel, and having stated the conclusions to which he had come, in each of such cases as regarded Mr. Watt's Steam-Engine, by reading these parts of his Manuscript to Mr. W. he had the satisfaction of receiving, his full assent thereto: and I beg to declare, that in these conversations, not the slightest complaint was made, of M. de Prony's conduct; nor on a later occasion, when Mr. W., only a very few months before his death, called here, and left for my Son (who was from home at the time) a copy of his own Work on the Steam-Engine, not then published; on which occasion Mr. W. sat and conversed with me for a considerable time, on the subjects treated of in his newly printed Book.

* See vol. xlix. p. 447.

† The great improbability of the Continental *Pitch* being at this time *higher*

the numbers in the 3d column: it will be perceived however, that the four last notes in the Table, viz. la, si, ut and re, being octaves of the first four notes therein, viz. A, B, c, and d, the numbers in the latter case, ought to have been, the exact halves of those in the former; whereas none of them are so; the difference being, in the latter case, or for d, 4·5 vibrations: which, as well as the 675 vibrations assigned to g, differ so widely from the system of M. Sauveur, to which the Baron says that his numbers approximate, as to divest the whole of any character for accuracy. I have not seen the original account; but the translation in a contemporary journal, introduces near the middle of the Table, the note mi or e, making 630 vibrations per 1″, which is omitted in p. 294.

I should be glad to hear, that one of these new Instruments had been brought to this country, or rather that one had been made here, with every attention to accuracy of construction; and that by means thereof, and without trusting to *the fallacious use of Unisons* * in comparing the Notes, the numbers of Vibrations in the Scales, of several of our most approved Tuners, may be several times ascertained, and carefully noted for publication, in yours or some other of our scientific journals. I am

Your obedient servant,

Howland-street, May 3, 1820. JOHN FAREY Sen.

LI. *Strata of the Environs of St. Petersburgh, in the Order of Geological Position* †.

ALLUVIAL EARTHS.

SUPERFICIAL deposits, either in beds, or irregularly, on the surface of all the other formations.

POST-DILUVIAN FORMATIONS,

the disposition of which did not commence, till after the final retiring of the waters.

higher than that of Instruments in England, will appear, from what Dr. Robert Smith intimates, pp. 208 and 218 of the 2d Edition of his "Harmonics," as to the elevation of a whole Tone, which took place in the English Pitch, sometime previous to the middle of the last century: and from what is related in corroboration thereof, by an English Singer, in p. 152, vol. i. of the "Quarterly Musical Review," who lately took with him a standard English Tuning Fork, in his visit to the chief musical cities in Europe; in all of which, he declares the pitch to have been found by comparison, *a Semitone at the least lower than this Fork.*

* See the English Musical Gazette for April 1819, vol. i. pp. 68 and 69.

† From a Map Sheet, recently published by the Mineralogical Society of St. Petersburgh.

Class I.—ALLUVIUM (properly so called). The more modern local deposits, which still continue daily increasing.

(*Contemporaneous Deposits.*)

Characters.	*Minerals.*	*Organic Bodies.*	*Localities.*
Shifting sand. Light sand thrown by the sea upon the coasts; sometimes carried by the winds into the interior country.			Coasts of Peterhof and Oranienburg.
Marshy grounds. Earths, deposited by the waters, forming new lands, at the mouths of the great rivers.	Earthy phosphate of iron.	Wood, not petrified.	Delta of the Neva.
Turf. Spongy and inflammable deposits, proceeding from the decomdosition of moss, heath, &c.	Earthy ore of iron.	Wood, especially birch, not petrified, but impregnated with ferruginous particles.	Passim.
Tufa. Sediment, deposited by water which flows from a calcareous country; limestone; light yellow, soft, porous.		Mosses, branches and leaves of trees, &c. fresh-water shells; helicites; buccinites.	Gledina, Poudost, Ropsha.

DILUVIAN FORMATIONS,

the deposition of which was begun and finished, during the last sojourning of the waters.

Class II.—DILUVIUM (properly so called).

Deposits of an epoch more remote; universal; composed of the remains of every formation; and containing the bones of quadrupeds, the race of some of which is extinct.

	Characters.	Minerals.	Organic Bodies.	Localities.
(Contemporaneous Deposits.)	*Gravel*, or diluvian remains. a. Granitic sand, containing a number of loose stones. b. White or gray marl in very thin beds. c. Masses of separate stones, cemented together by an oxide of iron, serve as the basis to the beds of marl *b*.		d. Teeth of wild boars. e. Skulls of rhinoceros?	a. Martishkina. b. Banks of the Coïrovca and Crasninca. c. Banks of the Coïrovca. d. Pezalova. e. Canal of Moika.
	Detached blocks (boldres of the English geologists), and transported from a distance.			
	Primitive rocks. Reddish kernelly granite, found in the rocks at Wibourg. Grey granite not kernelly, and other varieties of primitive rocks, many of which are in a state of rapid disintegration.	(*a*) Felspar of Labrador. (*b*) Granites.		(*a*) Peterhof: A similar rock exists in Norway. (*b*) Strelna; the parent rock is probably near Serdobol.

Characters.	*Minerals.*	*Organic Bodies.*	*Localities.*
Secondary rocks.			
(*c*) Round masses of calcareous stone, Class III.	(*e*) Ferruginous freestone.	All these of Class III.	(*c*) Never far from the parent rock.
(*d*) Varieties of sandy beds, Class IV.		(*f*) The chamites of Class IV.	(*d*) Mountain of Penty.
			(*e*) Environs of Peterhof.
			(*f*) Mountain of Shoulcova.

ANTEDILUVIAN FORMATIONS.

Formations completed before the last sojourning of the waters.

Secondary Earths.

Rocks or solid formations in the heart of the earth, stratified, or disposed in beds more or less horizontal; they contain the remains of organic bodies, and fragments of the oldest rocks.

Salt-water shelly stone; in beds not thick, but conformable to that of the schist which they cover; it breaks in cubic fragments.

Class III.—(*Successive Deposits*) Limestone.

Characters.	*Minerals.*	*Organic Bodies.*	*Localities.*
a. Hard, sandy, yellow.	(1) Calcareous spar well crystallized.	1. Helix.	*a.* Doudorof.
b. Hard, crystalline, lilac.	(2) Green carbonated copper.	2. Remains of pentacrinites.	*b.* Doudorof.
c. Soft, argilous, whitish.	(3) Sulphurized copper.	3 Mass of fungites, &c. as at Reval.	*c.* Crasnoe Selo.
d. Hard, deep red.	(4) Pyrites.	4. Orthoceratites (white).	*d.* Poutyelova.
e. Spotted, yellow, red and green.			*e.* Banks of Poulcovca.
			f. Do.

f. Argilous stone, greenish, approaching to the nature of the next mentioned.	(5) Streaked, with grains of green earth.	5. Trilobites. 6. Encrinites paradoxus. 7. Terebratulites. 8. Hystérolites. 9. Tetes d'Alcyonium? in form of spathique citron.	(1) Cosheleva. (2) Popovca (3) Do. (4) Do. (5) Banks of Poulcovca. 1. Crasnoe Selo. 2. Pavlovsky. 3. Tzarscoe Selo. 4. 5. Poutyelova. 6. 7. Tzarscoe Selo. 8. Pézalova. 9. Crasnoe Selo.

Class IV.—SCHIST.

Argilous, or sandy, ordinarily in very thin beds, but subject to great irregularities of declination, as well as of composition.

Characters.	*Minerals.*	*Organic Bodies.*	*Localities.*
a. Greenish schistous argil approximating in its nature to the preceding. *b.* Alternating with sandy beds. *c.* Gray yellow mamellated. *d.* Yellow sand, spotted with green.	(1) Green earth of great purity. (2) Yellow pyrites in cubes. (3) Pyrites, colour of bronze, and in octahedrons. (4) Sulphurized lead. (5) Native and earthy sulphur. (6) Nodules, streaked with bituminous lime.	1. Chamites. 2. Ammonites? (Georgi.)	*a.* Banks of the Poulcovca. *b.* Pezalova. *c.* Pezalova. *d.* Peterhof. (1) Caporié. (2) Banks of Poulcovca. (3) Pavlovsky. (4) Popovca. (5) Do. (6) Crasnoe Selo, Pavlovsky, and banks of Poulcovca.

Characters.	*Minerals.*	*Organic Bodies.*	*Localities.*
e. Gray quartz with large grains.			*e.* Oranienbaum.
f. Ferruginous sand, black or red.			*f.* Toxova, &c.?
g. Black schist, brown or green, argilous, inclining to the nature of the next mentioned.			*g.* Crasnoe Selo.
			I. At Pezalova in beds, and at Shoulcova in detached blocks.

Class V.—Argil.

The stratification of which is not well marked; it has been pierced to the depth of 70 sagenes, without the rock being reached which serves as its base.

Characters.	*Minerals.*	*Organic Bodies.*	*Localities.*
a. Greenish blue argil, inclining to the nature of the preceding.	(1) Pyrites in small quantities.	This bed appears to be, hitherto, altogether destitute of organic bodies.	*a.* General character *e. g.* at Gorelova.
b. Spotted red and yellow.	(2) With a little mica.		*b.* Coïrova.
c. Intersected by veins.	(3) Veins of yellow argil.		*c.* Coïrovca.
d. Nearly oolitic.	(4) Grains of the appearance of iron.		*d.* Pavlovsky.
			(1) Pavlovsky.
			(2) Coïrova, and along the Couzminca.
			(3) Along the Crasninca near Crasna-Cabac.
			(4) Pavlovsky.

This system of formations appears to extend, at least from Esthonia to the southern extremity of Lake Onega.

Min. Soc. Petersburgh, Feb. 13, 1819.

LII. *On*

LII. *On the Management of Bees.* *By* Isaac Espinasse, *Esq.**

Chancery-lane, Feb. 7, 1818.

The cultivation of, and improvement in, the management of bees, being one of those subjects of rural œconomy which the Society have thought worthy of their attention, and for the encouragement of which they have offered premiums, and they having at the same time signified that the claimant was expected to suggest his observations on the subject, I am induced (though engaged in very different pursuits) to offer the following Essay to their consideration. It is the result of experience, and of many years actual observation, not the speculation of a theorist, nor a compilation from the works of other authors on the same subject.

It is presented to the Society with no view to the pecuniary premium which they have offered, for did what I here submit to them give me any claim, or entitle me to such mark of their approbation, it should be restored to them, as sought for with no other view than to entitle me to have the honour of ranking among its members.

Conceiving that the views of the Society are not merely speculative, but directed to objects of practical improvement, I cannot but consider the number of publications respecting the generation of bees, the prolific powers of the queen bee, the formation of their cells, and administration among themselves, as theories ingenious and entertaining, but as in no wise tending to inform those who wish to keep this valuable insect, to make it most productive, or render it more generally useful. Several of these publications I have read, and the accounts in them of the different processes by glass hives, boxes, &c. the object of which is to save the bees and take the honey, are open to these objections:—the descriptions in them are too complicated to be easily understood, and the necessary apparatus too expensive to be purchased, but by a few; and it must be recollected, that it is not from such, the supply of honey and wax necessary for our consumption is procured; as the number of those who indulge in these entertaining pursuits is small; it is the husbandman, the farmer, the gardener, and the cottager, who by small contributions furnish the necessary stock of wax and honey.

In offering, however, my observations to the Society, I feel considerable difficulty, as my opinions on the subject of their premiums are much at variance with theirs: they seem to consider, that he performs a meritorious service to the public, who points out

* From the *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* for 1818. The Silver Ceres Medal of the Society was voted to Mr. E. for this communication.

out the mode of increasing the number of hives, and of preserving the insect. If the object of the Society is, to increase the quantity of wax and honey necessary for consumption, nothing is more fallacious than to suppose that it is *necessarily* effected by offering a premium for the greatest number of hives, without reference to their weight; though the assertion may seem a paradox, that an increase of hives does not necessarily produce an increase of honey. It is easy to cover the apiary with hives: if one is allotted to every swarm, cast, or colt, the numbers may easily be multiplied; but the quantity of wax and honey is lessened, as I shall demonstrate to the Society in the course of these observations.

I am no stranger to that complaint so continually in the mouths of those who are unacquainted with the subject, the cruelty of taking the honey by destroying the bees. Their fate is pathetically lamented, as if they alone of all the creation were to be exempt from their contribution to the support of man: while such persons walk through a market, and look into a slaughter-house without emotion, they weep over the apiary in the month of September. No one can better love the insect than I do, nor more exert himself to shelter, to save, and protect it: addressing myself therefore with due deference to the opinions of the Society, who have thought the preserving the lives of the insects an object meriting a premium at their hands, I avow it to be my opinion that I consider the speculation of saving the bees, and at the same time taking the honey, as impracticable, as well as unnecessary. It is unnecessary, for no insect generates faster than the bee; as I have ascertained from actual experience, that in three weeks after a swarm had been hived, not only were the combs formed, but full of the maggot, the infant state of the bee, and the combs will be found so filled with progressive generation from the month of March until October.

That it is impracticable, this observation will show: there is a certain relation in nature between the animal to be fed and the pabulum necessary for its support. The earth throws out but a certain quantity of flowers, and all that it does, do not afford honey for bees.—Their numbers therefore must be limited by the food necessary for their support; and to suffer them to increase *ad infinitum* would be to defeat its own object, for famine would soon destroy those whom a mistaken mercy had spared *. The profusion and variety of flowers necessary for them is found

* I am led to this observation, from what passed within my own knowledge a few years since. In the village where my house is situated, many persons induced by my example procured bees; they were too numerous for what was to feed them; more than one half of them died in the ensuing winter; and nearly one-third of mine were saved only by being fed.

in

in few parts of the kingdom where I have been. And that pastures are to be sought for, other than those which surround the apiary in the beginning of the season, and before their numbers have been increased by swarms, is demonstrably proved by what is mentioned in some authors on the subject; that in Piedmont and Savoy, the bee-masters bring their hives down the rivers on rafts, anchoring in different places where flowers are found in abundance for their food. Is this necessary from any other cause than from the want of sufficient food at home?

In a treatise of this description, intended for the perusal and information only of persons of common minds, who have no capacity for experiment, nor means of trying its effects; who are satisfied with the animal as they find it, as well worthy of their care and attention for the value of its productions, as conducive to health and many of the most useful purposes, I put by all the systems which speculation has raised, and the theories in which ingenuity has indulged itself. Used to bees from my childhood, and for the last eighteen years the proprietor of a large stock, which I have watched, observed, and studied, and having directed my observations only to increase them as far as they could be kept, and having protected them from every thing that could injure them, I address the observations contained here, to enable those who have none, and who may be disposed to keep them, to have the benefit of my experience, and those who have to increase their produce. This object I propose through the medium of this honourable Society.

I am aware that many of my observations will be found to be not entirely new, and equally so, that I shall have to controvert many received opinions; but I will state nothing which I have not tried, and for the effect and success of which I will not answer.

It requires no argument to establish the position, that where there is the greatest quantity of flowers, that is the situation best adapted for keeping bees, but there are few where nature has not thrown out abundance. My house is situated in the village of Bexley in Kent: it is in the vale of Cray: Dartford heath is within a mile of it to the east, and Bexley heath two miles and a half to the north-west. I mention these matters with respect to situation, as heaths are supposed to be peculiarly productive of pabulum for the bees, as affording in abundance the blossoms of the heath, furze, and wild thyme. The situation in that respect is a matter of primary consideration. Those who are unacquainted with the insect suppose that he possesses the fee simple of every flower, and that the whole regions of that description are devoted to his use. It is very far otherwise; and when poetry makes it light upon a rose and inhale its sweets, the truth is, that it never sucks it at all, or many other flowers;

in fact, all whose cups are so deep that its sucker cannot reach the bottom of it, where the honey is deposited. He, therefore, who wishes to keep bees must not rely upon every flower as furnishing support to his apiary; he must rely only on those which are shallow and small, or which expand widely in the leaves, so that the bee can get to the bottom of the flower; neither must he rely wholly on the produce of the fields, meadows, pastures or heath, but make his garden, or its borders, a separate plantation for such herbs and flowers as are fit for their use. For this purpose, nothing I have found so grateful to the bee, so easily propagated, so fruitful in produce of flowers, and so delightful in its flavour, as lemon thyme. It ornaments every part of my garden; and as it flowers in the beginning of August, and the honey derived from it is last deposited in the cells, it gives a flavour to all the honey which before had partially filled them:

——— redolentque thymo fragrantia mella.
VIRG. *Georgic* iv.*

The common thyme, winter savoury, and mignonette, I have also found greedily sought after; and as they blow late, they are therefore valuable.

In the garden, as affording food for bees, I must much recommend large plantations of gooseberries, currant trees, and raspberries.

Few of the ornamental flowers of the garden, as the ranunculus, anemone, pink, or carnation, afford any pabulum for the bees; and as for the tulip, I am induced to think that it possesses some quality injurious to them, from the following circumstance: In the month of May 1817, having gone down to my garden when the tulips were in blow, and looking into the body of the flowers for those which were well coloured, I observed a dead bee in almost every one of them, and in some two. This led me to examine nearly the whole of my beds, and there was scarcely any flower which did not exhibit the same appearance †.

The blossoms of the first-mentioned shrubs afford to them the earliest flowers, particularly the gooseberry; and the cupidity with which they suck them presents the image of a sailor on his return tasting fresh meat and vegetables, who during a long voy-

* I cannot help remarking here, that almost every observation in that delightful book, the Georgics of Virgil, I have found verified from my own experience.

† From what cause this proceeded I have not been able to ascertain, unless from some deleterious substance which the insect might have imbibed from the flower: apprehending, however, that it might have been caused by the inability of the bee to escape from the depth of the flower, I took a few bees from a hive, and put them into several very deep flowers; but they all got out, and flew away without any difficulty.

age had fared on nothing but salt provisions. The blossom of the turnip is also very useful at that season of the year, as it furnishes both wax and honey in abundance.

I do not pretend to enumerate all the flowers which afford food to bees: the general descriptions I have already given: those from which mine have principally drawn their support, are the white clover and the blossoms of the lime tree; and for wax, the blossoms of the furze and broom: it is the farina collected from these, that repairs the cells that have been tapped for their food during the winter: proximity therefore to these sources of support is a very desirable circumstance in the formation of an apiary, the benefit of which I fully experienced in the course of the summer of 1816. The lime trees which usually blossomed about midsummer, did not fall into bloom in that year until late in July: at midsummer the old hives are preparing, or have just cast swarms, and the combs in the new are not in a fit state to receive the honey; but in the late season of 1816, when those trees went into blossom, in every hive the combs were formed, and were in a state to receive the honey from the flower. The beautiful and rich grove of Mount Mascal, near Bexley, composed entirely of lime trees, furnished pabulum for millions, and all the bees which then left their hives, I observed in thousands to take that southerly direction which led to it: the consequence was, that all my hives, which until that time were uncommonly light, from the effect of an unfavourable season, in a fortnight's time were amply filled with honey.

The next object, in order to increase and improve the apiary, is, attention to the hives used, and the mode of hiving the swarms. There is no prejudice which I have had more to combat with, than the using of large hives, that is, the use of hives without any attention to the proportion they bear to the swarms to be put into them. In Kent they are partial to hives nearly two feet high, and as large in diameter: they seem to consider that the hives cannot be too large, and that it is of no importance whether the loaded and wearied insect is to drag his burthen up two feet of the side of the hive, or one; or whether the hive is to be wholly, or only partially, filled with comb. Both are of the utmost importance. I have uniformly observed, that bees never work well, unless the hive is full *; and when, after all their labour, they find it empty, it is cold and uncheering, and they work with

* In hiving the swarms, therefore, this must be attended to with particular care, that the hive be proportioned to the swarm: if it is too small, they will lie out in clusters on the board, and not work, having no place in which to deposit what they gather; if it is too large, and of course in great part unfilled, when the bees come home they find the hive empty or cold, and work no longer with industry and spirit.

no spirit. I therefore always endeavour to have a swarm fill the hive, when first hived, to within two or three rims at the bottom; and so necessary do I consider it to have the hive full of bees before they begin to work, that I add to a small swarm either another, or a cast, and put two or three casts together, till a proper proportion of bees is had: this is done with the greatest ease, by spreading a cloth on the ground, and striking the hive which contains the swarm to be added quickly and sharply on it, when the swarm will fall in a cluster on the cloth; and then quickly covering it with the hive which is to be reinforced with the addition, those on the cloth readily ascend, and fraternize without difficulty with the others: by these means a strong hive is formed, well filled with bees, and they work with spirit; the hum of the many round the door, seems to rouse and cheer the rest; whereas, if the hive is weak of bees, the want of vigour and spirit is visible. It is this circumstance which has induced me to offer that observation which I before made, respecting the number of hives of bees, as making that a circumstance of reward: if every cast is hived, you have a numerous apiary; but I never had the good fortune to get an ounce-weight of honey out of a cast in July, which is the usual month for such swarms: I have, therefore, added them together, and often three into one hive, and they have worked cheerfully.

But the size and make of the hives is not the only matter to be attended to; the warmth arising from the thickness of the straw composing them, and the sweetness of it, are also deserving of much consideration. In all the hives which I have had in Kent, the rolls are thin and hard, and little attention is paid to the colour or sweetness of the straw; the consequence of which is, that they are cold in winter: I have, therefore, procured the hives which I use from Chelmsford, and Hertford, where the rolls are thick and not so hard, and the shape well proportioned and handsome. The shape which I prefer is low, wide, and pointed at the top. Bees begin to work near the top of the hive, and work downwards; and as the combs approach the bottom, their labour is diminished. The board on which the hive is placed should be of well seasoned elm, of the thickness at least of an inch, as it will otherwise warp with the heat of the sun; and when every part of the bottom of the hive does not touch the board, vermin, such as earwigs, snails, &c. get in; and from the moment such unwelcome guests intrude into a hive, the bees become discontented, and no longer apply themselves to work with vigour.

The bee-house is the next object of care. I consider mine as the best ornament of my garden: on an adjoining seat, most part of my day is spent in contemplating the activity and busy industry

dustry of these valuable insects; and in fact on the master of the bee-house being himself fond of the pursuit, great part of the success of keeping them depends: they must be watched during their time of swarming, and the attacks of their enemies must be warded off, for of these they have not a few. There are but two aspects in which the bee-house should be placed; the south, or some point towards it; and the east. I have in my principal apiary two houses, each containing eight hives; the one fronting the east, and the other the south; I have endeavoured to ascertain in which of them the bees throve best, but in that respect I could find no difference. I however prefer the southern aspect, as it enjoys the benefit of the winter sun, whose warmth and effect in keeping the house dry, is of infinite consequence: this leads me to the most decisive dictum on the subject, that it is essential to the thriving of bees, that they should be kept sheltered from damps and wet, in summer and in winter; as, unless they are kept dry and warm, they will not work. Turn up an unsheltered hive on a rainy day, in the midst of summer, a drowsy hum announces their torpor; but if sheltered, so that the chilly effect of the rain has not reached the interior of it, the moment the atmosphere is cleared, and a glimpse of sunshine is seen, they will be observed to issue from their hives to work, as alert as if not a single drop of rain had fallen.

The absolute necessity of warmth during the winter to the preservation of bees is, independent of what has been already observed, the best answer that can be given to the possibility of taking the honey without destruction of the bees. If the honey is taken, the combs must be taken too. What is the effect? The hive becomes empty; there is no exclusion of cold, from the circumstance of its being filled with combs: there is no resting-place for the bees, who are sheltered in the intervals, and some food is derived from the bee-bread and wax which is taken away. They do not live in winter clustered as in a swarm, but are dispersed over the combs, and either feed on their appropriate cell, or what the combs contain. How, therefore, a swarm deprived of combs is to live in that cold, inactive, and unprotected state, I cannot conceive. Without food they cannot exist; the quantum to be administered is not easily ascertained, and it is attended with much trouble; and though we well know that ingenious glass appendages have been filled with combs, such as the shop of Mr. Wildman exhibits, how is the cottager to procure this costly apparatus, and how to attend to it? for, except by such means, ingenuity has never suggested any mode, nor speculation glanced at its practicability. If you mean to carry your hives through the winter, you must give them their natural comfort and support; you must leave them their combs and their hives, and liberty to follow that mode of living which nature has pointed out to them.

Every bee-house should therefore be constructed of sufficient depth, that the hives may be brought forward, or pushed back, as the weather serves. In summer they cannot be brought too far into the light and heat, which rouses them into life, and stimulates their industry; nor in winter be too far removed from the external wet and cold.

My two bee-houses are sheltered on every side by hedges, except towards the south and east. The hedges to the north and north-east are beech, which are kept closely clipped. No trees should be planted in the front, as that keeps off the sun, whose light and warmth are the great stimulants to their working. Six hives are, in my opinion, enough for each house, and the houses should stand at some distance from each other: the reason is, that when the bees swarm, the buzz of those which rise first is apt to induce the other hives to swarm also, and two good swarms often unite; but when they do so, they should be put into a hive proportionably large. The bee-house opens by folding doors at the back, by which every hive can be easily got at for any purpose required; the places in the front being made too narrow for a hive to be taken out there: the back may be secured from thieves by locking, or nailing up the doors during the winter: the floors should be made accurately level, for the purpose of feeding the bees, if necessary.

To the construction of the bee-house it is indispensable that free access should be at all times had to the hives, to take them out when necessary. For this purpose, the doors at the back of the bee-house should open, whether it be to take them in autumn, to weigh them at that time, to ascertain their ability to support themselves during the winter, to plaster them to the board to exclude the cold or wet, or to destroy any vermin that may be lodged in the house, or under their boards; all which matters must be attended to. Previous to shutting up the bee-houses for the winter, the hives should all be taken out, and the whole interior carefully swept; when infallibly there will be found a plentiful collection of those insects whom the approach of winter drives into their hiding-places, and who seek for shelter in every cranny of the bee-house: snails, earwigs, and spiders are of this description; and that mice will also seek for shelter in the same place, and in the hive itself, I had this demonstrable proof.

On leaving the country in October 1809, among others, I left my oldest hive of considerable strength. In the beginning of the spring, I observed but few bees come out to work, and a general torpor seemed to pervade it: on turning it up, having before just lifted it on one or two occasions, I observed one half of the hive

hive filled with beech-leaves (a beech-hedge forms one side of my apiary): upon examining it I found that the back of the hive had projected beyond the board, through which a mouse had entered: the bees had retired to one side of the hive, and he had taken up his winter quarters in the other. He had made his nest with the decayed beech-leaves, and he and the bees had lived joint-tenants of the hive during the whole season. I removed the broken combs, and all the remains of his dwelling; the bees seemed to recover new life; the hive was restored; and though it did not swarm the succeeding year, it became one of my best, and continued so for many years after.

Birds will be found enumerated in every publication on the subject of bees, as their enemies; but I have found that there are but few birds destructive to the apiary; most of those which are so represented I have found are those who come to feed on the dead bees or maggots, which are thrown out from the hives: and though the swallow,

> "—————— Procne, manibus signata cruentis,"—Virgil,

is reckoned among the chief, I have observed that the most destructive is the bird called the house lark, a small ash-coloured bird of the size of a tit-lark and next to it the tom-tit; these should, therefore, be destroyed whenever they present themselves where bees are kept.

Of the insect tribe, there are many inimical to bees; of these the moth is the most deadly: these insects are among the smallest of the genus, and are of a light whitish brown; they are remarkably active in their motions, and are seen running with great quickness round the hive, watching their opportunity to enter; which when permitted to do in any number, the combs become the depository of their eggs; the top is filled with silky film; the bees are expelled; and the combs, when torn, resemble paper: these should be carefully watched and destroyed whenever they appear; for when oncethey enter a hive and breed, the loss of the hive is inevitable*. Spiders also, though but in a small degree destructive, interfere much with, and obstruct the working of the bees without, by drawing their webs across the recesses of the apiary: this is however in summer and autumn only. They are very easily destroyed, by going out at night with a candle, at

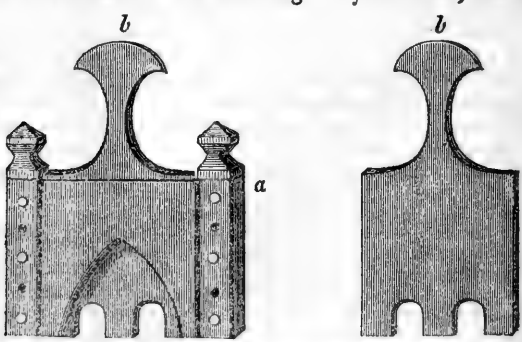
* Mr. Mills, in his treatise published by order of the Society, says, "the moths break the combs to pieces!" He could never have seen a hive which the moth had attacked: they are the feeblest of all insects, and incapable of breaking the combs; the effect of the attack of the moth is as I have described it. I never had but one so destroyed; but such is my opinion of the certain and destructive effect of the moth in hives, that it is sufficient for me to condemn a hive, and take it in the ensuing autumn, from which I have occasionally seen a moth come out, or into which this insect has entered.

which time only they are visible on their webs in the centre of them, and are easily taken by the hand and destroyed. These are the insects which Virgil points out—

——— dirum tineæ genus, aut invisa Minervæ
Laxos in foribus suspendit aranea casses.—VIRG. *Georg.* iv.

But of all the numerous enemies which bees have to encounter, the most daring as well as the most destructive is the wasp. Rapacious, lively, and bold, he attacks the hive; he enters it without fear, and plunders it without mercy: to them many hives fall sacrifices in the course of every season. I have found the only efficient remedy is, to destroy their nests in the neighbourhood of the apiary: they are easily discovered in the side of a ditch, or near the water, where the decayed roots of the alder have left a cavity, in which places I have observed they much delight to breed: this is done at night, when they have all returned to their nests, by putting port-fire into their holes, or by burning straw on them: their nests, however, are not discovered until they have become numerous, and so attract observation by their flight, directed in numbers to a particular place; and before they are so discovered, they commit great and destructive devastation among the hives. I have uniformly closed the entrance of my hives by the means pointed out in the following drawing, where the bees enter the hive by two small doors, sufficient only to admit a single bee at a time.

After trying the common experiments of hanging up bottles of honey or sweet liquor near the hives, I found I lost as many bees in those as I destroyed wasps: and it therefore occurred to me, that the best mode of securing the hives would be to narrow the doors while the wasps were out, or on the wing, by some expedient which at other times would allow the bees their full scope of exit and entrance. The following is my invention, and it has



answered admirably, not for the purpose only of enabling the bees

to

to defend themselves from the wasps, but to shut them up during the autumn and winter; during the autumn to keep out the vermin, and in winter to keep them warm *, yet give them air. This I put on the hives early in August, as I by these means can give them more or less room to work, at the same time that I thereby exclude the entrance of every insect whatever; and to this early closing up the doors, but with a power at the same time of giving them full liberty to go out in any number, by lifting the slide, I attribute much of my success in the preservation of my stock hives, as their stores are left untouched, and they are kept dry, and all offensive insects, such as snails, moths, &c. are thus excluded.

There are four periods or seasons at which bees are to be particularly attended to. The first I consider to commence from the earliest bloom of the willow, and blossom of the gooseberry. It has been recommended at this time to lift the hive, and sweep the board, as the floor of the stool will be found covered with the dross of wax, which was what closed up the combs at the beginning of winter, and when the cell was tapped for use, the wax would fall to the ground. I have never adopted that practice, for this reason, that to do so, the hives which had been plastered close to the boards during the winter, must necessarily be lifted from them, and the cold thereby introduced, which, at that season, I think would do more injury to the bees, than any benefit which they would derive from taking away the loose wax from the board. If at this season they appear brisk, and issue from their hive in numbers, let no feeding on any account be allowed. It dulls their early activity and industry, by their finding at home what they would othewise gain abroad; and the honey drawn from the flower gives them a more healthful activity than they could have from any feeding: but if a hive is observed when struck, to emit a dull and heavy buzz, and if the bees do not issue in any numbers, it may be of service to feed them, but scantily, and merely to give them strength to go abroad. From this time, until the period of swarming, they require some little attention; if the season is open, dry, and mild, in fact none; but if May should be wet and cold after the first swarms are cast, so that

* The drawing *a a* represents two small pieces of wood about an inch and half long, about the sixth part of an inch broad, and of the same thickness; there is a groove in each, to receive a sliding piece of board *b*, about an inch and a quarter wide, in which are cut two small doors wide enough to admit one bee at a time only. The pieces of wood *a*, are fixed on each side of the door cut in the hive itself, and fastened to the hive by two large pins; the piece of wood *b*, is then fixed in the two grooves, and is raised or put down at pleasure.

they cannot work, they must be attended to and fed, as they have no support from within, or they will perish*.

During the summer the only attention requisite is to watch the hives which show any indication of swarming, to take care that none go off.

As I do not profess to give full and perfect directions as to every matter connected with the hiving of the bees, I content myself with recommending it as essential to the hiving of good stocks of bees, that the swarm, when hived, should nearly fill it, and if it does not, to add to it another swarm or cast, so that it shall fill it; the reason for which I have already given.

I now come to the season when the honey is to be taken, a task never undertaken without reluctance, nor executed without regret. When from the failure of the flowers, little or no addition is made to the stock of the hive in wax or in honey, and I calculate on none after the middle of August, having ascertained from observation, that to be the fact, I consider that that is the proper period to take those hives which are too weak to get through the winter. This only reconciles us to the taking of them; for if not then done, a slow and lingering death by famine awaits them, their industry not having given them provisions for the season. I therefore begin to take my hives in the middle of August, by which means the honey runs more readily from the combs; the refuse, when thrown out, affords food for the bees who remain, and there are fewer competitors for whatever pabulum the autumn flowers afford.

The honey when taken, in order to be pure, fragrant, and well flavoured, must be such as flows spontaneously from the comb, cut into a hair-sieve transversely when the hive is taken; left to flow through the sieve into a vessel beneath, and never squeezed, or the running of it forced by heat; and after it has so flowed through the hair-sieve, it should be again strained through a finer one, so that not a particle of wax should mix with it, which creates a fermentation, and spoils the honey.

Honey, however, so prepared, by flowing from the combs, always becomes saccharine: but if the hive itself is kept, and the combs taken out as wanted, the beautiful and transparent honey will preserve its clearness, purity, and sweetness for at least a year.

I have until the last season, which was so unusually late, always considered, that bees add little to their store after the first

* In the last year I had a swarm on the 17th of May; soon after, the weather became very unfavourable, and continually wet. I was obliged to feed them for a fortnight; very many died, but the survivors repaid my care, and when the weather cleared up began to work. At the end of the season, they gave me a hive of near thirty pounds weight.

or

or second week in August: at this period, therefore, and for the ensuing month, much attention is required, to protect for them what they have acquired, as well from the depredations of wasps, the most destructive plunderers of their stores, as from the spoliation of each other. When an old hive has the unconsumed honey of former years in any quantity, in addition to what the present year has given, and the number of bees is small, it is astonishing to observe how soon the weakness is discovered, and that hive is marked out as a prey to its neighbours.

The constant buzz that is round it, caused by bees flitting round the door, and on their attempting to light being attacked by those on the stool, who are guarding their magazine, is an infallible sign and direction that that hive is marked for plunder. On these occasions I have closed up the door to a very small entrance, and I have covered the front with a cloth: it has given only a temporary suspension to complete pillage; for nothing will save it, or deter its assailants from their object. The hive is attacked by numbers, overpowered, and all their honey taken and carried away. Finding, therefore, the absolute impossibility of saving from the pillage of its neighbours a hive rich in honey, but poor in bees, and unequal to its defence when its weakness is discovered; I uniformly take it, considering that it may be useful, and save other hives which I might be disposed to take, from a mere trial of their weight, but in which the number of bees was considerably larger.

I am now arrived at the last period of my tract, but not the least important, the judgement to be pronounced on such as are to be kept for stock hives; either relying on their acquired stock of honey, or such as are to be brought through the winter by means of feeding, and what is necessary for bringing them through that season: on this much depends: but whatever hives are selected for that purpose, they should be closely plastered with lime to the stool: this contributes to their warmth, to the exclusion of vermin and insects; and if any water falls on the board, it does not reach the hive, which would otherwise become rotten and damp at the bottom: but when plastered round and up to the door, there is no entrance but through the two nicks in the slider. The front of the bee-house should be covered with board or oil-cloth, the hives pushed back, whereby they become darkened, and the bees less disposed to go abroad in winter. If the number of hives is so great that any given number may be selected, that selection depends on the weight of the hive, and the heaviest should be kept for stock, taking care, however, not to be decided by it only, but to endeavour to ascertain whether they are also strong in numbers: this can be known from having observed them

out

out at work in autumn, or by throwing the loose combs of the taken hives from whence the honey has flowed, on the grass before the bee-house, from whence they will issue in great numbers to suck the refuse honey remaining in the combs; and they may be then watched on their return to the hive, and their numbers calculated. This attention to the number of bees, as well as to the weight of the hive, is a matter of observation indispensably necessary, and on which every thing depends. It often happens that a hive heavy in honey has overswarmed itself, and the number remaining in the hive is small; such is perfectly unfit to be carried through the winter. I have always weighed my hives at the close of autumn, not by guess, but accurately by the scale and weights; I weigh them with the board on which they stand, having before ascertained pretty nearly the weight of the stand. If the hive weighs twenty-five pounds, it will safely get through the winter without feeding; I have had some have gone through it which weighed twenty-two or twenty-three pounds only; but upon these, without feeding, I cannot absolutely decide on their abilities to pass the months of winter and the early part of the spring. But with the aid of feeding, it is perfectly easy to bring any hive through the winter. And here I beg leave to give the most decided contradiction to any author who has treated it as an idle, useless, and almost impossible expedient to save such a hive during the season. Many have done so, and many such assertions I have often heard made, but from which I totally dissent, and from repeated experience am enabled to contradict them. I have had many supported by these means during nearly the whole winter, and those at the ensuing season have thriven out of measure, and become my best for the ensuing year.

It frequently happens, that in a late and wet season, the swarms come out in the latter part of June, and also numerous, strong, and large casts of the same description; they have not time to complete their store of combs and honey, but they are valuable for their numbers and their future promise; their industry only enables them to half fill their hives, and gather a support for a few winter months. Nothing is of more facility than to bring those hives through the winter by feeding; and when that is done, the remaining half of the hive is filled early in the succeeding year; and though they probably will not swarm, from having their hive to fill with combs, as well as honey, I have always found the quantity of the latter to be considerable, and the number of bees great in proportion: their attention is undiverted by throwing out swarms; their brood is employed at home, in what, if hived in a new dwelling, they would be called upon to do, filling it with comb as well as with honey.

For

For the effect of this I can answer. It has been done by me repeatedly for many years back, particularly at the commencement of this my favourite pursuit; and so far am I from thinking the feeding of bees to be a useless and unavailing attempt, that I have, without deviation, year after year adopted it, beginning the feeding earlier or later according to the weight or strength of the hives when they were put up in October. Having ascertained their relative weights, I calculate from them how long their stock of honey will last them, and their feeding is commenced accordingly. My whole stock is numbered 1, 2, 3, 4, &c. and on coming to town, I leave with my servant instructions in this way: for example, No. 1, to be fed at Christmas; No. 2, the middle of January; No. 3, the latter end of February; No. 4 need not be fed, as I consider it to be strong enough to get through the winter without assistance.

A decided advocate, therefore, for feeding bees during the winter, and having ascertained the utility and advantage, as well as the mode of putting it in practice, I communicate my method, as I have successfully pursued it. The composition which I give to my bees is moist sugar and sweet beer boiled to the consistence of treacle; it is inserted into the hive in a small trough or scoop made of wood, hollow, and of this shape.

This is to be filled daily. To insert it into the hive through the front door, would be to bring the bees of all the adjoining hives to share it, and to engage them in fight and destruction: my bee-house opening wholly at the back, I cut a hole large enough to admit the trough at the back part of the hive; and when the trough is inverted I shut the outside doors quickly: thus no bees of the adjoining hives are roused; and the hive fed, enjoys the whole of what is given to it: the only attention required on the occasion is, to take care that the board or stool of the hive is perfectly level, lest the food should be spilt, which would take place if the board was higher either in the front or back part of the hive.

It is a received opinion among the common people, that no hive should be suffered to stand longer than three years; for it will certainly die in the fourth, or be good for nothing. No opinion can be more erroneous: I last year took a hive which had stood fourteen years, which I kept for the sake of the experiment, and which gave me a swarm the last season; but having cast a swarm last year, I found the number of bees so re-

duced,

duced, and the hive so light, that it was impossible it could get through the winter, and I therefore unwillingly took it.

I shall now conclude what I have the honour to offer to the Society with this observation, that the reward which the Society offers, either pecuniary or honorary, should be held out to him who produces the hive of given dimensions, of the greatest weight, and of course containing the greatest quantity of honey and wax. To give to him whose pretensions are the number and not the weight of the hives, is to enable any one to practise a fraud on the Society, whose object is, to hold out encouragement to the production of the greatest quantity of wax and honey, by inducing him to hive as a distinct stock, every late small swarm or cast, not one of which I have ever found yield the smallest quantity of honey fit for any purpose whatever.

I declare to the Society, that I was possessed of fifty-six stocks of bees, previous to my beginning to take them in August last, 1817, all of which were composed of my old stock of the preceding year, and of swarms from them in the year 1817. My stock of the year 1816 consisted of 52 hives, 24 of which I carried through the winter without the loss of one, having taken the rest. From the 24 hives I had upward of 40 swarms and casts, which were hived in 32 hives.

Isaac Espinasse.

LIII. *Catalogue of Ancient Eclipses, with the Dates of their corresponding Eclipses at one and two Periods Distance. With Remarks.* By Mr. Thomas Yeates.

[Continued from p. 248.]

Remark 6. In 912 solar years the Julian calendar falls short of the true solar time by seven whole days, at the rate of eleven minutes per annum; and therefore, to reduce the ancient with the reformed calendar, seven whole days must be added to the former, to bring it up with the latter during one lunar period.

7. And in 912 solar years the moon gains eleven whole days when she comes up with the sun within four days, which produces a compound equation and a third number, amounting to three days in the joint motions of the sun and moon in one period, according to the table of the ancient eclipses, and the doctrine of the *anticipation of the moon*; but whether this recession of the conjunctions of the sun and moon in the ecliptic is any *real* and absolute anticipation, or whether it is produced by some unknown variation of the calendar reckoning during the above period, is an astronomical question to be inquired into.

8. The

8. The falling back of the conjunctions of the sun and moon one day in 304 years, or three days in 912 years, extends the great lunar period to 120 periods of 912 years, or upwards of 100,000 years, which is an unbounded length; and however enormous this may appear, yet an author since Mr. Ferguson has calculated the grand lunar period at 7,948,800 synodical revolutions, or more than 600,000 years, to go through all her variations and complete her cycle. Mr. Ferguson, it must be allowed, was more decent when he assigned the very moderate period of 12,492 years! See his Astronomy, art. 321, p. 245. All this arises from our imperfect knowledge of the lunar theory.

9. If my positions are true, the entire revolutions of the moon are limited to the period of 912 solar years, in which time her relative motions with the sun, and the whole phænomena of eclipses, are completed. The very supposition of enlarging this period by any supposed anticipation of either sun or moon beyond this period, produces nothing but doubt, uncertainty, and useless speculation.

10. The reform of the calendar in different ages may in some cases affect the true dates of ancient eclipses; but I cannot discover that even the reform of the calendar in the age of the Nicene Council has at all affected the dates of the eclipses prior to that period, nor subsequent thereto, until the time of Pope Gregory, and the introduction of the New Style.

11. It appears from the ancient writers, that the memorable year commencing the æra of Nabonasser 747 years before Christ was a famed astronomical epoch, when the Babylonian and Egyptian calendars coincided with the equinoctial points; concerning which Dr. Keill remarks, Astron. Lectures, xxvii. p. 367: "The æra of Nabonasser," says the learned Professor, "has always been famous among astronomers, and began on the sixth of February of the Julian year carried backward, and before Christ 747 years: and because that day was then the first of the Egyptian year, Ptolemy and after him Copernicus computed the motions of the stars according to that æra by Egyptian years."

12. If we compute the difference of the Julian and solar reckoning from the said epoch, at the rate of eleven minutes per annum to the date of any eclipse, and add thereto seven whole days for one period afterwards, we shall reduce those Julian dates to the true solar account; and by another equation, viz. subtracting four days for any period amounting to seven days, or fourteen days for two periods: by these two simple equations we shall be enabled to reduce all the above ancient eclipses to an harmony with the modern tables with a surprising exactness.

To reduce the date of the eclipse of the sun observed by the Chinese astronomers twenty-four years before the Christian æra, see page 247, find the solar equation from the epoch A.C. 747 —24 = 723 years, which at eleven minutes per annum amounts to five days twelve hours thirty-three minutes: add this to the date of the said eclipse, and it will stand equated as under:

				D.	H.	M.
A.C. 24	⊙	Pekin	April	7	4	11
		Solar equation	+	5	12	33
		Equated time		12	16	44

The solar equation not amounting to the quantity of one period does not affect this example.

The return of this eclipse was in A.D. 889 observed by the Grecian astronomers at Constantinople: this being at one period distance, seven whole days must be added to the former equation, and from the sum the lunar difference of four days must be subtracted as under:

				D.	H.	M.
A.D. 889	⊙	Constantinople	April	3	17	52
Solar equation		.. ..	+	12	12	33
				16	6	25
		Lunar difference	—	4	0	0
		Equated time	..	12	6	25

Thus this eclipse returned, after 912 years, within ten hours nineteen minutes of the former.

The second return was in April 1801, and although invisible in England was computed in the Nautical Ephemeris to happen April twelve days sixteen hours twenty-one minutes; which is one hour and a half of the equated time it was observed 1824 years before. Now in the year 1801 the Julian style stood corrected by twelve whole days; therefore to bring back the date to the ancient style, twelve days must be deducted, and the date stands as follows:

				D.	H.	M.
A.D. 1801	⊙	Eclipse.	O.S. April	0	16	21
Solar equation		.. ..	+	19	12	33
				20	4	54
		Lunar difference	—	8	0	0
		Equated time	..	12	4	54

Thus by two equations only these eclipses happened on the very same day, and within a fraction of a day.

The next example I shall take from the date of the memorable and total eclipse of the moon at Arbela, which happened in the

the year of the Julian period 4383, on the 20th day of September, a little before midnight, or A.C. 331, when Alexander the Great overthrew Darius, and decided the fate of all Asia.

		D.	H.	M.
A.C. 331 ☽ Arbela	September	20	20	9
Solar equation for 416 years, viz. 747−331=416 ..	+	3	4	16
Equated time ..		24	0	25

This eclipse returned visible at Paris

		D.	H.	M.
A.D. 582 ☽ Paris	September	17	12	41
Solar equation 3 4 16 / 7 0 0 ..	+	10	4	16
		27	16	57
Lunar difference	−	4	0	0
Equated time ..		23	16	57

The difference between the equated dates of these eclipses is therefore seven hours twenty-eight minutes.

The second return was

		D.	H.	M.
A.D. 1494 ☽ Eclipse	September	14	19	45
Solar equation added to the former	+	17	4	16
		32	0	9
Difference for two periods ..	−	8	0	0
Equated time		24	0	9

Therefore the second return of the eclipse seen at Arbela happened within sixteen minutes of time at two periods, or 1824 years distance. The year A.D. 1494 we see does not stand affected by the reformation of the Nicene calendar, although it may be corrected from that date, but from the epoch of the Nabonassarean æra; and hence it should appear that the astronomical calendar remained unaltered until after ages.

If we correct the date of the eclipse A.D. 1494, by the rule on which the Gregorian calendar was corrected A.D. 1582, the difference will be found nine days to be added, and the corrected date will correspond with the above, thus:

		D.	H.	M.
A.D. 1494 ☽ Eclipse	September	14	19	45
	+	9	0	0
Corrected date		23	19	45

Thus we see the equations here assumed are verified by several eminent examples. [To be continued.]

 LIV. *Me-*

LIV. *Method of preventing and curing the Dry Rot in Ship Timber.* By Ambrose Bowden, *Esq.**

Navy Office, April 9, 1818.

Sir,—In June 1815 I published a treatise on Dry Rot, a copy of which I presented to the Society for the Encouragement of Arts, &c. I am now happy to inform the Society that several of the measures I then recommended have been since adopted by Government, particularly that of sinking ships in sea-water, which has been attended with success equal to my most sanguine expectations.

A ship of 451 tons (the Eden) was sunk in November 1816, and raised in March 1817; and on her being taken into dock and opened last month, there was not the least appearance of recent fungus, although many of her timbers had been destroyed by this vegetation, previously to her being sunk; and what old fungus was found on the timbers had been completely deprived of vegetable life. It is with infinite satisfaction I reflect on having discovered a simple, easy, cheap, and effectual remedy for a decay which has consumed the navy for many years past, at an expense of many millions sterling to the public. I now beg to appeal to the liberality of the Society for that consideration which so important a discovery deserves. I shall be happy to wait on the Society with documents sufficiently attested to prove the truth of these representations.

I am, sir, &c.

A. Aikin, Esq. Secretary, &c. A. Bowden.

Navy Office, April 12, 1818.

Sir,—I omitted to state in my letter, as a confirmation of the efficacy of sinking ships to cure the dry rot, that many pieces of timber, which had lain for some years in the yard at Milford, had been much decayed by the growth of the fungus; but after they were immerged in the sea, the vegetation was entirely destroyed, and those timbers which were not too rotten, are now used for building ships without the least apprehension. In consequence of these satisfactory experiments, an order has been issued by the Lords Commissioners of the Admiralty, directing *all the timbers* and planks to be submerged after they are cut to the proper shape, and previously to their being used for building a ship. They have also ordered the Mersey (a ship built at the same time as the Eden, of the same tonnage, and nearly in the same state of decay) to be

* From the *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* for 1819. The Society's gold medal was presented to Mr. Bowden for this communication.

repaired

repaired immediately, and sunk when she is completed, in order that the *new timber* used in repairing her may have the benefit of submersion as well as the old. I beg also to state to the Society that I have learnt, since the publication of my treatise, that accident has furnished us with two other instances of the good effects of sinking ships, in the San Fiorenzo frigate, which was sunk off the French coast about 18 years ago, and the Resistance frigate, which was also sunk in attempting to heave her down in the Mediterranean about eight or ten years since: the latter ship is now under repair at Plymouth, and is perfectly free of fungus, which excited considerable surprise and inquiry; and the former is in the same favourable state. The fact, therefore, appears to be established as completely as any fact can be proved, the Eden having been raised one year, the Resistance about ten years, and the San Fiorenzo about 18 years. Nothing more strongly shows the opinion entertained by the Lords of the Admiralty than the orders they have issued, and the consideration that they never would have given such orders (so contrary to former prejudices) on any other than an unquestionable foundation.

I am, sir, &c.

A. Aikin, Esq. Secretary, &c. A. Bowden.

Besides the foregoing letters, copies were communicated to the Society of the following official papers confirming the statements in Mr. Bowden's letter, viz.

1. A recommendation from Mr. Commissioner Seppings, that the Eden and Mersey sloops-of-war, both infested with fungus, being new ships, built at the same place, and launched in the same year, should be selected for a comparative experiment to ascertain the effect of submersion in sea-water. The Eden to be submerged, the Mersey not to be submerged.

2. Warrant to the officers of Plymouth yard to sink the Eden, November 16, 1816.

3. Survey of the Eden before submersion, November 16, 1816.

4. Report on the state of the Eden and of the Mersey, after the former ship had been submerged, September 8, 1817.

5. Report on the state of the Eden, March 7, 1817.

6. Letter from the Navy Board to the Admiralty on the state of the Eden, March 11, 1818.

7. Order from the Admiralty (7th April 1818), to repair the Mersey, and afterwards to sink her, that the new timber may have the benefit of the immersion in sea-water.

LV. *Re-*

LV. *Remarks on the "Proposal for establishing a more correct Account of Civil Time," published in our last Number; with a Suggestion for a still more correct Account.* By Mr. James Utting, *of Lynn Regis.*

To Mr. Tilloch.

Sir,—I observed in your valuable Journal for April 1820, page 314, "A Proposal for establishing a more correct Account of Civil Time," affecting to be *no less simple than the Gregorian computation.*

That it is equally *simple* I acknowledge, but not that it is also correct: the average tropical year to which your correspondent W. W. approximates exceeds by more than eight minutes that of the true solar year as used in our best solar tables. This error in 500 years would amount to nearly three days, an error twenty times greater than exists in that which he attempts to correct.

The following appears to come nearer to the point of accuracy (which I discovered ten or twelve years since in the act of constructing a comparative table of years of different denominations): *i. e.* 900 revolutions of the sun* are performed in 328718 days precisely, which is the least whole number contained in any period of solar years constituting complete *centuries.* This period contains seven days less than the Julian account: consequently, by suppressing seven days in 900 years, the calendar would stand corrected, *i. e.* every fourth and fifth centenary year alternately would be leap years, but the intermediate centenary years would constitute common years; so that instead of suppressing three days in four centuries as in the Gregorian account, we ought to suppress seven days every 900 years: thus a correct calendar may be obtained in reference to the solar year, admitting it invariable. But according to the celebrated M. Laplace, the tropical year is found to diminish one second in about 460 years; and admitting this decrease uniformly to continue, forty millions of years would elapse before the error would accumulate to a single day! And as the accelerated motion of the earth in its orbit has its limits, it would probably change to a retardation before the above period terminates, and a consequent increase in the length of the solar year would obtain. Hence the above correction of the calendar would undoubtedly be the most simple

* The true solar or tropical year at the commencement of the present century, as deduced from recent observations, contains 365 days five hours forty-eight minutes forty-eight seconds.—Vide Laplace's *Système du Monde*, and Vince's Astronomy, 3 vols. quarto, 2d edition (1814).

and

and perfect that has hitherto been discovered, according with the present existing order and constitution of the planetary system.

I remain, sir, with respect,

Your most obedient servant,

Lynn Regis, May 5, 1820. JAMES UTTING.

LVI. *Letter from Mr.* CAVIGLIA *to the Editor of the* '*Journal des Voyages.*'

Paris, Nov. 23, 1819.

IN your Number for July, noticing the voyage of M. de Forbin in the Levant, you express his concern, that he was unable to profit by the discovery of the Temple of the Sphinx, which an unpardonable egotism, he says, had caused to be buried up or covered again. As this leads to an implication, that it was Mr. Salt who discovered that beautiful monument, I think it right to exculpate this gentleman from the above charge of egotism.

It was I, and not Mr. Salt, that caused the temple to be covered up again; and here are my reasons for it. I had already removed obstructions from the newly discovered passages, and from the new subterranean chamber of the great pyramid; and finding nothing all around but the live or natural rock stone, I set about exploring the base of the Sphinx, in hopes of lighting on some communication that might lead to any new points of the pyramid. After having been at work for several months, with a hundred and fifty Arabs, and not unfrequently at the risk of being buried in the downfalls of sand, I was at length enabled to clear out the area of a temple of Osiris: its site at about the depth of 40 feet, and within the very claws of the Sphinx. M. de Forbin is within the limits of strict truth, when he asserts that this is one of the finest monuments of the power of the arts in ancient Egypt.

After having taken the dimensions and the most correct designs of all these antiquities, I was concerned to find a number of Arab women, allured by superstition, coming, at first, to worship and kiss the images, on their first view of them, but, not content with this, proceeding afterwards to break off fragments or pieces to serve as amulets or charms: in this way, several hieroglyphics have been already disfigured. At length, being apprehensive that this fine workmanship, which it had cost me so much labour (even at the hazard of losing my sight) to explore, should come to destruction, I resolved to inter it anew, till circumstances more auspicious might authorize the disclosure of it to every eye.

The learned will, I hope, be shortly enabled to appreciate these antiquities, whether deserving or not of the care expended for their preservation. It is intended to publish, as soon as possible,

sible, the result of my discoveries, in a periodical journal: my plan of the temple, and a brief notice of my labours, have indeed already appeared in one of these for January last.

It appears to me that the whole aggregate of Egyptian antiquities would speedily be laid open for the investigation of European archæologists, were it not for a sort of jealous rivalship that has crept in among the explorers of these scientific riches. The most valuable and indeed the most proper instrument for these purposes, in respect of his physical force and capabilities, I mean M. Belzoni, is about to leave Egypt. A report prevails that, on his return to Cairo from his last expedition, one of the agents of M. D. assaulted, and actually fired a pistol at him. This circumstance was mentioned to me by M. Briggs, on his arrival from Alexandria.

This event should not, however, be a source of alarm to Europeans inclined to undertake journeys into a country so interesting. For my own part, it is my intention to return thither; and I can only speak well of the native inhabitants, with whom I have lived for some time in the bonds of an amicable intercourse.

LVII. *Reply by Mr.* P. J. Brown *to the Answer of a Correspondent on Professor* Jameson's *System of Mineralogy.*

To Mr. Tilloch.

Sir, — A correspondent under the signature of G. M. having, in your Magazine for April, offered some reply to my remarks on Professor Jameson's 3rd edition of his System of Mineralogy, I beg to submit the following observations in return.

I am first accused of confounding a natural history method with a natural method. Not having had the advantage of Professor Jameson's instruction, I can reconcile myself, without an effort, to my incapacity for appreciating distinctions without differences; and shall be content with the observation that, whatever shades of Jamesonian twilight may exist, a natural history method at variance with a natural method is a solecism in language certainly beyond my "inferior apprehension."

G. M. erroneously supposes Professor Jameson to have been censured for deviating from a *chemical arrangement:* he is blamed for adopting a *derangement* which compels him to deviate from *nature.*

"Nunquam aliud natura, aliud sapientia dicit."

Your correspondent evidently considers me a staunch advocate for a chemical system. I can with pleasure assure him, that I am am not either in mineralogy, or any other subject, bigoted to a particular theory: and that I will cheerfully abjure my chemical heresy the moment any one will point out a safer and more certain guide. At present I certainly prefer a system which tells me what a thing *is*, to one which informs me what it *looks like.* I prefer a system which admits pure "white lead spar" to be an ore of lead, to one which, in direct opposition to truth and common sense, calls it an earth! Anxious for improvement, I have inquired amongst the best mineralogists of my acquaintance, by what anti-alchemy the numerous metals in the Professor's second volume have been transmuted into earths. Some have stared; others have smiled and shook their heads; but I have not yet been fortunate enough to meet with one who could *explain.* Finally, I prefer an arrangement, let its basis be what it may, whose consistency with natural affinity will enable me to judge with some degree of correctness where I am to look for a mineral, to a chaos which compels me to refer eternally to the index, or to dive at random amongst the leaves for a quarter of an hour, consoling myself with singing

"Whither, my love, ah! whither art thou gone?"

I am told that I "cannot perceive the high advantage of being able at once to place a mineral either in the earthy or metallic class, from the simple and obvious distinction of specific gravity." What an unfortunate observation! Has G. M. ever seen the book he is vindicating? Has it escaped his notice, that some of the Professor's new earthy minerals are upwards of 6·9, and his metallic ones as low as 4·0?

We are also told, that by adopting the method in discussion, we should be enabled "to recognise a mineral at first sight, and give it its proper place in the system." Here is at once the point at issue:—and the above proposition I unequivocally deny. It would enable us to form groups of heavy minerals, white ones, black ones, and shining ones;—in short, to form such an arrangement as would be adopted by a school-boy; except that the school-boy's system, not being spun into such fine distinctions, would enjoy the advantage of having the limits of its divisions better defined.—G. M. does not appear to be aware of the source of that "precision which has conferred such superiority on botany and zoology."—No such superiority could be boasted while the Jamesonian principle of externals guided the botanist, and produced an arrangement divided into trees, shrubs, undershrubs, and herbs; the pre-eminence arose from the rejection of externals, for the study of organization and natural affinities; a plan which did not give rise to the facility pointed out by G. M.

 but

but sacrificed facility to precision*: and if mineralogy be doomed to rank amongst the sciences, its foundation must rest on a firmer basis than colour, hardness, and weight.

As for the discrepancies which are "so trivial as scarcely to deserve attention," they may rest in peace: it is waste of words to argue for mathematical exactness with men who think nothing of an unit or two in specific gravities, and eight or ten degrees in the measure of a right angle! †

We are reminded how easily I might perceive that the observations to enable a student to distinguish particular minerals from those which they more nearly resemble, are not delivered as essential characters. This I did perceive; and considered that very circumstance a great objection. The principal complaint is, that by "a very absurd adherence to a system" (to use G. M.'s own words) the essential characters are made to depend wholly on circumstances which have frequently nothing characteristic in them; and the discrimination of the species is left to be ultimately determined upon principles which the founder of the system has not the candour to avow.

Towards the conclusion we are told, that the excellence of a system will consist in "laying down such methods, founded on the external characters of minerals, as shall afford the means of immediately placing the mineral in the class, order and species to which it belongs." Expunge the word "external" and the sentence will appear unexceptionable; but, while minerals remain destitute of organization, and liable to continual variations from accidental contamination with foreign substances, it will be found that a system, *altogether dependent on external characters*, will never impart a degree of knowledge worthy the name of a scientific attainment. Of this we have a lamentable proof in the work under discussion, which presents us with groups of earthy minerals without an atom of earth in their composition.

When the observations which you honoured by insertion in your Magazine for January were written, I had not learnt any

* Facility applied to the obsolete system, which enabled any one within sight of an oak to decide on its belonging to the 1st class, or trees. Linnæus rejected such misleading facility, and obtained precision by the more careful and scientific examination which guides the modern botanist in referring it to the class *Monœcia*.

† "By describing a mineral thus" (according to Werner's system, the *vis vitæ* of the externalists) "native sulphur, whose specific gravity is 2·0332, and telesia, which weighs 3·9941, would be included in the same expression."—Chenevix in Phil. Mag. vol. xxxvi. p. 420.

"I have heard M. Werner say (and I have written his lectures as he delivered them) that a difference of 10° did not prevent him from considering any angle as a right angle." Ibid.

other

other person's opinion of Professor Jameson's 3d edition: since then I have had the opportunity of being made acquainted with the sentiments of many of the first mineralogists in England; and I can assure G. M. that he affords the first instance, within my knowledge, of any one having expressed a single sentence in its defence.

To the advocates of the new system I would give one piece of advice: When they undertake its defence, let them explain those things which must otherwise appear absurd, instead of vindicating in general terms; or Professor Jameson, like many others, may have occasion to exclaim: "Save me from my friends, and I shall not fear my enemies." At the same time I am ready to admit that credit is due to G. M. for his anxiety to repel the censure to which his old preceptor has exposed himself: having been educated in the Professor's principles, he does not see the absurdity of them, and affords proof of the correctness of another passage in the author already quoted:

——"Docilis imitandis
Turpibus ac pravis omnes sumus."

I remain, sir,
Your most obedient humble servant,
P. J. Brown.

Thistle Grove, Old Brompton,
May 6, 1820.

LVIII. *Account of the Importation into France of the Cachemire-wool Goat, extracted from a Memoir on this Subject read to the Royal Academy of Sciences, September* 1819.

M. Amadeus Joubert quitted Paris in April 1818, and proceeded first by Odessa, Tangarock, and Astracan, to the camp of general Jermoloff in Caucasus, gaining information on the way relative to the object of his journey, from the Bucharians, the Kirghiz, and the Armenians, who frequent Astracan. He was there told that there existed amongst the numerous hordes of Kirghiz (a nomadic tribe residing in Bucharia, on the banks of the Oural lake) a species of goat of a dazzling white, bearing every year a remarkable fleece about the month of June. The specimens of it which he there collected convinced him of the identity of this wool with that which is imported into France through Russia. This discovery was the more important to him, as it promised to save him the long and difficult journey which he would otherwise have had to encounter in penetrating to Thibet through Persia and Cashemire. In this he was not deceived; for he actually collected scattered samples of this fine wool, at some hundreds of wersts from the Wolga, amongst the *steppes* that separate Astracan from Oremburg, which satisfied him that he needed not to penetrate further. He had besides observed, that in the language of the country they gave the name of Thibet-goat to the animal which furnished this fine fleece. He therefore bought of the Kirghis in this district, from the hordes called *Cara-Agadgi* and *Kaisacks*, twelve hundred and eighty-nine of these animals, and directed his course homewards with them by Tsaritzin, where he brought them across the Wolga. After making all the deductions from this number occasioned by losses on the road, by the shipment of them at Kaffa, and the passage home, there now exist in France four hundred of this stock of Cachemire-wool goats.

During a short stay which M. Joubert made at Constantinople, in his passage homewards with his goats, he held a conversation, through the second interpreter to the French embassy, with an Armenian named Khodja-Youssuf, who was sent eighteen years ago by a house in Constantinople into Cachemire to procure shawls made after patterns which he carried with him. This Armenian resided a long time in Cachemire, Lahore, and Pichawer, and in learning the language of these countries, he obtained much positive information as to the manufacture of these valued articles. He stated to M. Joubert, that the animal which yields this beautiful material is neither a camel nor a sheep, as some have reported, but is a goat, resembling the common goat in appearance, having straight horns, and a white or clear brown coat. A coarse hair covers the fine downy wool, which last is the only material from which the shawls are wove.

Khodja-Youssuf had seen at Cachemire twenty or thirty of these goats, which were kept there for curiosity. The women and children pick out the fine wool from the coarse hair, and other heterogeneous matter; which is afterwards carded by young girls with their fingers on India muslin, to lengthen the fibre, and clean it from dirt and foulness; and in this state it is delivered to the dyers and spinners. The loom that is used is horizontal and very simple; the weaver sits on the bench, a child is placed below him with his eyes on the pattern, and gives him notice after every throw of the shuttle, of the colours wanted, and the bobbins to be next employed. The finest shawls cost from 5 to 600 rupees (12 to 1500 francs). The most beautiful wool comes from the provinces of Lassa and Ladack in Thibet; and also a good deal of it is imported into Thibet and Cachemire, from Casgar and Bucharia, all of which goes to form the fine shawls, of which there is such a great demand throughout Asia. The fine wool is brought into Cachemire in bales, mixed with coarse hair.

LIX. *Re-*

LIX. *Remarks on an important Error in a Table for computing local Attraction, circulated by Order of the Board of Longitude.*

To Mr. Tilloch.

Deal, May 1, 1820.

SIR, — A YOUNG nautical friend of mine, whose perseverance in acquiring a complete knowledge of the scientific principles of his profession is only equalled by his diffidence and modesty, consulted me a few days back on a difficulty he had met with in the practical application of the table alluded to in the head of this article; and I soon perceived that it was not one that could be removed, as it arose out of some erroneous principle employed in the computation of the numbers in question; and as I conceive it to be of the highest importance that no error of this kind should be allowed to slide unnoticed into our nautical practice, I have been induced to draw up the following remarks, which may be the means of checking the evil that might otherwise arise out of the use of this table.

The subject of local magnetism has of late attracted the attention of many of our most eminent navigators and philosophers, and various observations have accordingly been made in different quarters of the globe, the results of which show that in the northern hemisphere, the north end of the needle is drawn towards the vessel, and in the southern hemisphere, the south end; or we may say generally, that the end of the needle which dips is always attracted by the iron of the vessel. It appears also by the observations of Captain Flinders, that the effect increases in both hemispheres as the dip increases; a fact which has been amply verified by the experiments of Captain Ross in his recent voyage to the arctic regions.

In consequence of this change of effect in parting north and south from the magnetic equator, it seemed to follow of necessity, that the attraction vanished entirely where the dip is zero; and such is the principle laid down by Captain Flinders, who however had obviously failed in establishing the law of increase of effect in different latitudes.

The recent voyages of discovery towards the north having thrown additional light on this subject, Dr. Young, the learned Secretary to the Board of Longitude, undertook to establish a new law of increase, and computed a table for the use of navigators, which is published or circulated by order of the members of that board, whereby the correction for local attraction is reduced to a simple logarithmic operation, *the dip being first ascertained.* It is to this table I am desirous of calling the attention of your nautical readers.

We

We have seen that all experiments confirm the fact, first noticed by Captain Flinders, that although the effect of local attraction is the same in quantity in north and south latitudes, where the dips are equal, yet it is directly opposite in direction, the north end of the needle being in one case attracted by the vessel, and in the other the south. For example: Suppose a ship's head to be east, at the Nore, and that the local attraction of the vessel at the maximum is half a point, then the north end of the needle will be drawn half a point from its true bearing towards the east. Now, at Port Jackson the dip is as nearly as possible the same as in England; but it being the south end that dips, the effect will still be half a point the contrary way; that is, the north point will now be deflected half a point to the west, the ship's head being east as before.

This contrary effect has place in both cases till we arrive at the equator, where according to Captain Flinders it vanishes entirely; but according to Dr. Young's table (although the effect must change as we cross the magnetic equator) it does not vanish, being at the equator itself equal to about one half what it is in England. This was the difficulty of my young friend, and it is certainly one which I am unable to explain; and I will add that it is a result absolutely impossible in itself, and contrary to that principle maintained by all philosophers, that the operations of nature are never made *per saltus*. To put the case in a stronger light, let us suppose three vessels, whose local attractions are equal, the *maximum* of each being half a point at the Nore: then according to Dr. Young's table, the attraction of each at and very near the magnetic equator will be about 3°. Let us suppose these three vessels to be a mile distance from each other, one being on the equator and the other two a mile on each side of it, the one in north and the other in south magnetic latitude; and let us further suppose, that they are all sailing due east: and that each takes an azimuth observation at the same instant. No one can doubt for a moment that these azimuths will be the same in each vessel, and let us suppose that they are such as to give the computed variation 10°. If now each vessel correct her variation according to Dr. Young's table, the one in south latitude will make her corrected variation $10° - 3° = 7°$ west, and the one in north latitude will make hers $10° + 3° = 13°$ west, a difference of 6°: while the vessel on the equator will have no guide to know in what direction her correction is to be applied. The case which we have here supposed is by no means one advanced for the purpose o exaggerating the amount of the error; it is a case that would actually occur in sailing from Pernambuco to the Congo river, and it is not improbable might be the means of running one of the vessels down immediately on the

the island of Ascension, while she would suppose herself in perfect safety.

Enough has now been said to prove that the principle upon which this table is constructed must necessarily be erroneous, and we hope it may be the means of preventing any further issue of the copies of it; for we must not consider an error of this kind in the light of a mere philosophical inaccuracy, but as one which may involve in it the destruction of lives and property to an immense extent. I make no pretensions to scrutinize the mathematical introduction to the table in question, I have every respect for the mathematical talents of its learned author, and have little doubt this part is correct; it is however highly probable that the error is in the data or hypothesis. It is certainly very improbable that iron at the equator should lose all its effect on a magnetised needle, as supposed by Captain Flinders; but it is actually impossible that the change should be such as is indicated in the table before us.

I shall make only one other observation, which is, that an error once introduced into nautical computations is extremely difficult to eradicate. The midshipman who learns the use of Dr. Young's table this year will use it all his life, and with the utmost confidence, from the authority and weight which the sanction of the Board of Longitude very properly has with men in the nautical profession; a circumstance, however, which renders it the more imperiously necessary that all the remaining copies of this table should be destroyed. Yours, &c.

NAUTICUS.

LX. *On the Alteration which Sulphuric Acid experiences in acting upon Alcohol.**

[Concluded from p. 295.]

It is in the 60th volume, p. 54, of the *Annalen der Physik*, September 1818, that the memoir of M. Sertuerner is to be found. This chemist affirms that the sulphuric acid, in acting upon the alcohol to form ether, produces three acids, which he designates by the names of *acidum protœnothionicum, acidum deutœnothionicum, acidum tritœnothionicum.* The following is the manner in which the first of these acids is obtained. Equal parts of sulphuric acid, and of pure, or at least very concentrated, alcohol, are mixed together; the mixture is heated, and when saturated with chalk, as soon as it is a little reduced, sulphate of lime is formed, which is separated from the liquid by filtration and pressing. On evaporating the liquid by a slow heat, and filtering it at the

* *From Ann. de Chimie*, Jan. 1820.

moment

moment when it is sufficiently concentrated to crystallize, there is obtained by the refrigeration the proto-sulphovinate of lime.

This salt is in small flakes; it has a peculiar burnt taste; it attracts humidity, and inflames when a lighted candle is applied to it. Strongly heated it becomes black and very acid. The decomposition which is announced by this black colour, takes place as soon as, by the progress of the evaporation, salt is deposited on the sides of the vessel; it extends even to the whole of the salt, if at the moment when it begins to manifest itself it is not stopped by the addition of a little chalk; and the liquor, in which no trace of sulphuric acid could previously be discovered, contains then a great excess of it. The acid of proto-sulphovinate of lime or of barytes may be easily obtained by decomposing the liquor by sulphuric acid. This new acid gives the whole secret of etherification, since it is produced with sulphuric ether. Many chemists believe that the sulphuric acid loses none of its saturating property in acting upon alcohol; but the reverse is the case, as any person may convince himself by testing the acid before and after its action on alcohol.

Almost all the organic substances and powerful acids act in the same manner.

The second acid (*deut-œnothionicum*) is found in the residue of the distillation of the ether, treated several times with alcohol and perfectly exhausted, but fresh, and not black or altered. This residue is saturated with chalk or with carbonate of barytes, as already mentioned. The deuto-sulphovinate of lime is very sweet, and possesses the property of absorbing the oxygen from the air, and of producing sulphate of lime, a good deal of sulphuric acid, and the third acid (*trit-œnothionicum*). It is thus seen why the residue of the preparation of ether restored to the air the property which it had lost of giving ether; and why it saturates then more than before. The sulphovinous deut-acid produces thus by decomposition much sulphuric acid and the sulphovinous trit-acid.

The sulphovinous trit-acid is obtained very easily by exposing to the air, until there is no longer any absorption of oxygen, either the second acid or the exhausted residue of the distillation of sulphuric ether. By saturating with chalk the trito-sulphovinate of lime is obtained, which is deliquescent when exposed to the air, and, like all acetates, combustible. It has very nearly the same savour as the first sulphovinate of lime, and has many affinities with this salt.

The sulphovinous acids are easily isolated from their combinations with lime or with barytes. Distilled, each of them gives a different product. The three sulphovinates of lime give, for example, by distillation, three new liquid crystallizable acids remotely resembling

sembling succinic acid and pyroligneous acid, sulphurous gas, sulphuric acid, sulphate of lime, and an ethereal gas of a very agreeable odour. Similar products are obtained with the acids formed by the powerful acids, and organized substances. M. Sertuerner mentions that in distilling the proto-nitrovinate of lime the fumes caused him inexpressible pains, like meconic acid; but of this he promised to say more thereafter.

Such are the principal facts contained in the memoir of M. Sertuerner relatively to acids formed by the reaction of sulphuric acid upon alcohol. He pleads the great extent of the subject which it embraced, as an apology for not entering much into detail on any of the points. But M. Gilbert properly observes, that it was only by entering into such detail that he could carry conviction to the minds of his readers; for it must be confessed that he is far from having demonstrated the existence of his three acids, and that by the vagueness of his assertions he has left the field free to those who, occupying themselves with the subject, place their reliance on precise experiments.

M. Vogel read to the Academy of Sciences of Munich on the 9th of October 1819, "Some Inquiries into the mutual Decomposition of Sulphuric Acid and Alcohol," &c. which he has printed in the *Journal de Pharmacie*, tom. vi. p. 1, and the object of which was to repeat the experiments of M. Sertuerner. We shall extract from this also some particulars.

Equal parts in weight of alcohol at 40° and concentrated sulphuric acid have been slowly mixed together, and the mixture kept eight hours in a flask closely corked up. The liquid after being diluted with double its volume of water was divided into two equal parts, one of which was neutralized by carbonate of barytes, and the other by carbonate of lead.

After separating the sulphates of barytes and of lead by filtering, the liquors were charged with salts of barytes and lead. These saline solutions evaporated to dryness and heated in a crucible yielded sulphurous acid gas, and a heavy white oil of ethereal odour; there remained in the crucible some sulphate of barytes or sulphate of lead. The sulphuric acid yielded similar results with three parts of alcohol.

Equal parts in weight of alcohol and sulphuric acid were set to boil in a crucible until ether began to escape. The crucible was then recooled, and the liquid which remained, divided into three equal parts; the first was restored to the crucible and set to boil until the half of the ether which it could furnish had passed into the receiver; the second was heated until it had given forth all its ether; the third was not heated at all.

Each of these portions saturated with carbonate of lead gave a new soluble salt (sulphovinate of lead), but it was the second which furnished the greatest quantity. The residue of the ether may however be turned to advantage when it is not too much carbonized.

To obtain the acid contained in this salt a current of hydro-sulphuric acid was employed. We may also decompose the salt formed by the same acid with barytes by means of sulphuric acid.

The new acid, or the sulphovinous acid, cannot be concentrated on the fire; for the liquid has scarcely begun to boil, when it is already forced to contain a great quantity of sulphuric acid. Placed under an empty receiver, by the side of a jar filled with sulphuric acid, it concentrated, and at last appeared almost as oily as sulphuric acid. It had not yet suffered decomposition, and its density was 1,319. Left for a longer time in the empty reciever it became decomposed; some sulphurous gas was disengaged, and there remained in the vase concentrated sulphuric acid, and some drops of ethereal oil. Sulphovinous acid of the density of 1,319 may remain in cold contact with nitric acid without being decomposed; but heated nitrous vapours are sent forth, and sulphuric acid remains. Similar phænomena occur when the sulphovinates are treated with nitric acid. M. Vogel has not remarked any difference between the acids extracted at different periods before or after the separation of the ether, and he does not adopt the three modifications of M. Sertuerner.

Sulphovinous acid concentrated cannot be long preserved in its state of purity; after the lapse of about fifteen days it begins to affect the salts of barytes.

The sulphovinate of lime, obtained by saturating the residue of the ether with chalk, is in the form of quadrilateral tablets, bisected at right angles; is unalterable when exposed to the air, but when the salt has evaporated in a body, it attracts humidity. The crystals have a taste slightly sweet, and are very soluble in water and alcohol. In the empty receiver by the side of the lime they lose their transparency, as also their water of crystallization. Thrown into a hot crucible they burn with flame, and blacken; but by continuing to heat the crucible, they become white, and are nothing then but sulphate of lime.

The sulphovinate of lime heated slowly in a crucible, bubbles and becomes black; an ethereal empyreumatic liquid passes into the receiver, accompanied with a yellow oil which sinks in water. This oil has something of the *oleum vini;* but it does not possess its lightness. Sulphurous gas is at last expelled, and there remains in the crucible sulphate of lime mixed with a little charcoal.

The sulphovinate of barytes is prepared by saturating the residue of the ether with carbonate of barytes. By a slow evaporation of the liquor crystals very brilliant and possessed of transparency

parency are obtained; they are foursided and unalterable by the air. Although very soluble in water, the sulphovinate of barytes is scarcely so in alcohol. Washed with very concentrated alcohol, in order to see whether the oil adhered mechanically to its surface, it yielded on distillation the same products as if it had not been washed; viz. an ethereal empyreumatic liquid and a heavy yellow oil. From this result we may conclude, that the oil is not mixed mechanically with the salt, but that it is in a true state of combination.

The sulphovinate of lead may be obtained like the two preceding salts, by saturating the residue of ether with carbonate of lead. By evaporation there remains a saline mass, which is so attractive of humidity that in some hours it deliquesces entirely. The salt dried, requires scarcely the half of its weight of water to dissolve it; it is also very soluble in alcohol. Heated in a crucible, it yields a heavy oil, and the residue consists of sulphate of lead mixed with a little charcoal.

The sulphovinate of potash was prepared by neutralizing the carbonate of potash by pure acid. It makes its appearance in pearly scales similar to those of boracic acid; it is greasy to the touch like talc; its taste is sweetish; it dissolves easily in water, and fuses with a mild heat.

The sulphovinate of soda presents itself in brilliant crystals, the form of which is not very regular and which effloresce slightly when exposed to the air.

The sulphovinate of copper is prepared by dissolving carbonate of copper in sulphovinous acid. The solution gives by evaporation blue crystals in large pieces, very soluble in water and in alcohol; the salt passes into the state of sulphate when heated to some degrees above the temperature of boiling water.

Sulphovinous acid dissolves iron with a disengagement of hydrogenous gas. The solution is colourless, of a sweetish taste, and yields no precipitate with salts of barytes. By spontaneous evaporation, foursided prisms are obtained of a yellow-white colour. These crystals effloresce on exposure to the air, and promptly lose their transparency.

The sulphovinates may remain a long time exposed to the air without suffering decomposition. Several solutions of these salts may be even boiled without sulphuric acid being formed; it is only very concentrated solutions which do not support a long ebullition.

After having examined many salts formed by sulphovinous acid, M. Vogel compares this acid with hypo-sulphuric acid.

Both in a liquid state are colourless, very bitter, and cannot be concentrated by ebullition without being changed into sulphuric acid, and into sulphurous acid.

Both can be rendered more dense, and in the same degree, in the vacuum of the pneumatic machine.

Both form soluble salts which have the strongest resemblance.

Both, as well as their salts, are decomposed by nitric acid aided by heat, and give sulphuric acid and sulphates.

The difference between them consists in this, that the sulphovinous acid and its salts contain a volatile oil, which escapes at a high temperature, and is partly decomposed. The hypo-sulphuric acid, on the contrary, is converted by heat into sulphurous acid and sulphuric acid without yielding oil, and the hypo-sulphates do not carbonize at a red heat.

From these facts, it results that sulphuric acid mixed with alcohol is decomposed without the assistance of heat; that it abandons the oxygen, and gives birth to a particular acid. During etherification, the action of sulphuric acid is not confined to determining the formation of water, as Fourcroy and Vauquelin have announced, and the theory of ether ought therefore to be modified. The new acid has the greatest affinity to the hypo-sulphuric acid, and only differs from it in the volatile oil with which it is combined.

On leaving sulphuric acid in contact with birch saw-dust, or oil of lavender, it produces an acid which forms with barytes and the oxide of lead very soluble salts, which analysed are also reduced into sulphates.

As soon as we received an account of the experiments of M. Vogel we hastened to repeat them.

We made accordingly a mixture of equal parts of sulphuric acid and alcohol, and exposed it to heat until sulphurous acid began to manifest itself. The residue saturated with lime and filtered, furnished by evaporation a salt in small flakes; but to obtain more precise results on the nature of sulphovinous acid we transformed this salt into sulphovinate of barytes, decomposing it by a slight excess of that base, and passing afterwards through the filtered solution a current of carbonic acid gas. By a crystallization effected in the course of twelve hours we obtained the salt crystallized in small pearly scales of a square form; but by a spontaneous evaporation, we obtained beautiful four-faced rhomboidal prisms terminated by a pyramid with four faces corresponding to those of the prism. These crystals have a very beautiful appearance, and are not affected by exposure to the air; in the receiver, however, by the side of the concentrated sulphuric acid, they become opaque in the course of twenty-four hours, and abandon a certain quantity of water. One hundred parts of salt simply dried in the air have lost by calcination 45·07; but a hundred parts of the same salt dried in the receiver have only lost 41·5. The

The purest sulphovinate of barytes subjected to the action of heat is very easily decomposed, even by a slight evaporation, to dryness. Its products are an inflammable gas which burns like olefiant gas, sulphurous acid, very little carbonic acid, water, and an ethereal oil, the odour of which mixed with that of sulphurous acid, has much resemblance to that of acetic ether. It is not improbable that this ether might be formed; but we made our experiments on too minute quantities to obtain the certainty of it. There remained in the apparatus sulphate of barytes, blackened by a very small quantity of charcoal, which a short exposure in the air to a red heat sufficed to purify completely.

The analysis of sulphovinate of barytes not having been yet made, and M. Vogel having contented himself with remarking the analogy which exists between sulphovinous acid and hypo-sulphuric acid, it appeared to us interesting to inquire whether the composition of the first of these acids, abstracting the vegetable matter, was the same as that of the second, and in what the manner vegetable matter influences its capacity of saturation.

One hundred parts of sulphovinate of barytes dried in the air lost 45·07, and furnished 54·93 sulphate of barytes very white and very pure. One hundred other parts of the same salt, calcined with a mixture of chlorate and carbonate of potash, and afterwards precipitated by chloruret of barium, produced 111·47 of sulphate of barytes, being nearly double 54·93.

By thus abstracting the vegetable matter the sulphovinous acid appears composed in the same manner as hypo-sulphuric acid, and its capacity of saturation is not changed by the presence of this vegetable matter, which seems to play there the same part as water of crystallization.

Nevertheless the vegetable matter gives to the sulphovinates particular characters. That of barytes has a crystallization different from that of the hypo-sulphate, and it loses 45·07 by the calcination, while the latter loses only 29·9. The other salts formed by sulphovinous acid and hypo-sulphuric acid appear equally different, and merit particular attention. M. Dabit thinks that the vegetable matter was accidental in the sulphovinates; but as it is on the contrary essential to them, the proofs which he has offered to demonstrate the existence of an acid more oxygenated than sulphurous acid, and less so than sulphuric acid, cannot now be regarded; for the decomposition of nitric acid by the sulphovinate of barytes which he had observed, and even the direct absorption of oxygen gas by this salt, which besides has not been confirmed, may be attributed to the vegetable matter itself.

Agreeably to the observations which have been made, and by ourselves in particular, we admit that the greater part of the vegetable

getable and animal matters on which the concentrated sulphuric acid exerts an action at a moderate temperature, and without manifesting sulphurous acid, on being treated by that acid, gave birth to hypo-sulphuric acid combined with a vegetable or animal matter, which appears in general different for each kind of substance. Ought we accordingly to distinguish as many particular acids as there are vegetable or animal matters combined with hypo-sulphuric acid? We think not: but it would be premature to attach at present too much importance to this opinion.

The theory of etherification as it has been given by Fourcroy and Vauquelin can no longer be maintained. Sulphuric acid really yields oxygen to alcohol, and the result of the etherification appears to be ether, hypo-sulphuric acid, and a vegetable matter of an oily nature, which has the greatest analogy to mild spirit of wine. It forms in fact a considerable quantity of hypo-sulphuric acid, relatively to the ether produced, and the spirit of wine does not manifest itself, except at the same time with the sulphurous acid—that is to say, that these two bodies are the result of the decomposition of sulphovinous acid. The alcohol, to change itself into ether, only requires to abandon hydrogen and oxygen in the proportions in which these bodies enter into the composition of water; but since the sulphuric acid really yields oxygen, it should separate from the charcoal, and it is in the spirit of wine we ought to find it again.

It is very probable, according to these new facts, that in the bitter of Welter, and other analogous compounds, the acid is in the state of nitrous acid.

The investigations which we have made are still too imperfect to be much insisted on, and we should even have deferred making them known had it not been for the opportunity furnished us by those of MM. Dabit, Sertuerner and Vogel.

LXI. *Account of the Origin of the Art of manufacturing Tin-Plate. By* SAMUEL PARKES, *F.L.S.**

FORMERLY none of the English workers in iron or tin had any knowledge whatever of the methods by which this useful article could be produced; our ancestors, from time immemorial, having supplied themselves with it from Bohemia and Saxony. The establishment of this manufacture in those districts was doubt-

* From A descriptive Account of the several Processes which are usually pursued in the Manufacture of the Article known in Commerce by the Name of Tin-Plate, inserted in Vol. III. of the Memoirs of the Literary and Philosophical Society of Manchester.

less

less owing to their vicinity to the tin mines in the circle of Ersgebirg, which, next to those of Cornwall, are the largest in Europe. The ore which is found there is not the *tin pyrites*, but the mineral called *tin-stone;* and it is curious that it should occur in abundance both on the Bohemian and Saxon sides of the mountain group: accordingly, manufactories of tinned iron have been established in both those kingdoms. Alluvial deposits of grain-tin are also found in the same vicinity.

From the time of the invention of tin-plate to the end of the seventeenth century, not only England, but also the whole of Europe depended upon the manufactures of Bohemia and Saxony for their supply. However, about the year 1665 one Andrew Yarranton, encouraged by some persons of property, undertook to go over to Saxony to acquire a knowledge of the art—and on his return, several parcels of tin-plate were made of a superior quality to those which we had been accustomed to import from Saxony; but owing to some unfortunate and unforeseen circumstances, which are all detailed by Mr. Yarranton *, the manufactory was not at that time established in any part of Great Britain.

As it is now difficult to procure a copy of the work from which I have obtained a knowledge of the manner in which this manufactory was brought into England, an abridgement of the author's own account of the transaction may perhaps be interesting to some of the members of the Society.

"Knowing," says Mr. Yarranton, "the usefulness of tin-plates, and the goodness of our metals for that purpose, I did (about sixteen years since †) endeavour to find out the way for making thereof; whereupon, I acquainted a person of much riches, and one that was very understanding in the iron manufacture; who was pleased to say, that he had often designed to get the trade into England, but never could find out the way. Upon which it was agreed, that a sum of monies should be advanced, by several persons, for the defraying my charge of travelling to the place where these plates were made; and from thence to bring away the art of making them. Upon which, an able fireman, that well understood the whole nature of iron, was made choice of to accompany me; and being fitted with an engenious interpreter, that well understood the language, and that had dealt much in that commodity, we marched first for Hamburg,

* "England's Improvement by Sea and Land," with many plates of Plans, Charts, &c. in two parts, by Andrew Yarranton, Gent. Part I. Quarto. London, 1677. Part II. London, 1681.

† This account is dated February 2, 1681; I therefore conclude that Mr. Yarranton's journey to Saxony must have been about the year 1665.

then

then to Lipswick; and from thence to Draisden, the duke of Saxomes Court, where we had notice of the place where the plates were made; which was in a large tract of mountainous land, running from a place called Seger-Hutton, unto a town called Awe, being in length about twenty miles; the tin-works being there fixed upon a great river running clear along the valley, and also upon some little rivulets that run out of the mountains of Bohemia and Saxony; and coming to the works, we were very civilly treated, and, contrary to our expectation, we had much liberty to view, and see the works go—with the way and manner of their working and extending the plates, as also the perfect view of such materials as they used in clearing the plates, to make them fit to take tin, with the way they use in tinning them over, when cleared from their rust and blackness. And having (as we judged) sufficiently obtained the whole art of making and tinning the plates, we then came for England, where the several persons concerned in the affair thought fit to make some trial in making some small quantities of plates and tinning them, which was done; and all workmen that wrought upon them agreeing that the plates were much better than those which were made in Germany; upon which, preparation was making to set this beneficial thing at work;—but, it being understood at London, A PATENT was trumpt up, and the patentee was countenanced by some persons of quality—and what with the patent being in our way, and the richest of our partners being afraid to offend great men in power, who had their eye upon us, it caused the thing to cool, and the making thereof was neither proceeded in by us, nor possibly could be by him that had the patent; because neither he that hath the patent, nor those that have countenanced him, can make one plate fit for use*."

This enterprising individual, who spent the greater part of his life in promoting schemes for the good of his country, and who, in the opinion of Bishop Watson, ought to have had a statue erected to his memory †, proceeds to inform us, that before they

* England's Improvement, &c. page 149-152, Part II.

† The following particulars which I have collected respecting Mr. Yarranton, will tend to justify the good Bishop in this opinion. He was bound as an apprentice, early in life, to a linen draper, but after some years he left that situation in disgust. In the year 1652 he took some iron works, which he carried on for several years; and during this period, he made regular surveys of the three great rivers in England, and by means of associations which were formed by himself, he rendered three other rivers navigable—he studied agriculture with such effect, that many of the arable estates in the midland counties were rendered doubly productive by the new methods of husbandry, which he either brought from abroad, or discovered;—he laid a plan for the junction of the Thames and Severn *at that spot* where of late years

they were stopped by the patent, they had made "many thousand plates from iron raised in the forest of Dean, and tinned them over with Cornish tin, and the plates proved far better than the German plates, by reason of the toughness and flexibleness of our forest iron. One Mr. Dison, says he, a tinman in Worcester, one Mr. Lydiate near Fleet-bridge, and one Mr. Harrison near the Kingsbench, have wrought many, and they know their goodness *."

In another place this interesting writer informs us that "when he was in Saxony, the different establishments for making tin-plates were very numerous, and that most of them belonged to the duke †." "The trade," says he, "is so great, that by computation, no less than 80,000 men depend upon it, and when the plates are finished, they are sent by land to Lipsick, from thence to the Elbe river, and so down to Hamburg, and from thence sent by sea as far as trade is known ‡." "There was," says he, "no tin any where in Europe, except in Cornwall, until a Cornish man found tin in the mountains of Saxony, near a town called Awe, where his statue is yet to be seen. The tin works are fixed upon a great river running down the valley—and the tin, iron and woods, grow in and upon the mountains adjoining to both sides the river; and those tin-works have proved so beneficial to the place, that there are several fine cities raised by the riches proceeding therefrom §." He adds, "the trade of making tin-plates was about sixty years since fixt in Bohemia, and had there long continued; but the woods decaying, and there being at that time a wise duke of Saxony, willing and ready to improve his own revenue, and his subjects, did accept of directions how this trade might be brought away and fixt in the duke of Saxony's territories ||. A Romish priest, converted to be a Lutheran, was the chief in-

years this very scheme has been effected;—he proposed the cutting of several navigable canals, half a century before any such project had been executed in this country. He made the necessary surveys and planned docks for the cities of London and Dublin;—besides his journey to Saxony already mentioned, he went to Holland, under the patronage of the ancestors of some of our present nobility, to examine the inland navigations of the Dutch, and to investigate the nature of their linen manufactures;—and on his return promulgated the plan for a new manufacture of linens, which he calculated would employ all the poor of England. He published schemes for the improvement of our national fisheries; he made several tours through Ireland, for the express purpose of planting new manufactures and devising the increase of the staple trades of that country. He made a regular survey and estimate of the expense of rendering the river Slane in Ireland navigable, for the purpose of bringing timber down to the coast for His Majesty's navy; and rendered many other signal services to his country.

* Page 173. † Page 155. ‡ Page 172.

§ Yarranton, part II. page 176. || Ibid. page 178.

 strument

strument in the whole affair, until it was was perfected—and a Cornish miner, a protestant, who had been banished out of England for his religion, found out the tin in Saxony—both which persons proved instruments of great wealth to that duke and country*."

Notwithstanding Mr. Yarranton had so completely introduced the knowledge of making tin-plate into this country, I do not find that any manufacture of that article was established in these kingdoms until some time between the years 1720 and 1730, which must have been long after Mr. Yarranton's death. The first establishment of this kind was, I believe, fixed in Monmouthshire, where it continued to flourish many years †.

About the time that this manufactory was established, the amiable and intelligent M. Reaumur, to whom the French are indebted for a new mode of graduating the thermometer, and for many discoveries and improvements in the arts ‡,—undertook to discover the method of making tin-plates for the French people. This eminent man, whose mind was cast in a mould very similar to that of Mr. Yarranton, but who possessed more science, never relinquished any thing which he undertook; and accordingly, notwithstanding the innumerable difficulties which he had to encounter, at length succeeded in acquiring such a knowledge of the principles of the manufacture, as enabled him to instruct several people in the vicinity of Paris, in an art which, until then, had never been practised in that country.

Soon after the time of which I am speaking, several similar manufactories were erected in Great Britain; and now the establishments of this nature are so numerous and extensive in many parts of these kingdoms, that the manufacture of tin-plate is become of great national importance, and more than one hundred thousand boxes of these plates are annually exported.

January 12, 1818.

* Saxony is only separated from Bohemia by a chain of mountains called the *Erzegeberg*; which in German signifies hills that contain mines.

† Upon further inquiry, I find that this was at the town of Ponty Pool; and it is remarkable, that after the lapse of nearly 100 years the manufacture has recently been re-established at the same town, on a very extensive scale.

‡ It was Mons. Reaumur who was the means of introducing into France the methods of making porcelain; for when Francis d'Entrocolles, who had resided many years in China as a Christian Missionary, sent to France specimens of the materials used by the Chinese in their porcelain, M. Reaumur immediately instituted a series of experiments to discover the method of imitating their productions, and in the years 1727 and 1729 communicated the result of his researches to the Academy of Sciences; and his two memoirs were published by the Society in their Transactions.

LXII. *Account of a Visit to the Crater of the Volcano Goenong Apie, one of the Islands of Banda. By Captain* Verheul*.

In the year 1817 (says M. V.), being in the Archipelago of Banda, with His Majesty's ship Admiral Evertsen, of which I had the command, commissioned by the Government to receive from the English possession of those islands so celebrated for their principal production, the nutmeg; I formed the design of visiting the Goenong Apie, or volcano, situated in one of these islands, and of ascending to the summit as nearly as possible. Being fond of drawing, I promised myself a rich harvest for my collection of Indian sketches and drawings.

The little island of Goenong Apie rises in the form of a cone above the surface of the ocean; two-thirds are covered with cocoa and other trees, the rest is arid, and bears the traces of lava in different directions, which lead to the flattened summit. The island does not produce any spices; but some of the inhabitants of Banda have made in the lower part some gardens, the most considerable of which belongs to Mr. Vetter, officer of the royal marine in the colonies, and commandant of the port of Banda. Formerly there were also some forts to protect the western outlet, but none now exist.

The 3d of April was the day I fixed for the execution of this arduous expedition. I preferred the night in order to avoid the excessive heat of the sun; and the more so, as the full moon favoured our enterprise.

Mr. Vetter offered some of his slaves to serve us as guides, and several officers of the ship Evertsen joined our party. An hour after midnight we went on board a boat, and proceeded to the country-house of Mr. Vetter, whose dwelling with several other habitations had been almost entirely demolished by the earthquake in October, 1816. There we put aside our superfluous clothes, and carrying bamboo canes, we proceeded to the execution of our project. Several negroes of our company carried the necessary refreshments, and the flags of the Netherlands, which we proposed to place on the edge of the crater. The night was magnificent: soon every thing around us was desert, and very often we were enveloped in darkness by the thick foliage of the trees, shrubs, and other plants, so that we were obliged to feel with our canes, and to climb from rock to rock, in order to find a practicable path. One of the Indians who preceded us, armed with a kind of axe called *klenang*, with which he cut away the branches that impeded our progress, pointed out to us the clefts in the rocks, produced as it seems by earthquakes. We were often obliged to go a long way about in order to avoid the crevices,

* New Monthly Magazine for May 1820.

vices, which were frequently very broad and deep, holding, as we proceeded along their black edges, by the brambles and ferns which grow there in an abundance, and to an extraordinary height.

The most varied scenes, sometimes agreeable, and sometimes terrible, opened themselves to our view whenever a beam of the moon penetrated the darkness which surrounded us. Here trees with their branches interlaced, there ancient trunks overthrown or rooted up, everywhere enormous steep rocks, some wholly barren, some covered with a kind of verdure, the damp clefts in which served as retreats for monstrous serpents, the sight of which terrified us whenever we discovered them by their hissing. We found here numerous majestic cocoa trees loaded with fruit. By degrees we advanced, and as the thickness of the trees and the verdure of them, as well as the rocks, visibly diminished, we enjoyed more and more the lustre of the queen of night, and we had reason to hope that we should soon be at the end of our pilgrimage on the barren part of the mountain. It was about half-past four in the morning when we at last arrived; and the scene which offered itself to our view amply repaid our fatigues. The whole cone appeared from the bottom like an extremely smooth mass formed of heaps of lava; the summit was enveloped in sulphureous clouds issuing from the crater, gently agitated by the night breeze, and their edges silvered by the beams of the moon. From time to time flashes or quick eruptions of fire issued from the interior of the volcano. A mournful silence prevailed around us, the ocean was blended with the clouds, and the islands seemed to float beneath our feet in the dim perspective.

After having reposed some moments in this place, we undertook the dangerous and fatiguing task of ascending a slope of 50°, and climbing over a mass of moving stones. As in coming loose they drew others along with them, we were obliged to keep in a line abreast of each other, in order to avoid mutual injury when we were thrown down by these stones. It often happened that when we leaned upon the largest masses, they loosened, and lamed our hands and feet. At pretty short intervals we had opportunities of resting upon ferns growing between the rocks; then we took breath, and refreshed ourselves with a beverage which we had carried with us.

Suddenly the horizon cleared up, and we perceived the extremity of the ocean, and the clouds floating around us like a gilded zone; it was the approach of dawn. At the same time the morning-gun was heard from the Evertsen, and the noise repeated from one island to another insensibly died away, like that of thunder upon the vast surface of the ocean. The dawning light showed us the verdure of the trees below our feet like an even

even carpet, varied only by different tints of colour of the roofs of the houses and the forts Neira and Lonthoer. The sun now rose in all its splendour: he issued majestically from the bosom of the ocean, and his brilliant rays were reflected by the waves of the calmest sea. We seemed to float with the steep Pico on which we stood, having above our heads the serenest sky. No expression is adequate to paint so astonishing a picture.

Meantime the sulphuric vapour which issued from the crater began to incommode us, and our situation appeared the more critical, as we found the stones at every successive step more and more loose, and the noise they made in falling was augmented by that which was heard in the interior of the crater. Thick sulphuric exhalations issued with violence from the clefts of the mountain, in the sides of which we heard a confused rumbling noise, resembling the roaring of the sea agitated by a tempest.

We at last arrived at the upper edge of the crater, with our shoes and clothes half burnt, and our hands wounded by the sharp lava. The appearance of the interior of the crater, which is in the form of a funnel, is singularly striking; the whole surface is covered with a lava of the most beautiful yellow colour imaginable; the smoke issued from a number of channels or tubes of sulphur, frequently accompanied by a hollow sound. The crater itself may be calculated at about 200 feet in diameter, and the bottom is divided into two parts. On the north side the bottom is not visible, the edges are steep, so that their greatest thickness does not exceed four feet. On the south side the bottom is seen covered with an immense quantity of rocks, which appear to have been thrown into it by the last eruption, the traces of which are visible in a deep furrow of lava across the whole mountain.

We went on as far as the top of a peak which rises on the northern side; it was there that Lieutenant de Tong and myself placed the standard of the Netherlands. We found there also a cassowary which had run away from the farm of Mr. Vetter, situated at the foot of the mountain. It seems that this bird was suffocated by the sulphureous atmosphere.

The wind began to blow from the south, and as it drove the thick sulphuric clouds from the entrance of the crater, we had a mind to examine more nearly a part of the interior. For this purpose we kept our handkerchiefs tied before our noses and mouths, to preserve them from the azotic gas, and descended into the volcano. The sulphur upon which we walked broke and crackled like frozen snow, and the heat of the ground obliged us to be always in motion. Sometimes a hollow sound was heard under our feet: the smoke, continually effervescent, issued from the veins of crystallized sulphur, and, escaping with violence, evaporated

rated in the air. The rays of the sun falling on the crust of sulphur, mixed with a very brilliant saltpetre, produced a magical effect; but when we arrived at the edge of the second region of the bottom of the crater we perceived a thick and boiling vapour.

This vapour not permitting us to advance, or remain any longer where we were, we turned back, and went out of the crater loaded with beautiful pieces of lava and crystallized sulphur. We were the more eager to get out, as we feared being suffocated by a change of the wind. Our Indians were still more afraid to remain than we were; and it must be confessed that our situation was imminently dangerous, since a few months after we had visited this place the south part of the crater, into which we had entered, sunk in, with all the rocks which it contained.

When we had got out of this abyss, we rested for half an hour, and then proceeded to the most difficult part of our expedition —this was to descend. One of our companions was so terrified at the sight of a declivity of four thousand feet, and of a slippery and almost perpendicular road which we had to pass, that his fear checking his respiration gave us a great deal of trouble; but with the assistance of the Indians he safely reached the forests of the inferior region.

I found that the best way to descend this cone covered with lava, was to keep to the places where the ashes of the lava are the finest, leaning upon a bamboo cane, and to let myself glide down till I was up to my knees in the ashes, then to extricate myself and begin the operation again. It was in this manner, which is, however, very fatiguing and painful to the feet, that I succeeded in reaching the end of the barren part of the mountain. In such cases great care must be taken not to fall forwards, as such a fall might be fatal.

We were all extremely fatigued and thirsty, our stock of liquor being exhausted; and proceeding with difficulty from one tree to another, we arrived, at length, weary and overcome, at a little Indian hut, where we lay down upon the grass. One of our party was unable to utter a single word: however, the cocoa-pine and a piece of water-melon, which each of us took, soon restored us, and at noon we regained the valley of Neira, with our clothes in tatters, almost without shoes, and bruised and scratched all over. During all the time of our expedition our friends below had observed us with telescopes.

The standard which we had placed on the upper rock was seen for a long time floating in the air; but at last it disappeared, being consumed by the sulphureous vapour.

LXIII. *Ac-*

LXIII. *Account of the Fall of the Glacier of the Weisshorn, which happened on the 27th of December* 1819, *and the Destruction of the Village of Randa, in the Valley of Vispach.*

THE village of Randa is situated about six leagues above Vispach, on the south or right branch of the valley of Vispach, commonly known under the name of the Valley of St. Nicolas. The village is about 2400 feet from the right bank of the Visp, on the steep declivity of a hill composed of fragments, the stony ground of which has been converted by the industry of the inhabitants of Randa into pastures. Opposite to this hill is another of the same nature, above which are the rocks covered by the Glacier of Randa; the highest summit of which, called the Weisshorn, is elevated about 9000 feet above the village. The breadth of the valley at the height of the village (nearly 250 feet above the river) is about half a league.

On the 27th of December, 1819, about six o'clock in the morning, towards the eastern and very steep side of the highest summit of the Weisshorn, a part of the glacier became loose, fell with a noise like thunder on the mass of ice below, and announced, by the most dreadful crash, the ravages with which the valley was threatened. At the moment when the snow and ice struck on the lower mass of the glacier, the clergyman of the place and some other persons observed a strong light*, which however immediately vanished, and every thing was again enveloped in the darkest night. A frightful hurricane, occasioned by the pressure of the air, instantly succeeded, and in a moment spread the most tremendous devastation. The fall of the glacier itself did not hurt the village, but the hurricane which it occasioned was so powerful that it threw mill-stones several toises up the mountain, tore up by the roots distant larch trees of the largest size; threw blocks of ice of four cubic feet over the village a distance of half a league; it tore off the top of the stone belfry, levelled several houses with the ground, and carried the timbers of others more than a quarter of a league beyond the village into the forest. Eight goats were whirled from a stable to a distance exceeding 100 toises; and it is remarkable that one of them was found alive. More than a quarter of a league above the valley, the barns opposite the glacier are seen stripped of their roofs.

* It is very desirable to obtain a satisfactory information of this phenomenon, which, as far as we know, has not yet been observed in similar cases; and which, in the darkness of the night, was much too conspicuous to leave any doubt of its reality.

On

On the whole, nine houses in the village are totally destroyed, and the other thirteen more or less damaged; eighteen granaries, eight small dwellings, two mills, and seventy-two barns are destroyed, or irreparably injured. Of twelve persons who were buried in this catastrophe, ten are still living; one was taken out dead, and the twelfth has not yet been found.

The avalanche, formed of a mixture of snow, ice, and stones, covers the fields and the pasturages situated below the village for the length of at least 2400 feet, and extends in breadth about 1000 feet. The mass which has fallen measures on an average 150 feet in height. The damage is estimated at about 20,000 francs.

It is remarkable that some barns on the other side below the glacier which were almost covered with the fragments, were thereby protected from the hurricane, and escaped uninjured; but what is much more extraordinary, is, that only two persons lost their lives, though some families were carried away with their houses, and buried under the ruins and drifted snow. The prompt assistance afforded by the clergyman, who did not suffer personally, and of the two sextons, who escaped, contributed to save several persons.

It is not the first time that such a disaster has befallen the village of Randa. In 1636 it was destroyed by a similar avalanche, when 36 persons lost their lives. It is said that at the time the whole glacier of the Weisshorn had fallen down. Two other less considerable falls happened in 1736 and 1786, but not precisely in the same place.

This time only a small part of the glacier fell down, and it is difficult to conceive how the rest, deprived of its support, can maintain its position. With a good telescope enormous clefts are discovered in it, which were long since perceived with much dismay by some chamois hunters; and the part of the glacier which has fallen was, as it has been affirmed, separated from the mass by similar clefts. It is therefore much to be feared that the glacier cannot much longer support itself on the very steep summit, and that the remains of the village of Randa are destined to destruction by the inevitable fall of the impending glacier. The unfortunate inhabitants must, therefore, abandon the place; and, not to remove them too far from their meadows, it is hoped that a village will be erected about half a league further up towards Tesch. But this measure will require the assistance of the government and other communes of the canton.

LXIV. *Me-*

LXIV. *Memoir upon Zircon.* By M. Chevreul *.

The zircon on which the author made his experiments was brought from the island of Ceylon.

Hydrochloric acid, mixed with nitric acid, separated from it a great deal of peroxide of iron and a trace of oxide of titanium; but the latter is not essential to the composition of zircon.

(*a*) A portion of zircon, which had been previously treated with nitro-muriatic acid, was plunged in double its weight of a solution of potash in alcohol, with which it was exposed to a cherry red temperature in a silver crucible; the water separated from the heated mass a considerable quantity of potash, retaining traces of silex and of zircon.

(*b*) The matter undissolved by the water was a compound of silex, of zircon, and of potash, which may be considered as a sort of double salt; this compound has the following properties.

(*c*) It is of the most beautiful white; it remains a very long time in suspension in distilled water. It precipitates itself on the contrary very promptly from a solution of potash, which proves that pure water has an action upon it which alkalized water has not: it is not impossible that this depends upon an attraction which the pure water exerts upon the potash which it (the compound) contains: in this view of the matter, it may be easily conceived why the water which is already united to this base has no longer any action on the compound.

(*d*) It is wholly soluble in weak hydrochloric acid; by evaporating, the silex is precipitated, and there remains in the liquor chloruret of potash, and hydrochlorate of zircon containing a little hydrochloruret of iron: ammonia precipitates these two bases.

(*e*) The following is the process which M. Chevreul has followed to obtain the zircon separate from iron, a result which no person before him has been able to accomplish. He fused the zircon containing iron with potash in a silver crucible: he exhausted the mass of all the matter soluble in water which it contained. There remained a zirconate of potash, blended with oxides of iron, of copper, and of silver (the two latter proceeding from the crucible). He poured upon this zirconate concentrated hydrochloric acid; there was an immediate disengagement of heat, of steam, and of hydrochloric gas. The substance, now in the state of a soft paste, was put into a glass cylinder of an inch in diameter and five inches in length; concentrated hydrochloric acid was added to it, until that acid detached from the matter contained in the cylinder nothing but hydrochlorate of zircon and chloruret of potash. This was ascertained—1st, by the washing mixed in water not precipitating chloruret of silver;

* From *Bulletin de la Société Philomatique.*

2d, by its not being coloured by hydrosulphuric acid; 3d, by the hydrosulphate of ammonia producing with it a perfectly white precipitate. M. Chevreul took the mass which had been washed with the hydrochloric acid; he diluted it in water, filtered the mixture, and precipitated the pure zircon by ammonia. The hydrate which he obtained he calcined in a glass capsule. This process, it will be observed, rests principally on this circumstance; that a quantity of concentrated hydrochloric acid insufficient to dissolve a certain quantity of hydrochlorate of zircon, suffices, on the contrary, to dissolve the hydrochlorates of iron and copper intermixed with the latter.

M. Chevreul afterwards subjected the zircon and the peroxide of titanium to a comparative examination.

The hydrated zircon dried in the air is soluble in hydrochloric acid. This combination crystallizes in small satin-like needles of the purest white. The excess of acid may be expelled from the hydrochlorate by evaporating to dryness: on rewatering the residue, but very little zircon is separated, especially if the solution which was evaporated was concentrated: on again treating it with hydrochloric acid, the process ends by redissolving it entirely, if the evaporation has not been pushed too far.

The hydrochlorate of titanium takes a yellow colour when a concentrated solution of it is evaporated to dryness: a greater quantity of it is decomposed than is the case with hydrochlorate of zircon when evaporated; and when the acid is added to the residue, it has not the effect of redissolving it. But what most distinguishes this hydrochlorate from the preceding is, that on joining three parts of water to one part of a solution of each hydrochlorate, and subjecting the two liquors to the action of heat, that of titanium allows a great deal of oxide or subhydrochlorate to precipitate even before ebullition; while that of zircon may be evaporated to dryness without yielding any deposit.

The hydrochlorate of zircon steeped in water is not decomposed, even after a lapse of many months; that of titanium similarly treated becomes milky, though the decomposition does not take place at the very moment when the water is added.

The hydrochlorate of zircon is precipitated of a dun yellow colour by nut galls: if the solution is concentrated, the gelatinous precipitate retains all the liquor in its particles. The hydrochlorate of titanium presents, it is known, this latter phænomenon; but the precipitate is of a very lively orange red.

The hydrochlorate of zircon is precipitated of a canary yellow by an excess of prussiate of potash; that of titanium, on the contrary, is precipitated of a brownish red. M. Chevreul has observed that the two precipitates were soluble in an excess of prussiate of potash; that in certain circumstances the prussiate of zircon was almost colourless; and that it became yellow by

an

an excess of prussiate, although the latter did not, however, cause any precipitate in the liquor which had given the white precipitate. The yellow colour of prussiate of zircon explains how Klaproth imagined that he had discovered nickel in zircon, because he obtained a green precipitate on mixing with the prussiate of potash a solution of zircon which contained a little iron.

The hydrochlorate of zircon does not become violet when a little zinc is added, which is the case with hydrochlorate of titanium.

The two hydrochlorates have an excessively astringent taste, and both precipitate gelatine; which proves that they have much more affinity for animal matters than the salts of yttria, of glucine, and of alumine, the taste of which is sweet, and only slightly astringent.

The two hydrochlorates are completely decomposed by a red heat; they lose their acid, and their base remains in a state of purity; the zircon is perfectly white; the peroxide of titanium is of a yellowish gray.

Lastly: The hydrates of titanium and zircon heated in a small glass capsule above the flame of alcohol blacken, and then become incandescent as if they experienced a combustion. The zircon is demi-vitrified, and of the purest white when exempt from iron; when it contains iron, it is of a greenish hue. The oxide of titanium is of a gray yellow.

M. Chevreul intends to publish in a second memoir the proportion of the elements of silicate and zirconate of potash. He will determine the composition of several salts of zircon, and will investigate, whether the colour of prussiate of zircon may not be owing to some foreign substances, perhaps to traces of peroxide of titanium.

LXV. *Free Remarks on Mr.* GREENOUGH'S Geological Map, *lately published under the Direction of* the Geological Society *of London. By Mr.* JOHN FAREY *Senior, Mineral Surveyor.*

LETTER I.

To Mr. Tilloch.

SIR, — AMONGST the Readers on Geological Subjects, within the last five Years, very high expectations have been raised, by several Writers (by two in particular*), as to *the vast superiority* which a Geological Map of England and Wales, which Mr.

* Mr. Wm. Phillips, in July 1816, in the Appendix to the 1st Edit. of his "Outlines of the Geology of England," p. 29, and in his 2d Edition of the same date; and Dr. Fitton (anonymously) in Feb. 1818, in the "Edinburgh Review," Vol. xix. p. 336, see P. M. Vol. 52, p. 184, note.

Greenough had in preparation, would possess*, over the original Geological Map of these Kingdoms, published by Mr. William Smith, in September 1815 (price 5 *guineas*, with its Memoir of 52 pages); and the same, and even higher ideas have been raised, by the verbal Representations of many of the Members of the Geological Society and their Friends, who have (like the 1st of the Writers alluded to) on very frequent occasions spoken of this intended Map, as "the *Geological Society's* Map"; and these expectations have not a little been heightened, by the perusal of the late Annual Report on the affairs of the Geological Society; wherein it is stated (see p. 228 of this Volume), that certain Individuals (who are *nameless*) have *contributed*, and had advanced large Sums of Money, and engaged to pay still further Sums, amounting in the whole to *seventeen hundred Pounds*, for the preparing and bringing out of Mr. Greenough's Map and its Memoir, "under the direction of *the Geological Society*"!

I am on three accounts happy in being able now to announce, to such of your Readers whose interest may have been excited, as above, that this Map and its Memoir, are now before the Public, having been published by Longman and Co. (at 6 *guineas*) on the 5th of May.

Firstly, because I find in the Map, many new divisions of Strata and minuter details of others, than have yet been given by Mr. Smith, regarding much of *the western half* of our Island.

Secondly, because (except in a few instances which I intend to speak of hereafter) the intention has not been acted on, which was avowed to me by Mr. G. on the 18th of Nov. 1811, of giving to this Map an entire *geognostic Character*†, that is, as his Friend and the herald of his fame, Dr. F., so lately as February 1818, intimated (in the Edinburgh Review, Vol. xix. p. 338) in so limiting *the formations* depicted by colours on the Map, that instead of the "barbarous Names" used by Mr. Smith, the Geognostic, or "the proper *scientific names of the* SUBSTANCES *composing the Strata*," could be used by Mr. G.; such names, *to be settled by a Committee of the Geological Society*; instead however

* Eight years ago (June 1812) I recorded my anticipation of these improvements, in p. 426 of your 39th Volume; formed while *relying on the word* and faith, of the *Member* for Gatton, the *Lieutenant* of East India Volunteers, the *President* of the Geo. Soc. &c. &c. that *no publication*, or *announcement of such an intention*, should take place, of the Map then preparing, which *I had* in its early stages been principally instrumental in furnishing, with a large portion of Mr. Smith's unpublished materials; particularly on the eastern half of our Island; being solemnly assured by Mr. G. at various times, between the 25th of March and a few days before the 18th of Nov. 1811, that this Map should be consulted, only by the Members of the Geological Society, resorting to him, or to its House, or as a specimen of what Mr. Smith then had, in very great part, *ready to publish*.

† See P. M. Vol. 39, p. 425, Vol. 45, p. 337, &c.

ever of attempting to follow this course, Mr. G. has, on his Map, *named* each of the 34 *Groups of Strata* that are separately depicted thereon by colours, after some *Place* or *District*: these Places or Districts, being those (as he says) "where each Group may be best studied†."

And *thirdly*, and chiefly I feel happy, because now, further, delusive and exaggerated promises, of the *high degree of excellency* to which this Map was to attain, made by Mr. G. his Contributors, and G. S. Friends, must be at an end; *it is now before the Public*, and many besides myself will carefully compare it with Mr. Smith's 15 Sheet, and his one Sheet Maps, and more especially so, with the 12 of his *County Geological Maps*, which are on Sale, separately: and I hope and trust, for the character of the Country and Age in which I live, as well as for the sake of my much-injured Friend Mr. Smith, that the result will be, such a future sale and spread of his Maps and Works, as shall at least re-instate the Fortune, of this ingenious and deserving Individual.

As to *Sections*, properly so called, exhibiting a supposed vertical division of the Earth, along some particular Line (a Road for instance, which is defined, and shown on the Map), for showing *the edges* of the Groups of Strata, and the manner and form of their overlieing each other, no such thing will be found, either in Mr. Greenough's Memoir, or on his Map; which is the more surprising, because Mr. Wm. Phillips, who has been supposed to be well versed in the G. S. secrets, in p. 29 of the Appendix to his "Outlines," 1st Edit., gave reasons to expect, that the "great Map" of the "Geological Society," would when published, be accompanied by numerous *Sections*, and by *an*

† I believe myself to have been the Writer who, in June 1814, first recommended this principle, of *naming* Assemblages (or *Groups*) *of Strata*, in page x. of the Preface to my Derbyshire Report, Vol. I. Mr. Smith, I believe, in a partial manner adopted the same principle, in the earliest stages of his investigations, although in the first account of the "Order of the Strata", which he privately circulated in 1799, (two years before I had heard or read of Mr. S. which first occurred in the spring of 1801,) and which account he published in September 1815, in p. 8 of the Memoir accompanying his Map, not one of his 23 Strata were so named:—while he was giving me Instructions, in the Autumn of 1801 and Spring of 1802, Mr. S. usually spoke of, and described a large portion of his mapped Strata, by the *Names of Places* upon them, and I have accordingly, in my first short account of his Order of the Strata, (p. 111 to 114 of Derb. Rep. V. I.) thus named 10 of his Strata: yet before the publication of his Map, Mr. S. appears to have so far abandoned this good principle of naming, as to have retained it, only for three of his Strata, viz. those of London, Purbeck, and Derbyshire:—Mr. Greenough has however adopted it throughout (although not with the best selection of Places in some instances) and for so doing has my thanks:—for me to have expected that this Gentleman should have acknowledged the sources of this suggestion, might perhaps be deemed unreasonable, by many of the M. G. S.

ample

ample Memoir, illustrative of the whole; instead of which, exclusive of 11 pages of *Names of Hills,* that are only indicated by Letters *in the Map,* and which therefore ought to have been made capable of *pasting on to the margins* of the Map (by being printed only *on one side,* instead of *both sides of the paper*) because, without this addition to its Margins, the Map is incomplete and in a great degree useless, only 5 pages and 6 lines are devoted, to the description of the Map, or the other proper subjects of an accompanying Memoir.

An awkward and lame sort of an Apology for the conduct pursued towards Mr. Smith (of which more anon) and acknowledgements to some Individuals who have assisted the Author, occupy full two of these pages; leaving only three pages to the historical and the descriptive *account of the Map,* and of the principles on which it has been constructed; those who may not have perused the Author's late Work on Geology, should not fall into the mistake of supposing, that the paucity and absolute defectiveness of this Memoir, will be found supplied by the separate Work alluded to, because, in neither of them, is the least mention or allusion made to the other; indeed, how could this have been done? because, the "Critical examination of the *first principles* of Geology," criticises to an excess, so as to unhinge it, every principle (which the Author notices, and as far as he has been able) on which Mr. Smith proceeded, in commencing his Map, 27 or 28 years ago, and on which he prosecuted the same until, at the very beginning of this Century, it was in a very *sufficient state to have been published,* as scores besides myself are still living to testify, and to identify the 15-sheet Map (still in existence) which Mr. Smith exhibited, to those whose *Names* (but not their Monies) he solicited as Subscribers, to a reduction of this Map, and a Work describing it. I say *sufficient state* of perfection, to have *at that time* justified its publication, had more adequate pecuniary encouragement been given (of which, more anon); because, to pretend that so great a Work as exhibiting the actual extent of Surface, the number and the order of super-position, of all the many Groups of Strata, included in the space of the 15-sheet Map, could at that period, or even in 1815, have been *quite correctly accomplished,* or that these important objects are, even *now accomplished,* by this Map of Mr. Greenough's, would argue, either a very shallow acquaintance with the subject, in its *practical* details, or else, little regard to truth in the Person advancing such pretensions.

High encomiums are, at the end of the Memoir, passed upon Mr. Thomas Webster, for his supposed matchless care and accuracy, in drawing the Map and preparing it for publication; it appears however, that great deductions are due from this representation; particularly in what relates to that highly essential matter,

matter, *the colouring* of the Map; in very numerous instances, the *Lines engraven*, as directions to the Persons who colour, do not *correspond at all with the limits of the Colours!*; through which, and also through the want of an *engraved Number*, corresponding with the number belonging to the *Group*, in each patch of Colour, in the Map, and through *different copies* of the Map, *differing in their colouring*, &c. &c. very great uncertainties must ever attend, the consulting and depending on this highly boasted Map. I am

Your obedient servant,

John Farey Sen.

Howland-street, Fitzroy-square,
May 15, 1820.

LXVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 20.—Two papers were read: one on an improvement in the Eye Tables of Achromatic Telescopes of a small size, by Mr. Kitchener; and the other on the different qualities of the Alburnum of Spring and Winter-felled Oak Trees, by Thomas Andrew Knight, Esq.

27. Read, a paper by Samuel Ware, Esq. on the Properties of Domes and their Abutment Walls.

May 4. Read two papers: one on Diarrhœa Asthenica, by —— Hood, Assistant-Surgeon; the other on the Formation of the Canal for containing the Spinal Marrow; and of the Fins of the Proteosaurus, by Sir Everard Home.

11. An account was read of some experiments made by Mr. F. Bauer on the colouring matter of the Red Snow found in Baffin's Bay.

18. Read a communication from Sir Thomas S. Raffles, containing some account of the Dugong.

Early in this month the venerable and much respected President of the Society (Sir Joseph Banks) intimated to the Council, that from the increasing infirmities attendant on his advanced period of life, he was desirous of giving in his resignation, and recommending them to look out for a fit successor to the high office which he has so long filled with universal satisfaction to the literary and scientific public. In consequence, however, of an earnest and affectionate remonstrance which has been since made by the Members of the Council, accompanied with strong assurances of their disposition to do every thing they individually can to relieve him of the arduous duties of the situation, Sir Joseph has consented to abandon his intention of resignation for the present. Such a mark of respect was no more than what was due

due to a gentleman who has dedicated a long life and an ample fortune to the promotion of those pursuits for which the Society is particularly incorporated, as well as to the general interests of knowledge.

LINNEAN SOCIETY.

May 2. The following gentlemen were chosen to fill up six vacancies which had occurred among the foreign members:—

F. E. L. Fischer, M.D. Moscow; Gotthelf Fischer, M.D. *Hist. Nat. Prof.* Moscow; J. B. de Monnet de Lamarck, *Zool. Prof. Inst. Sc. Paris. Soc.*; Jos. Pavon, *Acad. Reg. Med. Madr. Soc.*; K. Sprengel, M.D. *Bot. Prof.* Halle; and C. J. Temminck, *Soc. Sc. Harlem. Soc.*

And on Wednesday, May 24, the Anniversary of the Society was held at the Society's house in Gerrard-street, Soho, for the election of a Council and Officers for the present year; when the following Members were declared to be of the Council, viz.

Sir Jas. Edward Smith, M.D.	Wm. Elford Leach, M.D.
Edward Barnard, Esq.	Alexander MacLeay, Esq.
Samuel, Lord Bishop of Carlisle.	Matthew Martin, Esq.
	Wm. George Maton, M.D.
Henry Thos. Colebrooke, Esq.	George, Earl of Mountnorris.
Edward Forster, Esq.	Joseph Sabine, Esq.
Sir Alexander Johnston, Knt.	Thomas Smith, Esq.
Aylmer Bourke Lambert, Esq.	Edward, Lord Stanley, M.P.

And the following were declared to be the Officers for the present year, viz.

Sir James Edward Smith, M.D. President.

Samuel, Lord Bishop of Carlisle,
Aylmer Bourke Lambert, Esq.
Edward, Lord Stanley,
William George Maton, M.D. } Vice Presidents.

Edward Forster, Esq. Treasurer.

Alexander MacLeay, Esq.
Mr. Richard Taylor, } Secretaries.

His Royal Highness Leopold Prince of Saxe Coburg, was also elected an Honorary Member.

The Members of the Society afterwards dined together at the Freemasons' Tavern, Great Queen-street, according to annual custom.

ASTRONOMICAL SOCIETY OF LONDON.

May 12.—A letter was read from G. Peacock, Esq. of Cambridge, respecting the intended new Observatory at Cambridge. Also

Also a paper from James South, Esq. accompanied with a catalogue of double stars, arranged according to the order of their right ascension.

New Observatories.—The reviving taste for Astronomical pursuits is exemplified, not only by the formation of the Society just alluded to, but also by the intended establishment of two new Observatories. One of which is at the expense of Government, and is to be built at the Cape of Good Hope; with an observer, assistants, &c. The other is to be built at Cambridge, partly at the expense of the University, and partly by public subscription. The Plumian professor is to be the observer at this latter place; since it appears that, by the foundation deed, he is *bound* to observe; and his duties are there laid down with great minuteness. Both these observatories are to be furnished with the best instruments our artists can make. The observations made at Cambridge are to be printed *annually*, and circulated amongst the different observatories on the continent: a measure which cannot be sufficiently applauded, and which affords the best check against any negligence on the part of the observer. The observations made at the observatory at Oxford, are suffered to moulder, in their manuscript state, on the shelves of the Royal Society.

ASIATIC COLLEGE.

The first Report of the College for Asiatic Christian and other Youth, instituted at Serampore in August 1818, under the patronage of the Marquis of Hastings, has just been published. The Committee observe, That slow as may have been the progress it has made, they can say with truth that every view they have taken of the subject has served more fully to impress them with the necessity of weighing with the utmost care, every circumstance respecting an institution, the judicious planning of which, or the reverse, must in a great measure decide whether it shall ultimately fall useless to the ground, and like a heap of ruins remain only to discourage others from attempts of the same nature, or abide a blessing to the various tribes of India.

We quote with pleasure the following passages, which show that the immediate and distinct object of the College is liberally to enlighten the people of India, and to diffuse a competent knowledge of arts and sciences, unconnected with any exclusive system of religious instruction.

The Committee observe, "that they are fully convinced of the importance of supporting native youths who are not Christians while they prosecute their studies, as well as those who are. This will be attended with little disadvantage. As a brahmun cannot, without losing cast, eat with a soodra, nor even under the same roof with a brahmun of another province, all youth who are not Christians must live separately, and of course without the walls of the College, in order to preserve inviolable their own ideas of cast, which it is not the design of this institution to constrain them to violate in any degree. But without being thus supported, a youth of the brightest talents might be wholly debarred those advantages which might hereafter render him a blessing to his country; and to lay this as an additional burden on his generous European patron*, who, after subscribing to the College, may have sent thither from the most distant parts of India an ingenious native youth for education, from regard to the faithful services of his parent, perhaps a trusty and valued domestic, would be placing a barrier in the way of his obtaining knowledge of the highest kind, which in most instances would scarcely be surmounted. An institution which ought to combine within itself every advantage for instruction, ought to be as free as the air; and no native youth ought to be deprived of its benefits, for having the misfortune to be born and brought up within any particular circle; the barrier to admission ought to be none, beyond the inability of its funds to support and instruct more.

"They are equally convinced, that no native youth should be *constrained to do a single act as the condition of his enjoying the benefits of this institution, to the doing of which he attaches any idea of moral evil.* As it can be no crime in any youth that he did not regulate the circumstances of his birth and of his first reception of ideas,—to make it the condition of his receiving certain important literary advantages, that he shall be constrained to do what he himself deems it wrong, or to hear books read which he deems it wrong to hear, is the ready way to corrupt the moral principle implanted in his mind by nature. While therefore the Committee are aware of the necessity of guarding against the omission of College duties from mere idleness, under the pretence of conscience, they are firmly convinced, that to compel any native youth to violate *his* sense of right and wrong, would be to teach him to act against his conscience for the sake of advantage;—and that to deprive him in the least degree of the benefits of the institution for refusing it, would be, to turn a desire to act rightly into a crime, and to be guilty of the most

* In the Prospectus, published in August 1818, it was mentioned, that any gentleman who might send to the College a native youth not a Christian, would have to support him while there, in addition to his donation or subscription. This has been since weighed by the Committee for managing the College; and, on more mature consideration, it appears to them, that the donation or subscription which entitles any gentleman to send a youth to the College, ought to support him while there, whether he be a Christian youth or not. Hence the above article.

flagrant

this, if it be ever effected, must be attempted by suitable means; flagrant injustice. In their view, nothing but incorrigible negligence, or immoral conduct, can form a just reason for depriving any youth, whatever be his religious prejudices, of the advantages of this institution.

" They also feel the propriety of introducing into this College, *all the science now possessed by the natives themselves*. To an institution intended to convey superior information to native youth of the highest casts, it is desirable that there be that respectability attached in the eyes of the most learned among the natives, which shall prevent their undervaluing the instruction conveyed, because it is not *what they have*. All the science they really have, ought to be preserved, and not a particle of it lost. If they have carried the study of any branch of knowledge beyond us, this circumstance ought to be acknowledged and improved; if they have merely trodden in the same path, a knowledge of the science they really have, will enable us to take it up where they fail, and carry it to its proper extent; while the ideas they now possess, and the terms in which they express them, will facilitate the communication of superior ideas. This particularly applies to grammar, and to astronomy, which latter science, from its connexion with their religious festivals, is cultivated by them with peculiar eagerness."

There are thirty-seven students attached to the College, of which nineteen are Christians or of Christian families,—fourteen are Hindoos of cast, and four have neither cast nor religion. Of the fourteen Hindoos of cast, *eleven are Brahmuns;* nine of whom are thoroughly acquainted with astronomy and geography, and who receive for their monthly support three rupees each. They are now employed in studying the *Lilavatee*, the first astronomical work usually taught in India, preparatory to being made acquainted with European astronomy.

Since the institution of the College the Committee have decided upon printing

" *The Four* VEDAS, the great palladium of Hindooism, around which Superstition, for obvious purposes, has thrown such a degree of veneration, that the soodra who durst only listen to them, was deemed guilty of a crime to be expiated only by melted lead being poured into his ears. These days of darkness however are now evidently passing away: every thing sacred in the Scriptures has been exposed to public view; and the enlightened among the Hindoos themselves do not hesitate to acknowledge, that darkness and concealment can add no real worth to any work, but rather tend to create suspicion; that every work must found its claims to intrinsic worth wholly on its contents; and

 that

that these claims can never be substantiated as long as its contents are withheld from public view.—The expense of printing this work will undoubtedly be great, as it is voluminous, and its completion must necessarily occupy many years: nor do they expect that the College will ever be reimbursed for the expense of printing it; since, with the exception of learned bodies in Europe and America, who may wish to enrich their libraries with a copy of a work so ancient, few purchasers can be expected. As copies of the Vedas, however, are already so scarce, and are likely to become far more so, the Committee think it highly proper to take measures to preserve this ancient monument of Hindooism from complete oblivion."

The Committee having purchased a spot of ground, on the banks of the river, exactly facing the country residence of the Governor-general at Barrackpore, which cost 11,500 rupees, have determined on erecting the buildings for the College, to consist of a centre building which shall include a large hall for public examinations and the dispatch of public business, rooms for the various classes, suitable rooms for the library and the museum and an observatory above; and of two wings for the accommodation of the students and professors. The building will contain two stories, it being intended to render the rooms on the ground floor equally habitable with those above, with the view of combining the strictest œconomy with the greatest convenience and neatness. On the same principle the erection of a double row of rooms has been preferred, a single row involving a certain waste of expense, and a triple row being highly inconvenient for those who should occupy the middle rooms. The central building will form a front of about a hundred and twenty feet, each wing an extension of somewhat more than a hundred and eighty; and the whole when completed present a front of nearly five hundred feet. Each of the wings will contain rooms for the accommodation of nearly a hundred native Christian students, besides rooms for the professors. The College, when completed, therefore, will be capable of containing from a hundred and sixty to two hundred Christian students; and perhaps an equal or greater number of other students will preserve their cast inviolate by boarding in the town. It is not intended, however, to build more than one wing at present; the rest of the buildings can be added as the circumstances of the College may render them necessary.

The following are the concluding remarks of the Committee:

"The plan of the institution, thus fully developed, they respectfully leave before the public. If India needs enlightening, beyond almost any other blessings, as is universally acknowleged, this, and to be done efficiently, it should be attempted through the *natives themselves*, as Europeans are too far removed from them, and too little adapted to the climate, to become the immediate agents to any extent in this important work. But if it must be done by native agents, what method more likely to effect it, than that of collecting youths from every tribe and every part of India, and, restraining them from nothing but idleness and positive vice, to imbue their minds with the love of study and investigation, lay open to them, by means of an ample library and able teachers, the various stores of learning and science furnished by the western as well as the eastern world, and give them leisure and opportunity to pursue their researches free from interruption and distracting care, till they ultimately return to their own provinces, fraught with knowledge, and not corrupted by unreasonable expectations, to become a blessing in their own sphere to the end of life? But to accomplish this, some spot is necessary, secluded from those allurements to vice which abound in eastern capitals, together with a library and apparatus, the collection of which, with suitable buildings, and the support of able professors, involves too great an expense to be provided in many different places at the same time. Of the suitableness of Serampore for this purpose, sufficiently near the capital of India and yet perfectly retired—and the fitness to accomplish this object, of the plan now so fully laid before the public, the Committee leave the public to judge; in them it might appear presumption. They merely add, that these ideas are the result of many years devoted to the consideration of the state of India, and the most effectual means of promoting its best interests. To this complete disclosure of them, they have therefore nothing to add, but that every benefaction to the Institution, whether intended as a donation for the general purposes of the Institution, or for the support of particular native students,—or whether it be in the form of annual contribution for a few years, will be received with the warmest gratitude, and applied with the utmost consideration and faithfulness."

LXVII. *Intelligence and Miscellaneous Articles.*

ELATERIUM.

In the examination of elaterium, a substance whose action on the human body is still more violent than arsenic, Dr. Paris has discovered a new vegetable principle. The active principle of this plant is lodged only in the juice around the seeds, and in so small a quantity, that Dr. Clutterbuck obtained only six grains from forty cucumbers; and even of this fecula, or feculence, as Dr.

Dr. Paris proposes to call it, although an eighth part of a grain will purge violently, yet not more than one-tenth of this virulent substance possesses any active virtues. As Dr. P.'s experiments are new, we shall insert them at full length:—

"Experiments.—Series the First.

"A. Ten grains of elaterium, obtained from a respectable chemist, and having all the sensible properties which indicated it to be genuine, were digested for twenty-four hours, with distilled water, at a temperature far below that of boiling; *four grains* only were dissolved.

"B. The solution was intensely bitter, of a brownish yellow colour, and was not in the least disturbed by alcohol, although a solution of *iodine* produced a blue colour; the solution, therefore, contained no gum, and only *slight traces* of starch.

"C. The solution, after standing twenty-four hours, yielded a *pellicle* of insoluble matter, which, when burnt, appeared to resemble *gluten*.

"D. The six grains which were insoluble in water were treated, for forty-eight hours, with alcohol, of the specific gravity .817, at 66° of Fahrenheit; a green solution was obtained, but by slow evaporation *only half a grain* of solid green matter was procured. The insoluble residue obstinately adhered to and coated the filter, like a varnish, and completely defended the mass from the action of the alcohol: it is probable that it consisted principally of *fecula*.

"Experiments.—Series the Second.

"E. Ten grains of elaterium, from the same sample, were treated with alcohol, of the specific gravity .817, at 66° Fahrenheit, for twenty-four hours; upon being filtered, and the residuum washed with successive portions of alcohol, the elaterium was found to have lost only 1.6 of a grain. The high specific gravity of the alcohol in this experiment was important; had it been lower, different results would have been produced.

"F. The alcoholic solution, obtained in the last experiment, was of a most brilliant and beautiful green colour, resembling that of the oil of cajeput, but brighter: upon slowly evaporating it, 1.2 grain of solid green matter was obtained.

"G. The solid green matter of the last experiment was treated with boiling distilled water, when a minute portion was thus dissolved, and a solution of a most intensely bitter taste, and of a brownish yellow colour, resulted.

"H. The residue, insoluble in water, was inflammable, burning with smoke, and an aromatic odour, not in the least bitter; it was soluble in alkalies, and was again precipitated from them unchanged in colour; it formed, with pure alcohol, a beautiful

tiful tincture, which yielded an odour of a very nauseous kind, but of very little flavour, and which gave a precipitate with water; it was soft, and of considerable specific gravity, sinking rapidly in water; circumstances which distinguish it from common resin; in very minute quantities it purges. It appears to be the element in which *all* the powers of the elaterium are concentrated, and which have been denominated *elatin.*

"I. The residuum, insoluble in alcohol, weighing 8.4 grains, (Exp. E.) was boiled in double distilled water, when 5.9 grains were dissolved.

"J. The above solution was copiously precipitated *blue*, by a solution of *iodine*, and was scarcely disturbed by the *per-sulphate of iron.*

"K. The part insoluble, both in alcohol and water, which was left after Experiment I. amounted to 2.5 grains; it burnt like wood, and was insoluble in alkalies."

From these experiments, Dr. Paris expresses the chemical composition of elaterium in the following manner:—

	F.	Water	.4
I.	B.	Extractive	2.6
I.	B.D.J.	Fecula	2.8
I.	C.	Gluten	.5
	K.	Woody matter	2.5
	H.	*Elatin*	1.2
	G.	Bittter principle	(1.2, bracketed with Elatin)
			10

The bitter principle in elaterium is very distinct from its extractive matter: the solution obtained in Exp. G. being diluted and swallowed, produced only an increase of appetite; and the solution B. produced no effect whatever.

SMOKE FROM STEAM-ENGINES AND FURNACES.

On the 2d of May Mr. M. A. Taylor brought forward a motion in the House of Commons, for a Select Committee to consider of the practicability of compelling persons using steam-engines and furnaces to erect the same in a manner less prejudicial to the health and comfort of the public. His object, he said, was to consider of some means by which the smoke might be consumed before it issed from the chimneys. From the discoveries he had made, and from what he had himself witnessed, he was sure that the smoke which now issued to the destruction of all kinds of vegetables, might be removed and prevented. He had formerly brought this subject before the House, and a Committee had been granted, the report from which clearly showed that the plan for consuming smoke was not impracticable. Since then

then he had seen several places where smoke was actually consumed; and if the House would take the pains to inquire, they would find it no difficult matter to put a stop to those nuisances which now made London, Liverpool, Birmingham, and Manchester almost uninhabitable. Gentlemen might think this statement exaggerated—but he meant to say, that literally those places were uninhabitable, for many people residing near great furnaces were obliged to send their families away from their habitations to places hired for their reception, in order that they might be free from those unhealthy nuisances. This was done by numbers of people in Manchester. The plan he should propose had been successfully carried into effect at Warwick, in the manufactory of a Mr. Barnes; and he could assert that it had been done with scarcely any additional expense, and with one-third less consumption of fuel than was now generally used in furnaces. If gentlemen who heard him would take the trouble of going to the manufactory of Mr. Barnes, they would find this to be the fact; they would scarcely be able to find where the furnace was; and they would perceive no more smoke there than in any common chimney. He had himself gone to Warwick, to be satisfied of the fact which he now stated; and as a proof that no nuisance existed in the manufactory he spoke of, there was a bleaching-ground within ten yards of the furnace; there was also a garden, and a conservatory; and none of these sustained the slightest injury or annoyance from smoke. A similar plan had been adopted with equal success at a distillery at a little distance beyond the Penitentiary on Mill-bank. If gentlemen would go there, they would see a black intense smoke coming out of one furnace and passing into another, where it was entirely consumed before it got into the open air. In order to be fully satisfied of the effect of the opposite and generally prevailing system, they need only go over to the other side of the water, where there were several furnaces sending forth their masses of smoke every day. In Bridge-street, Blackfriars, there was a furnace which annoyed the whole neighbourhood. He had witnessed ebullitions of smoke near St. James's palace; and those who resided near or frequented Hyde-park must have been frequently annoyed by that which issued from the Cannon brewery. If he should be able to satisfy the House that the nuisance could be abated by the application of certain remedies, he hoped he might be permitted to bring in a declaratory law by which such nuisance might be abated by the verdict of a jury. If he succeeded in effecting an object so conducive to the health and comfort of the public, he should consider himself as having deserved well of his country. He concluded with moving that a Select Committee be appointed to take the subject into their consideration.

Mr.

Mr. Denman seconded the motion. He had himself witnessed all the inconveniences and nuisances stated by his honourable friend, and he was sure they could be removed.

Sir Charles Mordaunt spoke to the same effect; and expressed his conviction that the proposed law would be beneficial to the nation at large.

The motion was agreed to, and the Committee appointed.

STEAM NAVIGATION.

A new ship, intended to ply as a regular packet between New York and New Orleans, has recently been built, called the Robert Fulton. She is said to be, in every respect, one of the finest steam vessels ever constructed. She is upwards of 750 tons, of a very great length, rigged with lug sails; has three kelsons, (the centre one large enough for a ship of the line,) together with bilge-ways, and the whole secured and bolted in a very superior manner; her frame timber and plank are of live oak, locust cedar, and southern pine, copper bolted and coppered.

She will afford accommodation for more than 200 persons, and is fitted up with high and airy state rooms, thoroughly ventilated by means of sky lights the whole length of the cabin, which is very extensive. Her after cabin is neatly arranged for the accommodation of ladies, and separated by means of folding doors, in the modern style. She has also a range of births fore and aft, together with a commodious fore cabin. And, what adds to the greatest comfort and security of all, her engine and other machinery are completely insulated, and unconnected as it were with the other part of the ship. In the centre, lengthwise, is a kind of well-hole or square trunk, made both fire and water-proof; no possible accident, therefore, by the bursting of the boiler, can reach either of the cabins. This trunk or well-hole being inclosed by very thick plank, caulked and leaded, may be inundated with water at pleasure, without any inconvenience to the passengers.

The furnace is also completely surrounded by the continuation of the boiler, so that no part of the fire can ever come in contact with the wood. There is a space of about nine or ten inches filled in with materials, nonconductors of heat, which answer the double purpose of excluding the heat from the cabin, and at the same time deadening the disagreeable noise of the engine. She is also provided with a leather hose, similar to those used by our fire-engine companies in this city, which will enable the hot or cold water to be conveyed to any part of the ship, and furnishing at the same time the great conveniency to the passengers of a warm or cold bath at pleasure. Her engine was constructed by by Mr. Allaire, and is supposed to be the most powerful and most exact piece of workmanship ever turned out in America; and her boiler is said to be the largest ever known to have been made in that or any other country.—*American Paper.*

A royal brig, called *Le Voyageur*, was lately fitted out at L'Orient for a voyage to Senegal, as a steam-packet, the first of this construction that has quitted a French port for a distant expedition. Intelligence has been received of her safe arrival at the place of her destination.

VARIATION OF THE MAGNETIC NEEDLE.

In our 53d Volume, p. 387, we mentioned that the excellent and unremitting observations of Colonel Mark Beaufoy, made at Bushey-Heath, near Stanmore, in Middlesex, had shown that the magnetic variation to the westward of the true north had uniformly increased, on taking the means monthly, until the beginning of the last year, after which it had fluctuated, but giving a mean variation of 24° 37′ 0″ in the first three months of 1819. The observations since published by the Colonel in a contemporary Journal, seem to show that this was *the maximum variation*, occurring in February or March 1819: because he finds the monthly means, since the beginning of April of that year, to have uniformly decreased. It further appears from the Colonel's statements, that the western variation had been on the increase through 162 years, or since 1657: it was only 77 years before this period that the first authentic observations on the variation can be found, or in 1580, when the needle at London varied to the east 11° 15′.

MARINE THERMOMETER.

From many experiments made of late years by scientific persons, there seems every reason to believe that the thermometer is an instrument of far greater importance to navigators than it has been generally supposed.

The late celebrated Dr. Franklin was the first person who noticed the great difference between the temperature of the water on the North American coast, on and off soundings, and suggested the use of a thermometer as an indicator of an approach to that dangerous shore, as it had been uniformly found that the nearer any vessel approximated the shore, the colder the temperature of the water became.

Afterwards Col. Jonathan Williams, of Philadelphia, endeavoured with some success to call the attention of seafaring men to the importance of the thermometer as a nautical instrument; and satisfactorily succeeded in showing that no vessel on board of which a thermometer is, can possibly be cast away on the coasts

of

of the United States, without at least a sufficient warning of the approach to danger, to allow of its being avoided, unless the ship should be so entirely disabled as to be totally unmanageable.

The statements of Dr. Franklin and Colonel Williams applied only to the coasts of North America; and hence it came to be generally supposed that the increased heat of the sea, when out of soundings, was caused by the Gulf stream-current, which issuing from the Gulf of Mexico, sweeps to the northward along the coasts of the United States: it has of late however been established that the decreasing temperature of the water, as any vessel approaches the coasts of Spain, Portugal, and Barbary, is sufficient to give warning to any attentive navigator of his approach to these coasts; and it seems probable, from the experiments of Mr. Davy, (brother to the celebrated Sir Humphry,) that the thermometer will be found to point out, not only the proximity of land, but also that of extensive banks, &c. in all places.

A person whose experience had shown him that in quitting the American coasts, there was an increase of 12 deg. of Fahrenheit's scale in the temperature of the sea in a few hours run from the mouth of the Delaware, found also on approaching the coast of Portugal, that the mercury in the tube of the thermometer sunk from 69 degrees, at which it stood in the open sea, to $60\frac{1}{2}$ degrees, when his ship was about three or four miles from Cape St. Vincent; and subsequently, that in beating through the Straits of Gibraltar with a contrary wind, the mercury in the thermometer rose and fell in proportion to the distance he was from the Spanish or African shores, ranging from 68 degrees, at which it stood in the middle of the Strait, to 61 degrees, which was the lowest to which it sunk on the African side; and on the Spanish shore it never fell lower than 64 degrees; which is easily accounted for, as the ship was never so near that shore, it being considered adviseable to keep at a distance from the shoals, &c. near Tariffa.

The person already mentioned having discovered many objections to the mode of using the thermometer, recommended by Colonel Williams, and having had several thermometers broken, applied to different mechanics in various places to construct a marine thermometer case for him, which would protect the instrument and facilitate its use; but unsuccessfully until he some time since applied to Messrs. Gardner and Jamieson, mathematical instrument makers in Glasgow. Mr. Jamieson, of that firm, invented and made a case, which not only prevents the thermometer inclosed in it from being injured, but admits and retains water from any depth which may be desired; so that the results obtained by the experiments made with it are exempted from any chance of being influenced by the solar rays in summer

 weather

weather or warm latitudes, or by the chill of the air in winter or cold climates, as by an ingenious contrivance the bulb of the thermometer is kept immersed in a column of water admitted and retained by the case, from the greatest depth to which it has been sunk.

Mr. Purdy, the hydrographer of London, has expressed his opinion of Mr. Jamieson's invention in very flattering terms, as have also many highly respectable scientific and nautical men.

NOTE FROM MR. RIDDLE.

To Mr. Tilloch.

Sir,—From an expression in Mr. Meikle's note, printed in your last Number, I feel it my duty to request that those who think proper may examine for themselves, whether, in the quotations which I gave from his letter, I have in any respect distorted the meaning of the words which he actually used.

After what I have said in my former letters on the question in discussion, it is unnecessary to make any observations on the other parts of his note.

Your obedient servant,
EDWARD RIDDLE.

Trinity House School, Newcastle, 13th May, 1820.

ATMOSPHERICAL PHÆNOMENON.

May 2d, 1820. This morning, soon after sunrise, a very brilliant phænomenon was observed in the neighbourhood of Hartfield, Sussex, a highly-coloured discoid halo, accompanied by a parhelion or mock sun. The temperature has of late been exceedingly low for the time of year, and the atmosphere hazy and obscure. The thermometer scarcely rising to temperate, (52°), and at night often as low as 32° of Fahrenheit's scale.

AFRICA.

By the latest information, it seems that the expedition under the command of Major Gray, on whom the direction devolved after the death of Major Peddie, has returned to Galam, on the Senegal, after a most harassing journey through the country of the Foolado. Mr. Docherd, the surgeon attached to the expedition, had, with a few individuals, however, proceeded onwards to Bammakoo, in Bambarra, from whence accounts have been received from him, dated twelve months since, expressing his hopes of procuring the necessary permission to proceed further. Markets, it seems, were held twice every week at Sandsanding and Yamina, where provisions were reasonable, and every sort of European merchandize in great demand, especially articles of finery for the dresses of the females, who are fond of showy co-

lours.

lours. Among other things were Manchester prints in great abundance, which seemed to meet a ready sale, and which must have been conveyed by the caravan from Morocco across the Great Desert. Lieutenant Lyon, of the Royal Navy, who was the friend and fellow traveller of the late Mr. Ritchie, is appointed to succeed that gentleman as British Vice Consul at Mourzouk, the capital of Fezzan, in Africa, for the purpose of facilitating and attempting discoveries. By the Magnet, which left Cape Coast on the 23d March, we learn that Mr. Dupuis had proceeded to Cormassie, to enter upon his functions as Consul at the Court of the King of Ashantee, and had arrived in safety and been well received.

LIST OF PATENTS FOR NEW INVENTIONS.

To Major Rohde, of Leman-street, Goodman's Fields, Middlesex, sugar-refiner, for a method of separating or extracting the molasses or syrup from Muscovado or other sugar, communicated to him by a person residing abroad.—15th April, 1820.—6 months allowed to enrol specification.

To William Brunton, of Birmingham, engineer, for certain improvements on, and additions to, fire grates.—19th April.—6 months.

To George Lilley, of Brigg, Lincolnshire, gent., and James Bristow Fraser, of Blackburn House, Linlithgowshire, in Scotland, gent. for certain improvements in the application of machinery for propelling boats or other vessels floating in or upon water, and for attaining other useful purposes, by means of an hydro-pneumatic apparatus acted upon by a steam-engine or other adequate power.—19th April.—6 months.

To Thomas Hancock, of Little Pulteney-street, Golden-square, Middlesex, coach-maker, for an application of a certain material to various articles of dress, and other articles, by which the same may be rendered more elastic.—29th April.—6 months.

To Thomas Cook, of Brighton, Sussex, engineer, for his improved apparatus for the purpose of cooking, which he designates A philosophical cookery.—29th April.—6 months.

To John Hague, of Great Pearl-street, Spital-Fields, Middlesex, engineer, for certain improvements in the method of heating hot-houses, manufactories, and other buildings, and boiling liquids.—9th May.—2 months.

To John Ambrose Tickell, of West Bromwich, Staffordshire, gent. for a cement to be used in aquatic and other buildings and stucco-work, which is produced by the use and application of a mineral substance never before employed in the manufacture thereof.—9th May.—2 months.

To

To Josiah Parkes, of the Borough of Warwick, worsted manufacturer, for his new and improved method of lessening the consumption of fuel in steam-engines and furnaces in general, and for consuming smoke.—9th May.—6 months.

To James Jacks, of Camberwell, in the county of Surry, gent. and Arthur Aiken, of the Adelphi, Westminster, gent. for a new or improved method of preventing mildew in sail-cloth and other canvass, and in other manufactures made of vegetable fibre.—11th May.—6 months.

To James Scott, of Grafton-street, in the parish of St. Anne, in the city of Dublin, watch-maker, for his new method of combining, adjusting, and applying by machinery certain of the well known mechanic powers and modification thereof, where power and velocity are required.—11th May.—6 months.

To John Malam, of Romney-terrace, Horseferry-road, Westminster, engineer, for certain improvements on gas meters.—11th May.—6 months.

To Samuel Kenrick, of West Bromwich, Staffordshire, manufacturer, for his improved method of tinning cast-iron vessels of capacity.—13th May.—6 months.

To Robert Wornum, of Wigmore-street, Cavendish-square, Middlesex, piano-forte-maker, for his improvement on piano-fortes and certain other stringed instruments.—13th May.—2 months.

To Robert Bill, of Newman-street, Oxford-street, Middlesex, esq. for his improved mode of constructing beams, masts, yards, bow-sprits, and other parts of ships, vessels, and craft, used for the purpose of navigation, and of other parts of rigging of such ships, vessels and craft.—15th May.—2 months.

To John Barton, of Falcon-square, London, engineer, for certain improvements in propelling, and in the construction of engines and boilers applicable to propelling, and other purposes. —15th May.—6 months.

To Richard Watts, of Crown-court, Temple Bar, in the county of Middlesex, printer, for his improvements in inking printing types with rollers, and in placing and conveying paper on types, and in inking with a cylinder.—15th May.—4 months.

To Robert Winch, of Shoe-lane, London, press-maker, for his certain improvements on machines or presses, chiefly applicable to printing.—18th May.—4 months.

To Edward Massey, of Eccleston, in the parish of Prescot, in the county of Lancaster, and also of the city of Coventry, watch-manufacturer, for certain improvements in the construction of chronometers and pocket watches.—19th May.—6 months.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1820.	Age of the Moon.	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
April 15	3	52·5	29·60	Cloudy
16	4	57.	29·84	Fine
17	5	65·	29·97	Ditto
18	6	65·	29·90	Ditto
19	7	67·	29·85	Ditto
20	8	58·5	29·90	Cloudy
21	9	64·	30·	Fine
22	10	62·	30·11	Ditto
23	11	58·	30·30	Ditto
24	12	56·	30·34	Ditto
25	13	53·5	30·34	Ditto
26	14	51·5	29·60	Cloudy
27	15	42·	29·65	Stormy—rain A.M.
28	full	51·5	29·86	Fine
29	17	57·	29·86	Cloudy
30	18	54·	30·	Fine
May 1	19	51·5	30·14	Cloudy
2	20	51·	30·03	Ditto
3	21	44·	30·03	Ditto
4	22	48·	29·80	Ditto
5	23	52·	29·80	Ditto
6	24	52·5	29·65	Ditto
7	25	52·5	29·55	Ditto—rain A.M.
8	26	63·	29·50	Ditto
9	27	63·	29·40	Fine—rain P.M. and a beautiful [rainbow
10	28	66·	29·50	Ditto—brisk wind
11	1	60·	29·60	Cloudy—ditto rain P.M.
12	new	63·	29·73	Fine
13	3	67·	29·65	Ditto
14	4	65·	29·57	Ditto

METEOREO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For May 1820.

Days of Month. 1820.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
April 27	45	45	40	29·92	0	Cloudy
28	43	53	45	30·10	0	Fair
29	46	57	49	·16	0	Cloudy
30	49	57	45	·30	0	Fair
May 1	44	58	46	·40	0	Fair
2	46	59	44	·30	0	Cloudy
3	44	50	45	·23	0	Cloudy
4	47	51	41	·02	0	Fair
5	44	50	42	29·94	0	Fair
6	46	59	45	·92	0	Fair
7	47	60	60	·85	0	Fair
8	57	61	56	·80	0	Cloud
9	56	63	59	·76	0	Fair
10	57	60	55	·92	0	Fair
11	56	67	54	·97	0	Fair
12	54	66	55	·94	0	Fair
13	50	57	56	·92	0	Fair
14	58	65	50	·95	0	Fair
15	51	63	55	·90	0	Fair
16	56	66	53	·80	0	Showery
17	55	64	54	·80	0	Fair
18	55	55	48	·34	0	Rain
19	50	57	49	·86	0	Showery
20	52	67	54	30·29	0	Showery
21	55	69	56	·42	0	Fair
22	57	71	57	·30	0	Fair
23	59	73	56	·07	0	Fair
24	58	70	57	29·84	0	Fair
25	55	60	50	·83	0	Showery
26	58	60	57	·90	0	Showery

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

LXVIII. *On the apparent Place of the Pole-Star.* *By* FRANCIS BAILY, *Esq.*

THE following tables, showing the apparent place of the pole-star for every day of the years 1820, 1821, and 1822, were lately published at Dorpat in Livonia, by Dr. Struve, the director of the observatory at that place. They are computed from the formulæ and tables given by M. Bessel in various periodical works, which are not generally known or read in this country: and therefore the following account of them may perhaps be acceptable to the public.

It is well known to the practical astronomer that the pole-star, on account of its magnitude, and proximity to the pole, can be seen (with telescopes of no very considerable power) at all times of the year, by day as well as by night: so that, when the weather is favourable, it may be observed on the meridian twice within every twenty-four hours. On account of the extensive use to which such observations may be applied, it has become desirable that the apparent position of this star in right ascension and declination should be accurately determined for that precise moment in every day in the year, *when it passes the meridian.*

The tables, which follow, have therefore been calculated, for that purpose, by Dr. Struve abovementioned, and Dr. Walbeck the director of the observatory at Abo in Finland. The places of the star were calculated by each of them separately for every fifth day; and the values for the intermediate days were interpolated, after applying for each separate day the correction depending on the moon's longitude. The method of deducing these values, I shall now proceed to explain.

The mean place of the pole-star for the 1st day of January in any given year (denoted by $1815+t$, where t denotes the number of years from 1815) is first deduced from the formulæ given by M. Bessel, in his *Fundamenta Astronomiæ*, page 306: one of which formulæ serves to determine the right ascension of the star, and the other its declination. They are as follow:

Æ.=	D.=
13°. 57′. 11″,21	88°. 19′. 17″,30
$+ t.$ 212″,40793	$+ t.$ 19″,47475
$+ t^2.$ 0 ,61012 86	$- t^2.$ 0 ,00255 27
$+ t^3.$ 0 ,00190 9882	$- t^3.$ 0 ,00000 8231
$+ t^4.$ 0 ,00000 57903 5	$- t^4.$ 0 ,00000 0025491
$+ t^5.$ 0 ,00000 00170 851	
$+ t^6.$ 0 ,00000 00000 4863	
$+ t^7.$ 0 ,00000 00000 001321	

whence,

whence, the mean places, for the 1st of January in the years 1820, 1821, and 1822, will be as under: viz.

	Æ. in time.	D.
1820 =	0h. 57′. 0″,583	88°. 20′. 54″,609
1821 =	57. 15,203	21. 14 ,055
1822 =	57. 29,909	21. 33 ,495

Next come the corrections on account of precession, aberration, nutation &c: and it is here that M. Bessel has shown that peculiar talent for analysis, for which he is so justly celebrated. The formulæ for these corrections are thrown into a synoptical table, in page 404, for the sake of a more convenient reference; and are taken from the fourth part of the Astronomical observations made by M. Bessel at the Royal Observatory in Konigsberg; a work which contains a fund of useful and interesting information, and is by far the best specimen of an active observatory, of any extant. The quantities, expressed by Æ, D, ω, ☉, ☽, ☊, denote respectively the right ascension and declination of the star, the obliquity of the ecliptic, the true longitude of the sun, the true longitude of the moon, and the place of the moon's nodes.

In the formulæ No. 2 and 5, the reader will readily recognise the common formulæ for aberration and nutation: the co-efficients for the nutation are those given by M. Lindenau; and for the aberration, that adopted by M. Delambre. The other quantities are here for the first time deduced by M. Bessel, for the purpose of determining the apparent place of the star to the greatest exactness. By the help of a few subsidiary tables he has rendered the use of these formulæ very simple and easy. The formulæ 1. 2. 3. 4 he has thrown together into one table, showing their values for every fifth day of the year: 5. 6. 7, together with the mean place of the star, he has thrown together into another table, showing their values for every hundredth day, for the years 1805 to 1826: No. 8. 10 are in separate tables: and 9 is altogether omitted, as its maximum never exceeds 0″,01. These subsidiary tables are calculated for the meridian of Paris, and corrections are given for applying them to the meridians of other observatories: the correction for Greenwich is insensible.

From these subsidiary tables, the following tables have been calculated: and it should be particularly observed that they show the apparent place of the pole-star *at the time of its upper culmination;* and not its place *at noon,* from which it will frequently very sensibly differ. M. Bessel has explained the method, whereby he deduces the apparent place of a star at the time of its culmination, in Bode's *Astronomisches Jahrbuch* for 1817, page 197; as well as in his *Fundamenta Astronomiæ,* page 67, where

where he has moreover given tables and rules for determining the apparent places of 14 of the principal stars observed by Bradley, *at the time they passed the meridian* of Greenwich, for the years 1750 to 1762. And in the first part of his Astronomical observations above mentioned, he has given tables and rules for determining the apparent places of Dr. Maskelyne's 36 principal stars *at the time of their culmination*, for every tenth day of the year. As these stars are generally observed when on, or near, the meridian, it certainly would be desirable that their apparent places should be given for the time of their culmination, and not for *noon*, as is usually practised.

I have thought it the more necessary to make these observations, in order to obviate any false impression which may arise from comparing the values given in this table, with those which are now given for the first time in the Nautical Almanac for 1822. For instance, the apparent place of the pole-star on June 29, 1822 is stated in the Nautical Almanac to be $Æ = 0^h. 57'. 19'',6$, $D = 88°. 21'. 29'',6$; whereas, in the following tables, it is stated to be $Æ = 0^h. 57'. 20'',8$, $D = 88°. 21'. 29'',0$. Now various reasons may be assigned for this, and other differences: in the first place, the computations are not made for the same moment of time; secondly, the mean place, at the beginning of the year, is not the same in each; and thirdly, the corrections are more numerous in the latter than in the former. Still however this will not account for the *whole* of the differences observable in the two tables.

As the pole-star is sometimes obscured, when other principal stars in its neighbourhood, such as δ, β, γ *Ursæ minoris* and γ *Cephei*, are visible, the observations of which might be made available to many useful purposes, would it not be desirable to attempt the correction of the places of those stars in a similar manner? Some of the stars, also, situated still further from the pole, and immediately in the zenith, such as γ *Draconis* and others, of which considerable use is made in practical astronomy, may likewise be subject to variations not hitherto understood; but which may hereafter yield to the investigations of the analyst. In short, is it too much to presume that what is called the *proper motion* of a star may at some future time be resolvable into general formulæ; whereby its value may be ascertained in the same way as that of aberration and nutation? These subjects, however, I shall leave at present for the consideration of those who have leisure and disposition for the investigation: contenting myself with having directed the attention of the public thereto.

It is scarcely necessary to add that the following formulæ for the right ascension must be divided by 15, if it be required to determine it *in time*.

Formulæ for correcting the Right Ascension.

1. $+$ Annual precession $\times \frac{\text{mean long. of sun} - 280^\circ}{360^\circ}$.

2. $- 20'',255 \, (\cos \omega . \cos \text{Æ} . \cos \odot + \sin \text{Æ} . \sin \odot) \sec D.$

3. $- 1'',14292 \sin 2\odot - \tan D \, (0'',57998 \cos \text{Æ} . \cos 2\odot + 0'',49609 \sin \text{Æ} . \sin 2\odot).$

4. $+ \sec D^2 . \, (0'',00091228 \cos 2\text{Æ} . \sin 2\odot - 0'',00091568 \sin 2\text{Æ} . \cos 2\odot).$

5. $- 15'',39557 \sin \text{☊} - \tan D . \, (8'',97707 \cos \text{Æ} . \cos \text{☊} + 6'',68247 \sin \text{Æ} . \sin \text{☊}).$

6. $+ 0'',08768 \cos \text{Æ} . \cos 2\text{☊} . \tan D.$

7. $\begin{cases} - \tan D . \, (0'',00012469 \cos \text{Æ} . \cos 2\text{☊} + 0'',00033502 \sin \text{Æ} . \sin 2\text{☊}). \\ - \tan D^2 \, (0'',0001518 \sin 2\text{Æ} . \cos 2\text{☊} - 0'',00014542 \cos 2\text{Æ} . \sin 2\text{☊}). \end{cases}$

8. $+ \sin D \left[-0'',00073242 \sin 2\text{Æ} . \cos (\odot + \text{☊}) + 0'',00074173 \cos 2\text{Æ} . \sin (\odot + \text{☊}) \right] \sec D^2.$

9. $+ \sin D \left[-0'',00007622 \sin 2\text{Æ} . \cos (\odot - \text{☊}) + 0'',00013979 \cos 2\text{Æ} . \sin (\odot - \text{☊}) \right] \sec D^2.$

10. $- 0'',18464 \sin 2\text{☽} - \tan D \, (0'',08737 \cos \text{Æ} . \cos 2\text{☽} + 0'',08014 \sin \text{Æ} . \sin 2\text{☽}).$

Formulæ for correcting the Declination.

1. $+$ Annual precession $\times \frac{\text{mean long. of sun} - 280^\circ}{360^\circ}$.

2. $- 20'',255 \, (\sin \omega . \cos D . \cos \odot - \sin D . \cos \omega . \sin \text{Æ} . \cos \odot + \sin D . \cos \text{Æ} . \sin \odot).$

3. $+ 0'',57998 \sin \text{Æ} . \cos 2\odot - 0'',49609 \cos \text{Æ} . \sin 2\odot.$

4. $\tan D \, [\cos 2\odot \, (0'',00003942 - 0'',00045784 \cos 2\text{Æ}.) - 0'',00049726 \sin 2\text{Æ} . \sin 2\odot].$

5. $+ 8'',97707 \sin \text{Æ} . \cos \text{☊} - 6'',68247 \cos \text{Æ} . \sin \text{☊}.$

6. $- 0'',08768 \sin \text{Æ} . \cos 2\text{☊}.$

7. $\tan D \, [\cos 2\text{☊} \, (-0'',00007590 - 0'',00002177 \cos 2\text{Æ}.) - 0'',00007271 \sin 2\text{Æ} . \sin 2\text{☊}].$

10. $+ 0'',08737 \sin \text{Æ} . \cos 2\text{☽} - 0'',08014 \cos \text{Æ} . \sin 2\text{☽}.$

1820.								
JANUARY.			FEBRUARY.			MARCH.		
	Æ.	Dec.		Æ.	Dec.		Æ.	Dec.
	0 hrs.	88°		0 hrs.	88°		0 hrs.	88°
1	56′ 45″60	21′ 16″20	1	56′ 22″71	21′ 15″93	1	56′ 6″12	21′ 10″55
2	44,80	16,32	2	21,97	15,80	2	5,72	10,25
3	43,99	16,43	3	21,26	15,65	3	5,37	9,96
4	43,15	16,50	4	20,60	15,50	4	5,06	9,68
5	42,34	16,56	5	19,99	15,34	5	4,76	9,41
6	41,55	16,57	6	19,42	15,18	6	4,45	9,17
7	40,80	16,59	7	18,86	15,04	7	4,19	8,92
8	40,09	16,60	8	18,29	14,92	8	3,90	8,69
9	39,42	16,63	9	17,70	14,81	9	3,57	8,45
10	38,77	16,67	10	17,10	14,67	10	3,20	8,21
11	38,12	16,69	11	16,47	14,53	11	2,80	7,96
12	37,45	16,74	12	15,81	14,39	12	2,45	7,68
13	36,74	16,80	13	15,09	14,24	13	2,11	7,39
14	36,01	16,84	14	14,39	14,08	14	1,78	7,07
15	35,23	16,88	15	13,75	13,87	15	1,49	6,75
16	34,41	16,88	16	13,13	13,65	16	1,25	6,42
17	33,58	16,89	17	12,54	13,42	17	1,11	6,10
18	32,75	16,87	18	12,00	13,19	18	1,00	5,79
19	31,94	16,82	19	11,49	12,98	19	0,90	5,50
20	31,16	16,76	20	11,06	12,75	20	0,79	5,23
21	30,43	16,68	21	10,63	12,53	21	0,66	4,95
22	29,75	16,60	22	10,20	12,33	22	0,57	4,69
23	29,09	16,52	23	9,74	12,16	23	0,43	4,43
24	28,46	16,47	24	9,24	11,98	24	0,25	4,15
25	27,82	16,43	25	8,75	11,78	25	0,05	3,87
26	27,18	16,38	26	8,21	11,56	26	55′ 59,85	3,57
27	26,50	16,33	27	7,66	11,33	27	59,73	3,26
28	25,78	16,29	28	7,13	11,09	28	59,62	2,93
29	25,02	16,24	29	6,61	10,82	29	59,56	2,60
30	24,24	16,17				30	59,54	2,26
31	23,47	16,06				31	59,57	1,93

1820.								
APRIL.			MAY.			JUNE.		
	Æ.	Dec.		Æ.	Dec.		Æ.	Dec.
	0 hrs.	88°		0 hrs.	88°		0 hrs.	88°
1	55′ 59″68	21′ 1″62	1	56′ 6″59	20′ 52″97	1	56′ 23″69	20′ 47″58
2	59,80	1,31	2	7,01	52,76	2	24,29	47,47
3	59,90	1,03	3	7,38	52,55	3	24,92	47,35
4	59,98 56′ 0,04	0,75 0,49	4	7,73	52,34	4	25,56	47,21
5	0,05	0,22	5	8,07	52,11	5	26,26	47,10
6	0,08	20′ 59,94	6	8,47	51,87	6	27,01	47,00
7	0,09	59,65	7	8,89	51,62	7	27,80	46,90
8	0,11	59,35	8	9,35	51,37	8	28,60	46,83
9	0,15	59,03	9	9,85	51,11	9	29,39	46,77
10	0,22	58,70	10	10,39	50,87	10	30,18	46,75
11	0,38	58,37	11	11,01	50,66	11	30,93	46,73
12	0,59	58,05	12	11,63	50,46	12	31,64	46,72
13	0,84	57,74	13	12,23	50,28	13	32,29	46,70
14	1,11	57,44	14	12,81	50,12	14	32,97	46,68
15	1,37	57,15	15	13,37	49,97	15	33,61	46,66
16	1,69	56,90	16	13,91	49,83	16	34,27	46,62
17	1,97	56,65	17	14,42	49,67	17	34,95	46,58
18	2,23	56,41	18	14,91	49,50	18	35,66	46,53
19	2,43	56,16	19	15,40	49,33	19	36,42	46,48
20	2,61	55,90	20	15,91	49,14	20	37,21	46,46
21	2,83	55,64	21	16,47	48,95	21	38,04	46,45
22	3,05	55,36	22	17,07	48,77	22	38,89	46,46
23	3,30	55,07	23	17,71	48,58	23	39,71	46,49
24	3,57	54,76	24	18,39	48,41	24	40,52	46,53
25	3,90	54,46	25	19,10	48,25	25	41,29	46,59
26	4,32	54,18	26	19,84	48,12	26	42,03	46,66
27	4,76	53,89	27	20,56	48,02	27	42,72	46,73
28	5,22	53,63	28	21,25	47,92	28	43,39	46,78
29	5,69	53,39	29	21,91	47,84	29	44,05	46,82
30	6,13	53,17	30	22,53	47,75	30	44,74	46,86
			31	23,10	47,68			

JULY.

1820.								
JULY.			AUGUST.			SEPTEMBER.		
	Æ.	Dec.		Æ.	Dec.		Æ.	Dec.
	0 hrs.	88°		0 hrs.	88°		0 hrs.	88°
1	56′ 45″44	20′ 46″89	1	57′ 7″88	20′ 51″23	1	57′ 25″47	21′ 0″01
2	46,17	46,92	2	8,63	51,45	2	25,92	0,37
3	46,94	46,96	3	9,38	51,69	3	26,28	0,74
4	47,77	47,00	4	10,07	51,94	4	26,62	1,10
5	48,60	47,08	5	10,74	52,22	5	26,93	1,45
6	49,44	47,17	6	11,36	52,50	6	27,25	1,78
7	50,27	47,28	7	11,93	52,76	7	27,59	2,11
8	51,06	47,41	8	12,48	53,02	8	27,93	2,43
9	51,82	47,53	9	13,00	53,26	9	28,28	2,74
10	52,53	47,69	10	13,53	53,52	10	28,68	3,06
11	53,19	47,83	11	14,08	53,75	11	29,13	3,39
12	53,85	47,96	12	14,66	53,97	12	29,59	3,75
13	54,50	48,08	13	15,28	54,20	13	29,99	4,12
14	55,16	48,17	14	15,92	54,45	14	30,37	4,52
15	55,85	48,28	15	16,57	54,72	15	30,71	4,93
16	56,57	48,38	16	17,24	55,00	16	31,01	5,33
17	57,36	48,50	17	17,88	55,30	17	31,26	5,73
18	58,14	48,63	18	18,49	55,61	18	31,44	6,12
19	58,95	48,77	19	19,05	55,94	19	31,61	6,49
20	59,74	48,94	20	19,56	56,28	20	31,79	6,84
21	57′ 0,52	49,14	21	20,03	56,60	21	31,99	7,18
22	1,27	49,35	22	20,47	56,91	22	32,23	7,53
23	1,97	49,58	23	20,91	57,21	23	32,46	7,86
24	2,64	49,76	24	21,34	57,50	24	32,73	8,21
25	3,25	49,98	25	21,80	57,77	25	33,02	8,58
26	3,85	50,17	26	22,29	58,04	26	33,33	8,96
27	4,46	50,35	27	22,81	58,33	27	33,60	9,36
28	5,08	50,51	28	23,36	58,63	28	33,81	9,77
29	5,73	50,67	29	23,92	58,95	29	33,99	10,19
30	6,41	50,85	30	24,46	59,28	30	34,12	10,61
31	7,14	51,03	31	24,98	59,64			

OCTO-

1820.								
OCTOBER.			NOVEMBER.			DECEMBER.		
	Æ.	Dec.		Æ.	Dec.		Æ.	Dec.
	0 hrs.	88°		0 hrs.	88°		0 hrs.	88°
1	57′ 34″20	21′ 11″03	1	57′ 32″77	21′ 22″82	1	57′ 21″37	21′ 32″27
2	34,26	11,43	2	32,50	23,14	2	20,89	32,51
3	34,27	11,82	3	32,28	23,46	3	20,43	32,75
4	34,29	12,18	4	32,10	23,80	4	19,97	33,01
5	34,35	12,53	5	31,93	24,14	5	19,47	33,28
6	34,43	12,88	6	31,77	24,49	6	18,95	33,57
7	34,55	13,23	7	31,54	24,86	7	18,34	33,84
8	34,67	13,60	8	31,30	25,23	8	17,69	34,08
9	34,81	13,98	9	31,02	25,60	9	17,02	34,33
10	34,95	14,37	10	30,70	25,98	10	16,33	34,55
11	35,06	14,79	11	30,34	26,35	11	15,64	34,76
12	35,14	15,20	12	29,89	26,69	12	14,95	34,93
13	35,14	15,63	13	29,44	27,02	13	14,29	35,09
14	35,09	16,05	14	29,01	27,33	14	13,66	35,26
15	35,00	16,45	15	28,60	27,63	15	13,08	35,42
16	34,89	16,84	16	28,22	27,92	16	12,51	35,59
17	34,79	17,21	17	27,85	28,20	17	11,94	35,76
18	34,65	17,58	18	27,52	28,50	18	11,37	35,95
19	34,54	17,92	19	27,19	28,80	19	10,75	36,14
20	34,47	18,27	20	26,87	29,11	20	10,11	36,35
21	34,45	18,62	21	26,52	29,44	21	9,42	36,55
22	34,45	18,97	22	26,12	29,78	22	8,67	36,72
23	34,40	19,35	23	25,67	30,12	23	7,90	36,87
24	34,36	19,74	24	25,17	30,44	24	7,11	37,01
25	34,30	20,15	25	24,64	30,75	25	6,34	37,13
26	34,20	20,55	26	24,09	31,05	26	5,59	37,23
27	34,05	20,96	27	23,49	31,32	27	4,86	37,30
28	33,82	21,35	28	22,90	31,56	28	4,17	37,38
29	33,56	21,75	29	22,35	31,79	29	3,52	37,46
30	33,29	22,12	30	21,84	32,03	30	2,89	37,56
31	33,02	22,47				31	2,25	37,66

1821.								
JANUARY.			FEBRUARY.			MARCH.		
	Æ.	Dec.		Æ.	Dec.		Æ.	Dec.
	0 hrs.	88°		0 hrs.	88°		0 hrs.	88°
1	57′ 1″61	21′ 37″75	1	56′ 39″15	21′ 37″41	1	56′ 23″31	21′ 32″22
2	0,93	37,87	2	38,39	37,31	2	22,82	31,98
3	0,22	37,99	3	37,63	37,20	3	22,35	31,71
4	56′ 59,46	38,10	4	36,88	37,06	4	21,89	31,43
5	58,66	38,20	5	36,18	36,89	5	21,46	31,13
6	57,82	38,25	6	35,52	36,72	6	21,07	30,83
7	56,99	38,29	7	34,90	36,53	7	20,79	30,54
8	56,19	38,30	8	34,32	36,35	8	20,55	30,24
9	55,41	38,31	9	33,77	36,19	9	20,32	29,97
10	54,67	38,32	10	33,25	36,02	10	20,08	29,70
11	53,97	38,30	11	32,71	35,87	11	19,83	29,46
12	53,30	38,29	12	32,15	35,73	12	19,59	29,21
13	52,66	38,29	13	31,57	35,59	13	19,32	28,96
14	52,01	38,30	14	30,94	35,45	14	19,01	28,71
15	51,34	38,34	15	30,32	35,29	15	18,67	28,44
16	50,64	38,36	16	29,67	35,11	16	18,32	28,15
17	49,91	38,37	17	29,02	34,92	17	18,05	27,84
18	49,12	38,38	18	28,40	34,70	18	17,80	27,53
19	48,32	38,38	19	27,80	34,46	19	17,59	27,19
20	47,48	38,36	20	27,28	34,21	20	17,43	26,85
21	46,68	38,30	21	26,80	33,95	21	17,31	26,54
22	45,88	38,22	22	26,37	33,71	22	17,27	26,24
23	45,13	38,14	23	25,97	33,48	23	17,24	25,92
24	44,41	38,04	24	25,55	33,26	24	17,20	25,64
25	43,74	37,95	25	25,17	33,06	25	17,13	25,37
26	43,11	37,84	26	24,76	32,85	26	17,03	25,11
27	42,51	37,75	27	24,32	32,65	27	16,96	24,84
28	41,89	37,68	28	23,83	32,44	28	16,84	24,57
29	41,25	37,62				29	16,72	24,28
30	40,58	37,58				30	16,58	23,98
31	39,89	37,50				31	16,48	23,65

1821.								
APRIL.			MAY.			JUNE.		
	Æ.	Dec.		Æ.	Dec.		Æ.	Dec.
	0 hrs.	88°		0 hrs.	88°		0 hrs.	88°
1	56′ 16″47	21′ 23″31	1	56′ 23″34	21′ 14″46	1	56′ 41″10	21′ 8″97
2	16,48	22,97	2	23,86	14,22	2	41,79	8,91
3	16,55	22,62	3	24,38	14,00	3	42,43	8,83
4	16,65	22,30	4	24,87	13,79	4	43,04	8,76
5	{ 16,77 { 16,89	21,98 21,68	5	25,32	13,58	5	43,65	8,68
6	17,08	21,40	6	25,78	13,40	6	44,28	8,60
7	17,23	21,14	7	26,21	13,20	7	44,92	8,50
8	17,35	20,88	8	26,62	13,00	8	45,60	8,40
9	17,43	20,62	9	27,01	12,78	9	46,32	8,28
10	17,47	20,34	10	27,43	12,54	10	47,09	8,20
11	17,56	20,06	11	27,91	12,31	11	47,91	8,12
12	17,65	19,77	12	28,43	12,07	12	48,74	8,07
13	17,76	19,45	13	28,99	11,84	13	49,56	8,04
14	17,91	19,13	14	29,60	11,62	14	50,35	8,01
15	18,09	18,81	15	30,23	11,41	15	51,12	8,03
16	18,38	18,49	16	30,90	11,23	16	51,85	8,04
17	18,69	18,17	17	31,55	11,07	17	52,54	8,05
18	19,03	17,89	18	32,18	10,93	18	53,21	8,06
19	19,36	17,62	19	32,77	10,78	19	53,86	8,04
20	19,69	17,35	20	33,32	10,64	20	54,53	8,02
21	20,03	17,12	21	33,87	10,51	21	55,23	8,00
22	20,33	16,89	22	34,40	10,37	22	55,97	7,97
23	20,60	16,65	23	34,93	10,22	23	56,75	7,94
24	20,84	16,40	24	35,48	10,04	24	57,57	7,91
25	21,07	16,14	25	36,06	9,86	25	58,42	7,93
26	21,35	15,87	26	36,70	9,69	26	59,27	7,96
27	21,66	15,58	27	37,40	9,53	27	57′ 0,10	8,02
28	22,01	15,29	28	38,14	9,38	28	0,91	8,09
29	22,40	15,01	29	38,89	9,25	29	1,69	8,17
30	22,84	14,72	30	39,63	9,12	30	2,42	8,27
			31	40,38	9,04			

JULY.

1821.								
JULY.			AUGUST.			SEPTEMBER.		
	Æ.	Dec.		Æ.	Dec.		Æ.	Dec.
	0 hrs.	88°		0 hrs.	88°		0 hrs.	88°
1	57′ 3″11	21′ 8″35	1	57′ 25″46	21′ 12″67	1	57′ 43″18	21′ 21″19
2	3,79	8,42	2	26,10	12,85	2	43,73	21,51
3	4,46	8,48	3	26,79	13,03	3	44,24	21,86
4	5,13	8,53	4	27,50	13,24	4	44,72	22,22
5	5,85	8,57	5	28,23	13,45	5	45,18	22,60
6	6,60	8,62	6	28,96	13,69	6	45,58	22,98
7	7,40	8,68	7	29,68	13.95	7	45,94	23,34
8	8,23	8,75	8	30,38	14,22	8	46,24	23,72
9	9,08	8,83	9	31,02	14,51	9	46,52	24,07
10	9,91	8,94	10	31,62	14,79	10	46,81	24,41
11	10,74	9,09	11	32,16	15,08	11	47,12	24,74
12	11,52	9,24	12	32,69	15,35	12	47,46	25,06
13	12,28	9,40	13	33,20	15,61	13	47,80	25,39
14	12,97	9,55	14	33,71	15,86	14	48,19	25,72
15	13,64	9,71	15	34,24	16,10	15	48,60	26,06
16	14,29	9,86	16	34,81	16,35	16	49,00	26,43
17	14,94	9,98	17	35,42	16,59	17	49,40	26,82
18	15,62	10,11	18	36,07	16,85	18	49,73	27,22
19	16,31	10,22	19	36,71	17,13	19	50,02	27,62
20	17,04	10,34	20	37,33	17,42	20	50,27	28,03
21	17,81	10,48	21	37,95	17,74	21	50,48	28,43
22	18,60	10,62	22	38,54	18,08	22	50,66	28,81
23	19,41	10,79	23	39,09	18,42	23	50,79	29,19
24	20,20	10,97	24	39,56	18,76	24	50,94	29,55
25	20,98	11,19	25	40,00	19,09	25	51,10	29,90
26	21,71	11,42	26	40,41	19,41	26	51,29	30,24
27	22,40	11,64	27	40,83	19,72	27	51,52	30,59
28	23,04	11,86	28	41,26	20,01	28	51,76	30,94
29	23,67	12,07	29	41,68	20,29	29	52,01	31,31
30	24,26	12,28	30	42,14	20,58	30	52,27	31,69
31	24,84	12,47	31	42,65	20,88			

 OCTO-

1821.								
OCTOBER.			NOVEMBER.			DECEMBER.		
	Æ.	Dec.		Æ.	Dec.		Æ.	Dec.
	0 hrs.	88°		0 hrs.	88°		0 hrs.	88°
1	57′ 52″50	21′ 32″09	1	57′ 51″63	21′ 43″99	1	57′ 40″22	21′ 53″55
2	52,71	32,52	2	51,31	44,37	2	39,60	53,77
3	52,84	32,93	3	50,98	44,73	3	39,01	53,98
4	52,92	33,35	4	50,66	45,06	4	38,47	54,19
5	52,96	33,76	5	50,37	45,39	5	37,97	54,41
6	52,98	34,16	6	50,11	45,71	6	37,49	54,63
7	53,00	34,55	7	49,84	46,02	7	36,99	54,86
8	52,98	34,91	8	49,62	46,33	8	36,49	55,09
9	52,99	35,27	9	49,41	46,68	9	35,95	55,34
10	53,03	35,61	10	49,21	47,00	10	35,38	55,60
11	53,12	35,97	11	49,00	47,37	11	34,77	55,85
12	53,25	36,33	12	48,69	47,73	12	34,09	56,08
13	53,34	36,71	13	48,36	48,10	13	33,39	56,30
14	53,41	37,10	14	47,98	48,47	14	32,67	56,50
15	53,48	37,51	15	47,58	48,81	15	31,95	56,68
16	53,51	37,93	16	47,13	49,16	16	31,27	56,85
17	53,51	38,34	17	46,64	49,47	17	30,58	56,98
18	53,40	38,76	18	46,17	49,76	18	29,94	57,11
19	53,26	39,17	19	45,72	50,03	19	29,34	57,26
20	53,11	39,55	20	45,31	50,30	20	28,76	57,41
21	52,97	39,92	21	44,94	50,58	21	28,18	57,57
22	52,83	40,27	22	44,56	50,86	22	27,57	57,73
23	52,68	40,61	23	44,20	51,14	23	26,93	57,90
24	52,57	40,95	24	43,84	51,43	24	26,26	58,08
25	52,51	41,29	25	43,46	51,75	25	25,55	58,25
26	52,46	41,64	26	43,03	52,08	26	24,79	58,42
27	52,42	42,01	27	42,54	52,40	27	24,01	58,54
28	52,33	42,40	28	41,99	52,71	28	23,20	58,65
29	52,22	42,79	29	41,41	53,00	29	22,42	58,74
30	52,07	43,20	30	40,81	53,28	30	21,66	58,80
31	51,87	43,61				31	20,94	58,87

1822.								
JANUARY.			FEBRUARY.			MARCH.		
	Æ.	Dec.		Æ.	Dec.		Æ.	Dec.
	0 hrs.	88°		0 hrs.	88°		0 hrs.	88°
1	57′ 20″25	21′ 58″91	1	56′ 58″14	21′ 58″46	1	56′ 42″38	21′ 53″26
2	19,59	58,97	2	57,53	58,38	2	42,03	53,05
3	18,94	59,04	3	56,88	58,30	3	41,64	52,83
4	18,31	59,12	4	56,19	58,22	4	41,20	52,60
5	17,65	59,22	5	55,48	58,11	5	40,73	52,37
6	16,96	59,31	6	54,75	57,98	6	40,26	52,11
7	16,24	59,40	7	54,02	57,84	7	39,85	51,83
8	15,47	59,47	8	53,31	57,67	8	39,45	51,53
9	14,66	59,53	9	52,62	57,49	9	39,10	51,22
10	13,83	59,58	10	51,99	57,28	10	38,79	50,91
11	13,00	59,60	11	51,40	57,08	11	38,52	50,60
12	12,19	59,58	12	50,87	56,88	12	38,34	50,30
13	11,41	59,55	13	50,36	56,70	13	38,17	50,01
14	10,67	59,52	14	49,85	56,52	14	37,99	49,74
15	9,99	59,51	15	49,36	56,35	15	37,79	49,47
16	9,32	59,46	16	48,85	56,20	16	37,56	49,23
17	8,69	59,44	17	48,30	56,03	17	37,35	48,97
18	8,04	59,43	18	47,71	55,87	18	37,10	48,70
19	7,38	59,44	19	47,09	55,69	19	36,83	48,43
20	6,68	59,44	20	46,49	55,48	20	36,55	48,13
21	5,95	59,43	21	45,89	55,26	21	36,29	47,81
22	5,18	59,40	22	45,32	55,01	22	36,11	47,47
23	4,39	59,38	23	44,78	54,76	23	35,97	47,14
24	3,57	59,33	24	44,27	54,50	24	35,88	46,80
25	2,77	59,26	25	43,86	54,22	25	35,83	46,47
26	2,00	59,14	26	43,49	53,96	26	35,81	46,16
27	1,27	59,01	27	43,13	53,71	27	35,85	45,86
28	0,58	58,89	28	42,76	53,48	28	35,88	45,58
29	56′ 59,94	58,77				29	35,87	45,31
30	59,32	58,66				30	35,85	45,05
31	58,73	58,55				31	35,78	44,78

APRIL.

1822.								
APRIL.			MAY.			JUNE.		
	Æ.	Dec.		Æ.	Dec.		Æ.	Dec.
	0 hrs.	88°		0 hrs.	88°		0 hrs.	88°
1	56′ 35″74	21′ 44″49	1	56′ 42″26	21′ 35″65	1	56′ 59″98	21′ 29″89
2	35,69	44,19	2	42,67	35,36	2	57′ 0,76	29,78
3	35,63	43,88	3	43,13	35,08	3	1,54	29,68
4	35,58	43,56	4	43,63	34,82	4	2,30	29,61
5	{ 35,58 35,61	43,22 42,89	5	44,15	34,56	5	3,05	29,56
6	35,76	42,55	6	44,72	34,33	6	3,76	29,52
7	35,94	42,23	7	45,29	34,13	7	4,43	29,47
8	36,15	41,92	8	45,83	33,94	8	5,07	29,41
9	36,35	41,62	9	46,34	33,76	9	5,70	29,33
10	36,55	41,36	10	46,81	33,57	10	6,35	29,26
11	36,76	41,09	11	47,28	33,39	11	7,03	29,18
12	36,95	40,83	12	47,74	33,20	12	7,74	29,09
13	37,09	40,57	13	48,18	33,00	13	8,49	29,01
14	37,19	40,30	14	48,65	32,78	14	9,29	28,93
15	37,28	40,02	15	49,14	32,54	15	10,12	28,88
16	37,44	39,72	16	49,71	32,33	16	10,96	28,85
17	37,63	39,41	17	50,32	32,11	17	11,79	28,85
18	37,84	39,08	18	50,97	31,91	18	12,60	28,86
19	38,10	38,76	19	51,65	31,72	19	13,38	28,87
20	38,40	38,46	20	52,33	31,54	20	14,12	28,91
21	38,78	38,15	21	53,02	31,40	21	14,82	28,94
22	39,18	37,88	22	53,68	31,28	22	15,49	28,95
23	39,59	37,62	23	54,32	31,16	23	16,17	28,96
24	39,97	37,38	24	54,91	31,04	24	16,87	28,95
25	40,32	37,15	25	55,47	30,91	25	17,58	28,95
26	40,68	36,92	26	56,04	30,79	26	18,32	28,94
27	40,99	36,69	27	56,61	30,66	27	19,11	28,94
28	41,29	36,45	28	57,20	30,50	28	19,95	28,94
29	41,58	36,19	29	57,82	30,33	29	20,81	28,96
30	41,88	35,92	30	58,48	30,17	30	21,66	29,02
			31	59,22	30,03			

JULY.

1822.								
JULY.			AUGUST.			SEPTEMBER.		
	Æ.	Dec.		Æ.	Dec.		Æ.	Dec.
	0 hrs.	88°		0 hrs.	88°		0 hrs.	88°
1	57′ 22″50	21′29″10	1	57′45″34	21′33″48	1	58′ 2″97	21′42″01
2	23,32	29,20	2	45,93	33,71	2	3,40	42,30
3	24,10	29,29	3	46,53	33,92	3	3,84	42,59
4	24,84	29,39	4	47,12	34,13	4	4,32	42,90
5	25,54	29,49	5	47,72	34,33	5	4,82	43,21
6	26,22	29,58	6	48,35	34,53	6	5,33	43,55
7	26,90	29,66	7	49,02	34,74	7	5,84	43,90
8	27,59	29,72	8	49,74	34,95	8	6,28	44,28
9	28,31	29,78	9	50,45	35,19	9	6,69	44,67
10	29,06	29,85	10	51,17	35,44	10	7,06	45,05
11	29,86	29,93	11	51,86	35,71	11	7,38	45,43
12	30,69	30,02	12	52,53	35,99	12	7,67	45,80
13	31,51	30,13	13	53,17	36,29	13	7,92	46,16
14	32,37	30,26	14	53,73	36,60	14	8,17	46,50
15	33,18	30,42	15	54,25	36,89	15	8,45	46,84
16	33,96	30,60	16	54,76	37,17	16	8,75	47,17
17	34,69	30,78	17	55,26	37,45	17	9,10	47,50
18	35,39	30,94	18	55,77	37,71	18	9,44	47,84
19	36,05	31,11	19	56,29	37,96	19	9,80	48,19
20	36,69	31,27	20	56,83	38,21	20	10,18	48,56
21	37,34	31,42	21	57,42	38,48	21	10,53	48,95
22	38,00	31,55	22	58,04	38,76	22	10,86	49,35
23	38,70	31,68	23	58,68	39,05	23	11,11	49,76
24	39,43	31,81	24	59,28	39,35	24	11,32	50,16
25	40,19	31,97	25	59,88	39,69	25	11,47	50,57
26	40,98	32,14	26	58′ 0,43	40,04	26	11,60	50,96
27	41,78	32,33	27	0,94	40,38	27	11,73	51,34
28	42,57	32,54	28	1,40	40,72	28	11,84	51,70
29	43,34	32,75	29	1,80	41,06	29	11,96	52,04
30	44,06	33,00	30	2,18	41,39	30	12,12	52,39
31	44,72	33,25	31	2,57	41,70			

OCTO-

1822.								
OCTOBER.			NOVEMBER.			DECEMBER.		
	Æ.	Dec.		Æ.	Dec.		Æ.	Dec.
	0 hrs.	88°		0 hrs.	88°		0 hrs.	88°
1	58′ 12″32	21′52″73	1	58′ 11″99	22′ 4″46	1	58′ 1″04	22′ 14″06
2	12,56	53,09	2	11,82	4,85	2	0,46	14,33
3	12,76	53,46	3	11,61	5,24	3	57′ 59,84	14,61
4	12,97	53,84	4	11,36	5,64	4	59,21	14,86
5	13,17	54,24	5	11,07	6,02	5	58,58	15,10
6	13,33	54,67	6	10,75	6,38	6	57,96	15,31
7	13,45	55,10	7	10,38	6,73	7	57,34	15,50
8	13,47	55,52	8	10,03	7,05	8	56,76	15,69
9	13,46	55,93	9	9,69	7,36	9	56,22	15,88
10	13,42	56,33	10	9,38	7,66	10	55,71	16,08
11	13,40	56,71	11	9,12	7,96	11	55,21	16,30
12	13,39	57,06	12	8,84	8,27	12	54,68	16,52
13	13,35	57,41	13	8,59	8,59	13	54,11	16,74
14	13,36	57,76	14	8,34	8,92	14	53,52	16,98
15	13,41	58,11	15	8,08	9,27	15	52,87	17,21
16	13,49	58,46	16	7,77	9,63	16	52,18	17,43
17	13,59	58,84	17	7,39	9,97	17	51,44	17,61
18	13,62	59,23	18	6,96	10,32	18	50,68	17,78
19	13,63	59,64	19	6,49	10,65	19	49,95	17,93
20	13,60	22′ 0,05	20	6,00	10,97	20	49,24	18,07
21	13,53	0,47	21	5,51	11,28	21	48,56	18,20
22	13,42	0,88	22	5,00	11,55	22	47,90	18,31
23	13,23	1,28	23	4,51	11,82	23	47,28	18,42
24	13,03	1,65	24	4,05	12,07	24	46,67	18,55
25	12,84	2,00	25	3,64	12,33	25	46,07	18,69
26	12,67	2,35	26	3,27	12,60	26	45,46	18,84
27	12,53	2,68	27	2,86	12,87	27	44,80	18,98
28	12,37	3,02	28	2,44	13,15	28	44,12	19,13
29	12,26	3,36	29	2,01	13,44	29	43,39	19,28
30	12,17	3,71	30	1,55	13,75	30	42,62	19,40
31	12,09	4,08				31	41,84	19,51

LXIX. *An*

LXIX. *An Essay on the Reflection, Refraction, and Inflection of Light; on the Colours of thin transparent Plates, and a new Theory of Vision.* *By Captain* FORMAN, *R.N.*

TRUTH is the only legitimate object of philosophical research; and whoever believes his own opinions to be true, and fancies that he can add to the general stock of knowledge by imparting some new discovery, has not only the right, but is bound in duty, to make his opinions known. If, among a number of erroneous opinions, he has afforded but one hint which in the hands of a wiser man may lead to important results, he has conferred a real benefit on society; whilst his errors, though they may outlive his own time, will finally be dispelled by the light of true philosophy, to which his own hint has so materially contributed.

My design in writing this essay is to show the true cause of the reflection and refraction of light; and as Sir Isaac Newton has already accounted for these phænomena by his hypothesis of alternate fits of easy refraction and reflection in the medium, it follows of course that I intend to oppose his opinion; for, if both our opinions coincided, there could be no motive for my writing concerning the cause of these phænomena, and this essay, in that case, would be wholly unnecessary. For this opposition however I shall offer no apology, because I am only exercising a right which he exercised before me; and if all men in all ages had been bound to withhold their opinions whenever they happened to differ from those of great men who had preceded them, we should in a great measure have lost the benefit of Sir Isaac Newton's own discoveries, philosophy would still be in its infancy, and there could be no hope that the mists of ignorance would ever be dispersed.

For a great many ages the history of natural philosophy was little more than a record of errors, every one vieing with another in absurdity and extravagance. Here and there we meet with a transient gleam of true philosophy, like the sun shooting forth his beams through the gloom of a winter's fog, which just served to guide the traveller a few steps further on his way, and then left him to grope in the dark, perhaps for another generation, before any further advance was made in the progress of science. It is chiefly to Lord Bacon that we are indebted for the principle of making experiment the basis of philosophy; and all that he wrote besides, excellent as all his writings are, will bear no comparison to the obligation he has conferred upon science by the establishment of this law. Before his time, the *ipse dixit* of an acknowledged philosopher was sufficient to establish an opinion however absurd: but with this test, like another Hercules, he has cleansed the Augean stable of all its impurities, and no hypothesis now can long hold its ground whose foundation is not laid on the basis of experiment.

Newton was one of the first of the philosophers that conformed entirely to this principle, and made experiment the groundwork of his philosophy; and it is upon this basis that I have grounded all my opinions. I shall propose nothing but what I can prove by analogy to be at least possible, or what I shall show can be more satisfactorily explained upon my principles than in any other way. There can now be no danger of my propagating error, even if my opinions should be wrong, because, in that case, they will not stand the test of experiment; and moreover, where my opinions differ from those which have hitherto been received, if the argument should not be manifestly on my side, prejudice will always be against me, the name of Sir Isaac Newton and the preconceived opinions of all the philosophers will add weight to the opposite scale, and I can hope for no victory but in the confidence of having truth on my side, which, sooner or later, must inevitably succeed.

Rays of light on being emitted from any luminous body are propagated in straight lines; but on the intervention of a medium,—a glass lens for instance,—they are affected in two very remarkable ways: those rays that pass through are refracted, or bent out of their course by a power that we call refraction, while the remainder are turned back by the surfaces of the lens by a power which is called reflection.

I shall show presently that Sir Isaac Newton's hypothesis of alternate refraction and reflection is not borne out by analogy, and that the cause he has assigned for it is not adequate to the effect: but first of all I shall explain what is my own opinion concerning the cause of these phænomena.

As these two phænomena of reflection and refraction are totally dissimilar, it appears to me to be highly probable that they are produced by two distinct causes; that is, that there are two distinct substances in the medium, one of which possesses in its nature the power of reflecting light, and the other that of refracting it. When I speak of a medium however as possessing these properties, I am to be understood with some limitation. I believe glass, water, and every thing else that is called a medium, to be perfectly neutral in these respects, and that the power of reflecting and refracting light resides in a fluid or gas, composed of two distinct substances, that adheres to the surface of these bodies. This I shall prove, first, by showing that these phænomena cannot be explained by supposing that these powers reside in the bodies themselves; secondly, by showing that there is a substance adhering to water and glass—and, by analogy, to every other medium possessing these properties—that does reflect light, because it may be removed; and when it is, the surfaces of these bodies no longer do reflect any light; and lastly, by that part of the atmosphere of Saturn which reflects the light, usually denominated his ring; for it is evidently not his atmosphere that reflects this light, or the whole of it would be equally illuminated; and as it is contrary to all analogy to suppose that any thing could reside on the surface of air but what is lighter than air, the substance that reflects this light can be no other than some such a delicate fluid as I have supposed reflects the light from the surfaces of glass and water, and if it is true in one instance it may be in another.

1. The cause of the reflection of light* must be attributed either to the resistance of a substance that opposes its admission, or to the dislike of the rays to enter a medium where that substance resides. Now if this objectionable substance made a part of the glass, it is evident that the same substance would be found in equal proportions all through, and the rays would be reflected from the interior as well as from the surfaces, which is contrary to fact; but very few would, in that case, be able to get through to the other side, and glass in consequence would reflect a mass of light, as it does now when pounded into dust, but no distinct image of any object, and could be only a semi-transparent body, instead of a clear pellucid substance as it now is.

If it be said that glass, in consequence of some sort of chemical process, by the contact of the air might possess this property only on its surface, I answer, that this is not sufficient to account for the phænomenon, because, as glass reflects light *inwardly* from its further surface, that part of the surface that did so would be inside, and could not be in contact with the air; and therefore, if it were the particles of glass that reflected the rays back again, all the particles throughout would also reflect light, for they all have the same nature; and consequently we can only get rid of the difficulty by supposing that it is not the glass, but a fluid that adheres to it, that has the property of reflecting light.

2. I am now to show that there is a substance adhering to the surfaces of water and glass which may be removed, and that no light is reflected from those parts of the surfaces of these bodies from whence it is removed.

In the *Encyclopædia Britannica*, under the article OPTICS,

* I have separated these two phænomena, and begin with reflection in order to avoid confusion: when the cause of this is fully comprehended, the cause of the refraction of light will be very easily understood.

 and

and in the paragraph No. 45, there is an attempt to prove that bodies may seem to touch when they do not; that is, they rest suspended in the air without any support, and the following is a transcript of so much of it as is necessary for my purpose. " Mr. Melville, on examining the volubility and lustre of drops of rain that lie on the leaves of colewort and some other vegetables, found that the lustre of the drop is produced by the copious reflection of light from the flattened part of its surface contiguous to the plant. He found also that when the drop rolls along a part which has been wetted, it immediately loses all its lustre, the green plant being then seen clearly through it; whereas in the other case it is hardly to be discerned."

Now it does not appear to me to be at all philosophical to suppose that water, which is pulled towards the earth by the power of gravity, could rest suspended in the air unless there was something intervening which prevented its descent; and the more so, as there is here evidently no dislike on the part of the plant or the water to come in contact with each other, for when the plant is dipped in water, water always adheres to it. It is surely more reasonable to conclude, that there is some substance adhering to the surface of water which is sufficiently strong to resist the pressure of the drop when it is small and let down gently*, but which gives way when the drop, by an increase of quantity, becomes more weighty, and also when it comes nearly in contact with other water, because the attraction of the particles of water to each other is stronger than the resistance of this fluid.

But, whatever may be the cause of the separation, it only concerns me to show that there is a substance adhering to the surface of water, and that it is this substance and not the water that has the property of reflecting light. Mr. Melville has shown very clearly† that the drop reflected light while it was separated from the leaf, but that it ceased to do so the moment they were made to touch. Now, if it was the water that reflected this light from the further surface, that surface would remain after the drop had touched the leaf, and be still the same, and therefore it ought still to reflect the same light as it did before: but if there be such a substance as I have supposed between the drop and the leaf when they are separated, that substance must necessarily be removed when they are brought into contact, or they never could touch one another; and as light is only reflected from this

* This phænomenon is more common after dewfall than rain; and this is the cause of the brilliancy of the dew-drop, which so frequently seems to embellish the language of the poets.

† The same phænomenon is exhibited in drops of water sprinkled lightly on dust, paper, and a great many other bodies.

surface

surface while the drop and the leaf are separate, it is evident that there must be a substance between, and that it is this substance, and not the water, that reflects this light.

Persons under water and looking upwards towards the surface will behold the images of objects that are below, reflected downwards from above. Now if water had the property of reflecting light back again into the medium, all the particles throughout would reflect light as well as those that are upon its surface, for all have the same nature, and then there could be no distinct image of any thing, but only a mass of light; and, by the same rule, the power of reflecting light does not belong to the air, for we can only account for the distinctness of the image by supposing that this power is confined to the particles that are immediately on the surface of the water; and no reason can be given why those particles should reflect light any more than the others that are above, except by supposing that they undergo some chemical change or decomposition by coming in contact with the water from which the others are excluded; and in this case it could not be the air itself, but some fluid or gas which makes a part of the composition of air, that has the property of reflecting light; and this is all that I am anxious to maintain.

That there is a similar substance adhering to the surfaces of glass which has the property of reflecting light, may be proved beyond the power of a dispute, because it may be removed by the pressure of the finger, and when removed there is no light reflected from that part of the surface. Glass reflects light from both its surfaces; and whenever we look into glass, of any figure*, we may behold two distinct images of the same objects reflected from the two opposite surfaces. I have now in my hand a plano-convex lens, and on pressing my finger hard upon the spot where the image of any object is reflected from the further surface of it, so much of the image is removed as corresponds with the lines of my finger, whilst in the interstices between the lines, where there can be no pressure, the light is reflected as strong as ever.

Now, if the light from these objects passes through the glass before it is reflected, it cannot possibly be the glass that reflects it back again; and if it had not passed through, the pressure of my finger could not in any way produce the effect I have described. It is evident then that it is not the glass that reflects light inwardly, whatever it may do outwardly; and if what I have already said is not sufficient to satisfy the reader that it is not the air that has this property, Sir Isaac Newton has proved it past all controversy, by showing that glass reflects light from this surface

* When the surfaces are parallel to each other, as in plain glass, the two images enter the eye in the same angle, and then they can only be distinguished by holding the glass obliquely.

in

in an exhausted receiver full as much as it does after the air is let in; and consequently, as light is reflected in the absence* of the air, and after it has passed through glass, the reflection of light, at least in this case, cannot be owing to either of these bodies, but must be produced by some other substance which is independent of both.

There is evidently then a fluid adhering to the surface of glass—and by analogy we may suppose to every other medium producing the same effect—that, whatever connexion it might originally have had with the air, is so far independent of air that it remains attached to the surface of glass in a receiver where the air is exhausted, and it possesses the property of reflecting light, which does not belong to the air in general.

That it is this fluid that reflects light inwardly from the further surface of the medium, is a position that cannot, after these proofs, be disputed with any show of reason; and although we can get at no positive proof of the fact, there is every presumption that it is this fluid also, and not the glass, that reflects light outwardly from the first surface; because, as I before observed, all the particles of glass possess the same nature; and if the particles on the surface of glass reflected the rays of light, those rays that pass through would be reflected by the particles in every succeeding stratum; which is evidently contrary to fact: for it is only by supposing that the reflection of light is confined to the surface of glass that we can account for the image being so distinct. All this difficulty is removed by supposing that it is the substance adhering to glass that produces this effect; and surely no fair reason can be assigned why it should not reflect light one way, when it can be proved by positive fact that it does the other way; and that it does do so, is confirmed by the third class of argument I was to adduce in the phænomenon of Saturn's ring.

3. This luminous ring has excited the attention of the philosopher ever since it was first discovered, and various hypotheses have been formed in order to account for its appearance. Fortunately for my principle, it at once serves to explain and is confirmed by the phænomenon. The only philosophical way to account for phænomena which are out of our reach, is to prove by analogy that such phænomena may be produced by the cause we have assigned; that is, that the same causes do actually produce similar effects in instances that are within the reach of our experiments. Now I have already proved that there is a fluid ad-

* The air cannot be entirely exhausted in a receiver, but it may be made ten thousand times rarer than it was before; and consequently the power of reflecting light, if it belonged to the air, should be ten thousand times less than it was before, and the image reflected by it ten thousand times fainter.

hering to the surfaces of glass and water that has the property of reflecting light, and consequently there can be nothing unreasonable in supposing that the luminous appearance of Saturn's ring is produced by a similar fluid resting on his atmosphere: and this opinion is the more probable, because it coincides in every respect with the known laws of nature; for we observe in our own atmosphere that heavy bodies sink downwards towards the earth, while light ones are lifted up by it; and therefore to suppose that this ring is produced by the reflection of any solid body in that situation, and not by such a light substance as I have imagined, is to suppose what is contrary to all analogy, and what moreover is wholly unnecessary, as it can be proved that a fluid is capable of producing the effect, while no such proof can be adduced in favour of glass or crystal.

If this hypothesis of the cause of the phænomenon of Saturn's ring be admitted—and it is the only one analogy will warrant—the phænomenon confirms all I wish to establish concerning my principle of reflection. Whatever may be the substance that reflects this light, it proves that atmospheric air does not reflect light, because in that case the whole of Saturn's atmosphere would reflect light as well as this substance that reflects his ring; and as the light of the ring is reflected both ways, outwardly as well as inwardly, if this substance be such a fluid as I have supposed, the fluid adhering to the surface of glass may do so also; and then there can be no necessity for the agency of glass or water in the reflection of light, any further than as these bodies attach to themselves the fluid that does reflect light, and all the inconveniences attending that opinion are consequently avoided*.

Having now fully proved that there is a fluid adhering to glass and other mediums that has the property of reflecting light, it may be worth while, before I commence my inquiries concerning the refraction of light, to endeavour to ascertain the cause of its existence. The reader will however understand that what I have to propose is merely conjectural; and if I should be mistaken in my opinion concerning the cause of the existence of this fluid, my mistake cannot in any wise invalidate the principle that such a fluid does exist, because the existence of it has been proved, not upon the mere plea of a probable supposition, but by the

* If the ring of Saturn should be as I have supposed a fluid, and not a fixture, we are provided by it with a means of ascertaining the depth of his atmosphere; for as all bodies with which we are acquainted are subject to the laws of gravity, this fluid could not remain where it is, unless there was some substance immediately below it that prevented its descent, and that substance can be no other than the atmosphere of Saturn. So that by deducting the diameter of Saturn's bulk from the diameter of his ring, and dividing the remainder, the sum remaining will be the amount of the depth of his atmosphere.

evident

evident demonstration of a fact which is as palpable as the pen I am now holding in my hand.

That this fluid has originally some connexion with the air, is extremely probable; because, if it existed beforehand independent of air, it would not require the presence of a medium in order to reflect light. It is reasonable then to suppose that it originally makes a part of the atmospheric air, but that there is some substance in the composition of glass which attracts it more strongly than the air does of which it makes a part; so that whenever glass is exposed to the air for the first time, a decomposition of the particles that are nearest to it immediately takes place (in the same manner as chemists procure the decomposition of other compounds by applying to them some substance that attracts to it one part of the compound, and repels the rest); and, in consequence of this separation, the fluid that attaches itself to the glass, recovers an original property in its nature, that of reflecting light, which it had lost while it made a part of the composition of air. Whether I am right or wrong in this conjecture, it is evident that there is a substance of some sort that attaches itself to glass and other mediums, and that it only does reflect light when it is so attached; and, having proved this, I now come to consider the cause of the refraction of light.

The refraction of light, I conceive, may be explained much in the same way as the reflection of light, with this difference, that one substance repels the rays, and the other refracts them. Glass is a compound of different substances, and the fluid that reflects light may be attracted by one part of the compound, while the refracting substance may be attached to another part. These fluids cannot penetrate the pores of the glass, and therefore necessarily remain upon its surface; and as the substances of which glass is composed are blended together, they in like manner blend themselves on its surface, each fluid being immediately over that particular substance by which it is atttacted; and the rays of light, according as they strike upon either fluid, are reflected back or refracted onwards towards the further surface, where the same phænomena are repeated.

Now, as it has already been proved that there is a substance adhering to the surface of glass that has the property of reflecting light, it is more than probable that the refraction of light is produced by a similar cause; and if no other proof of it could be adduced, the analogy of the two cases would be sufficient to establish its authority. Fortunately however there is no necessity to beg the question, for the proofs I have to advance in favour of this hypothesis are sufficiently strong to establish it without having recourse to a mere probability.

In the first place, if the fluid that reflects light covered the whole

whole of the medium, it is evident that none of the rays of light would be suffered to pass through; and consequently we can only account for the transparency of a medium by supposing that it is only partially covered by this fluid; that is, only those parts of it are covered where the substance is to which this fluid attaches itself. Now, if there was no other substance to fill up those parts on the surface of glass that this fluid did not cover, the surfaces of the fluid would be rather spherical than flat (as water is when it is only sprinkled on a surface, and does not entirely cover it), or, at least, it would be bounded by sides; and in either case would reflect light irregularly, and make the images of objects appear confused. To prevent this, it is necessary that the whole surface of the glass should be covered, that is, that the space left vacant by this fluid should be filled up by the fluid that has the property of refracting light; and then, like all other fluids, it would naturally level itself with the surface of the glass, and consequently would reflect light only in such angles as would represent the images of objects distinct and unconfused.

Here then is a necessity for the existence of such a fluid in addition to the probability: but it is also to be proved by the still stronger evidence of positive fact; for the refracting substance, as well as the reflecting substance, may be removed from the surface of glass, and whenever it is, the power of refraction is visibly destroyed.

It has been supposed by Sir Isaac Newton and others, that when light passes very obliquely to the further surface of glass, none of the rays are suffered to pass through, but that all of them are *reflected* back, so that this surface then puts on the appearance of quicksilver, which it does not in any other instance.

This phænomenon however is not produced by reflection, but by *refraction;* and the philosophers themselves acknowledge so much, although by some strange oversight they have jumbled the two powers together, which in their effects are totally dissimilar (except in this instance, where it so happens that both of them send the rays into the eye by the same angle), and have agreed to call that reflection which by their own arguments is produced by refraction. Maclaurin, in his "Account of Sir Isaac Newton's Discoveries," page 114, in speaking of gravity or attraction, says that "the rays of light on entering a medium are constantly attracted towards the perpendicular, and when they are incident upon the further surface of the glass, with a sufficient obliquity, are all turned back into the glass, though there be no sensible medium behind the glass to reflect it." This phænomenon then, even by their own account, is produced by the rays of light being refracted or turned back by attraction in the medium, and should be called refraction, which is produced by attraction,—and not reflection, which is caused only by repulsion.

Light is reflected from the further surface of glass, as well when the rays strike perpendicularly upon it as when they are oblique: but this extraordinary reflection, as it is called, and which by the way is only observable in a prism, is never visible except when the rays are passing out so obliquely that they would be brought in again by the common law of refraction if there was no reflection at all. It is impossible that the same cause can produce different effects; and it is therefore contrary to every principle of sound philosophy to ascribe this phænomenon to the power of reflection, which involves so many difficulties, when it may be produced by refraction without supposing any difficulty whatever.

It is evident that the same cause must produce the same effect when all the circumstances are similar; and no reason can be assigned why the first surface of glass should not give out as much light as the other, except by supposing a different cause, and that cause can only be refraction. I take it for granted, then, that no one after this will attribute this extraordinary light on the further surface of the prism to any other cause than that of refraction; and admitting this, it is easy to prove that the power of refracting light is in a fluid that adheres to glass, and not in the glass itself, because this fluid may be removed by pressure, and the light, in that case, never makes its appearance. Upon pressing my finger hard upon that part of the surface of the prism that refracts the light into it again, the same phænomenon is exhibited that I before described,—the light is only visible in the interstices between the lines of my finger; and this cannot be explained by supposing that the power of refracting light is in the glass, and not in a fluid that surrounds it, because the glass itself cannot be removed by pressure, and its attraction of light must be just the same whether my finger press upon it or not. Neither can it be inferred that the rays of light are intercepted by the lines of my finger, because, if they can pass through the closer texture of the glass, they certainly can pass between my finger and the glass; and therefore it is impossible to account for the phænomenon, but by supposing that the absence of light, on the pressure of my finger, is caused by the removal of the substance that refracts light in consequence of that pressure; and as the glass itself cannot be removed, it is evident that the substance that has the power of refracting light is not the glass, but a fluid that surrounds it.

If then the facts I have stated be correct, and they are such as may be very easily proved, my hypothesis of the reflection and refraction of light is proved by as strong evidence as can be pro-

duced

duced in proof of the existence of any thing, by analogy, necessity, and matter of fact; it accounts for all the phænomena of light passing through a medium, without involving any difficulty, which cannot be said of any other system; and if further proof should still be required, it is to be found in the utter incapacity of the old hypothesis to produce the phænomena it is intended to explain.

"What kind of action or disposition this is," says Sir Isaac Newton in speaking of his hypothesis of alternate reflection and refraction, "whether it consists of a circulating or a vibrating motion of the ray, or of the medium, or something else, I do not here inquire*. Those who are averse from assenting to any new discoveries but such as they can explain by an hypothesis, may for the present suppose, that as stones by falling upon water put the water into an undulating motion, and all bodies by percussion excite vibrations in the air, so the rays of light by impinging on any refracting or reflecting surface excite vibrations in the refracting or reflecting medium or substance, and by exciting them agitate the solid parts of the refracting or reflecting body, and by agitating them cause the body to grow warm or hot†; that the vibrations thus excited are propagated in the refracting or reflecting medium or substance, much after the manner that vibrations are propagated in the air for causing sound, and move faster than the rays, so as to overtake them; and that when any ray is in that part of the vibration which conspires with its motion, it easily breaks through a refracting surface, but when it is in the contrary part of the vibration which impedes its motion, it is easily reflected; and, by consequence, that every ray is successively disposed to be easily reflected, or easily transmitted, by every vibration which overtakes it. But whether this hypothesis be true or false, I do not here consider. I content myself with the bare discovery that the rays of light are by some cause or other alternately disposed to be reflected or refracted for many vicissitudes."

Now it must be evident to every one who reads this paragraph with attention, that no part of the argument it contains is

* This reasoning, to say the least of it, is vague and unsatisfactory, and can hardly be said to account for the phænomena, when it is uncertain what the action is, whether it be a circulating or a vibrating motion, and whether this motion be in the ray, the medium, or any thing else.

† If the solid parts of a glass lens grew hot in consequence of light passing through, would it not be perceptible to the touch? and after all, what effect can the heated parts of a medium have upon the sun's rays, which are infinitely hotter than they can possibly be? Even if they have any effect, they must alternately grow hot and cold as many times in a moment of time as there are rays of light reflected and refracted in the medium in that time; which is absolutely impossible.

grounded on analogy, or on any acknowledged principle in the laws of nature; it is in fact nothing more than an opinion supporting an opinion, and consequently affords no proof of the truth of the position. Stones falling upon water certainly do cause undulations in the water; but there is no instance of a stone being lifted up by these undulations, and therefore this is no proof that light may be reflected by the undulations of a medium. It is moreover contrary to every principle in nature, to suppose that a medium which suffers one ray to pass through it, should be able to turn the next back, merely because the substance of which it is composed is put in motion by the admission of the first ray,—a circumstance which implies weakness rather than strength,—for the rarer any substance is, the more easily it is put in motion. It is contrary also to every principle, to suppose that the returning vibrations should be so exactly proportioned to the velocity of light, as to cause the rays to turn back in angles that are always equal to the angles of incidence,—a phænomenon which can only be explained by supposing the substance to be stationary that produces the reflection. Neither can any reason be assigned why vibrations, which are excited in the medium by the percussion of the rays of light, should be able to overtake the ray after it has passed through, and increase its velocity; for there is no instance of a stone being overtaken in the water by the vibrations occasioned by its percussion, and the velocity of light is infinitely greater than any thing with which we are acquainted. No reason, again, can be assigned why the reflection of light should be always on the two surfaces, and never happen in the interior of the medium let the medium be of what thickness it may; and not only are all these arguments unsupported by any principle, but the fact itself, even if the principle were true, is mis-stated.

Sir Isaac Newton calls his fits of easy reflection and refraction alternate; says, "this action or disposition intermits and returns by equal intervals," and has given a scale of alternate numbers, 1 3 5 7 9—0 2 4 6 8 10, to show at what distances the ray is disposed to be reflected or transmitted. Now if we look through a pane of glass at any object, the glass hardly intercepts any light at all, at least we do not discover any diminution or faintness in the colours; but the images of objects reflected from glass are always very faint, so much so, that we are obliged to silver our looking-glasses, in order to make them throw out a greater body of light. From this it is evident that at least twenty of the rays must pass through for one that is reflected; and it is surely begging the question, and not argument, to suppose that these vibrations will suffer nineteen of the rays to pass on, and then dispose the twentieth to be easily reflected.

Newton, in all probability, conceived that every part of a medium,

dium, instead of only a certain portion of it, possesses this disposition; and if it was as he supposed, my hypothesis could not account for the reflection and refraction of light; and his, faulty as it is, would be the best that has yet been imagined. It is necessary then for me to explain why the whole of the surface of glass is covered by the image of the reflected object, when only parts of it reflect the rays of light; and fortunately for me this is very easy to be done.

Whenever the table of a camera-obscura is removed further from the lens through which the light is admitted, the images of the objects represented upon it grow larger and become fainter, that is, the rays of light from these objects are more scattered, and occupy a larger space. Still however the rays, scattered as they are, are in contact with each other; for we can discover no break in the picture; and the images, except that they are fainter, are as perfect as before.

The cause of this phænomenon is as apparent as the fact is evident. The particles of light are evidently compressible, because they may be compressed by a burning-glass into a space much smaller than what they usually occupy; and it is only in this way that we can account for the rays of light crossing each other, as they are constantly doing, without meeting any impediment. Now, upon this principle, when the rays enter the eye in great numbers; that is, when they are reflected from a body like quicksilver, that reflects light from every part of its surface, they must necessarily be compressed into a smaller space, and in proportion to their numbers give out a greater body of light; but when they are reflected from glass, whose surface but partially reflects light, a great part of this pressure is consequently taken off, and they expand themselves accordingly, in the same manner as the air does in a receiver when a portion of it is drawn off; so that the retina, as before, is still entirely covered with the particles of light, no breaks are made in the picture, but the images are fainter in proportion to the paucity of the rays.

To suppose that the same substance has the power of reflecting and refracting light, is to suppose what cannot be proved upon the old principle of alternate reflection and refraction, or in any other way, and what moreover is contrary to all analogy, and is founded upon no principle whatever; whereas, it can be no objection to my hypothesis that it supposes that the rays of light are not reflected from every part of the surface of a medium, because it is very evident that they have the power of expanding themselves whenever they are not confined by the pressure of other rays, and there is no other objection with which I am acquainted that can be brought against it. By supposing a fluid adhering to the surface of a medium consisting of two substances, one of which reflects light, and the other refracts it, all the phænomena of light in passing through a medium are explained, and every difficulty vanishes. The rays of light that strike against the reflecting substance are turned back from it, while those that strike upon the refracting substance pass on to the other side, where, according as they strike upon either substance, they are reflected back into the medium or pass out altogether, being subject however at both surfaces to the law of refraction. I have proved that this substance does exist, by showing that it can be removed, and I have anticipated and answered every objection that I can conceive can be brought against it. If any other hypothesis can be produced that will explain these phænomena as satisfactorily as mine has, I am ready to withdraw my pretensions: but if no such hypothesis can be produced, I expect in common justice that mine will be received as the true cause of the reflection and refraction of light.

Having now proved what the substance is that produces the reflection and refraction of light, it may be worth while, before I quit this subject altogether, to make a few observations concerning the nature of these phænomena, or the manner in which they act; premising however, that if what I have to say should not be satisfactory, it cannot affect the truth of my hypothesis, because every other system is equally obnoxious to the same objection, if it is to be considered in that light. The reader will perhaps have observed that the substance in a medium that reflects light, reflects both ways, inwardly as well as outwardly, but that the refracting substance refracts only on the outside; for if it was otherwise, one refraction would counteract the other, and the direction of the ray would not be at all altered. The only way I can account for this, is by supposing that the power of refracting light is not in the substance itself, but in an effluvium that exudes from it, and which always proceeds one way, that is, outside into the air, not inwards towards the glass. This opinion, while it is sufficient to account for the phænomenon, is by no means improbable, for the magnet attracts only at one end, and therefore this substance may do the same. Admitting this, it cannot at all affect my system whether attraction be an effluvium or not; but there is good reason to believe that it is, because it is only in this way that we can account for bodies feeling the influence of attraction when they are at a distance from the attracting body. Refraction then in all probability is caused by an effluvium issuing from the refracting substance, that attracts the rays of light the moment they are within reach of its power, which however barely extends beyond the surface of the refracting substance. Reflection, on the contrary, is best explained without supposing any such effluvium; most likely it is passive

and

and stationary, like a wall, with this difference, that the power of reflection is not so much in the resistance of the fluid as in the dislike of the rays to pass through it.

By my hypothesis, and by no other that I have ever met with, we can satisfactorily account for the double refraction in Iceland crystal, and some other substances, that has excited so much attention and caused so many speculations among the philosophers; for if we can suppose a fluid on the surface of a medium consisting of two substances, each of which possesses distinct properties, there can be no difficulty in adding to it a third substance possessing a property of its own which is different from those of the other two. Almost every refracting medium possesses the power of refraction in a different degree to that of every other medium; water refracts less than glass; and even in glass itself, we find the quantum of refraction is more or less according to its quality, that is, according to the substance of which it is composed. Now it is very possible that Iceland crystal may be composed of two substances which refract the rays of light in different degrees, and which substances, though they are mingled together, are not united, so that the fluid adhering to it is composed of three distinct substances, instead of two as in glass, one of which reflects and the other two refract the rays of light, but in different degrees.

If we admit this, the phænomenon is explained; for the only difference between glass and Iceland crystal is, that as the latter has two refracting substances, and glass only one, it exhibits two images of the same object for every one that is shown in glass; for all the other phænomena that I have ever heard of in Iceland crystal* are merely dependent upon its figure, and glass of the same shape will exhibit the same phænomena, except that there will be only one, two or three images in glass, where there are two, four and six images in the crystal.

The properties peculiar to Iceland crystal, as I understand them, are, that it exhibits sometimes

1. Only one image of an object.
2. Two images of the same object.
3. When the plate is thin, the two images are very close to each other, and their distances increase in proportion to the thickness of the plate.
4. It exhibits six images of one object.
5. When any object is viewed through two pieces of Iceland crystal a little distant from each other, they exhibit but two images in one situation, and four in another.

* I have never seen any Iceland crystal, and consequently have had no opportunity of trying the experiments myself.

6. In

6. In looking at an object through a plate of Iceland crystal, if the plate be moved on its axis, sometimes one of the objects is stationary, while the other appears to move round it, and sometimes the reverse; that is, the image that before was moving is now stationary, and the one that was stationary is in motion.

7. The images of objects seen through double refracting substances are always much fainter than in those mediums which exhibit but one image.

In order to prevent the possibility of a misconstruction, it may be necessary to premise that rays of light proceed in every direction from every visible object, otherwise the same object could not be seen at one time by persons in different situations; so that whenever we look through a glass lens at any object, the whole surface of the glass is covered by rays from that object. But as all these rays are refracted in different angles, according to their angles of incidence and the figure of the glass, a great part of them must pass outside of the eye wherever it is placed; and hence it is that objects appear to magnify as the eye is removed further from the glass when it is convex, and diminish where it is concave; because, by the common law of refraction, those rays that strike upon the extremities of a convex or concave lens are more refracted than those that strike near the centre; and as these rays can only enter the eye when it is removed some distance from the glass, it follows as a natural consequence, that the images of objects must grow larger as the eye recedes from the glass when it is convex, and smaller when it is concave*. Now, as all the rays that strike upon the different parts of a medium are refracted in different angles, only one image can be seen through a single refracting surface, because all the other rays proceeding from the same object are refracted by it outside of the eye; but in a double refracting surface, as there are two refracting substances in it, one of which refracts more than the other, rays from the same object will be refracted into the eye by different angles, from two parts of the same surface, and two images in consequence must be painted on the retina.

With these premises, then, I proceed to explain:

1. Why only one image is exhibited in a double refracting medium.

Iceland crystal in form is a parallelogram with all its angles oblique; its two opposite sides consequently are parallel; and whenever a ray strikes perpendicularly upon the first surface in

* This is the phænomenon which Dr. Berkeley says, in his Theory of Vision, has so much puzzled all the writers on optics, and can be explained by none of their theories: and yet nothing can be more simple upon my principle, which will be evident when I come to show the true principle of vision, which hitherto has not been discovered.

such

such a part of it that it may pass out perpendicularly through the further parallel surface, it is evident there can be no refraction, because there is no angle of incidence, the direction of the ray is in the line of the power of attraction at both surfaces, and therefore cannot be affected by it. It is evident from this, that under these circumstances, a double refracting substance ought not to exhibit two images any more than a single refracting substance; because, if the rays falling but a little oblique upon the surface could be made visible to the eye in one instance, it must in the other, for the law is precisely the same in both cases; and even if it were so, it would not produce a double image, but the one image would be extended over the whole of the retina*, so that the object would not be doubled but only magnified.

2. The second peculiarity of Iceland crystal, its double image, after what I have just said, will be easily explained by a reference to the plate. Let K L M N (Plate V.) represent a piece of common glass, and a ray from the object A striking upon the first surface, and passing on through P by the ordinary reflection into the eye at E. The image of the object then will appear at P; but there will be only one image, because all the other rays from this object will be refracted outside of the eye, and consequently cannot be visible. Now if instead of glass the rays have to pass through a double refracting substance, while the ray R enters the eye from P by the ordinary refraction, the substance possessing the greatest power of refraction will refract the ray *r* down to Q in a greater angle, and from thence into the eye, so that two images of one object must always appear whenever the substance possesses a double refracting power, and the rays from it are oblique and not perpendicular.

The reader will understand that I am not here showing the power of refraction that each substance possesses, but am merely explaining the case.

The reader will understand that I do not pretend, in either of these figures, to give the exact quantum of the power of refraction that each substance possesses; my object is merely to show the principle of double refraction; and as long as there are two refracting substances in the same medium possessing different degrees of power, there must always be two images let the difference of power be what it will.

3. In order to explain the third peculiarity, that the distance between the images increases in proportion to the thickness of the plate, let K L M N represent a thin plate of Iceland crystal, and the rays R *r* refracted into the eye at *e* from the last surface at *p* and *q*, where their distance asunder is very small; but

* This would be the case if the commonly received theory of vision was true, as I shall presently make very evident.

if the thickness of the plate be extended to GH, these rays will have separated very considerably by the time they arrive at P and Q; and supposing the eye to be at E, the ray QQ will not enter it at all, but the rays will be refracted into it from T, and the two images will in consequence appear at P and T. Now, whether we consider the difference in the angles PET and *peq*, or the quantity of space between the images on the two surfaces, it is evident from this, that the distance between the images must always increase in proportion as the thickness of the plate increases.

4. In order to account for there being six images, it is only necessary to observe, that in this case there are three different surfaces for the rays to pass through; that is, some of them pass through the two parallel surfaces KM and LN (fig. 3.) others through KM and MN, and others again through KL and LN; and as there are two images for each surface, it follows of course that there must be six images altogether. Every one knows that the number of images in a multiplying glass is always in proportion to the number of its surfaces; and if I can make it appear that in common glass of this figure and in the same situation we should see three images, there can be no difficulty in understanding why there should be six images in Iceland crystal or any other double refracting substance of the same figure.

For the convenience of measurement, I have made the angles of refraction in this figure to be half the angle of incidence, which I believe is rather more than the ordinary refraction in glass; but if the angles had been less, the images would have been shown just as well,—with only this difference, that the eye must have been removed further from the plate. Let KLMN then represent a plate of common glass, and rays of light from A, striking upon OP and Q, refracted into the eye at E by the common law of refraction, and three images of the same object will appear at RS and T. Now if there was some other substance in the plate that refracted the rays in a greater or less degree, every one of these images would of course be doubled, as I have shown in fig. 1, and the phænomenon consequently is explained.

5. From not considering that the multiplication of the images in Iceland crystal, as in glass, depends upon the number of surfaces the rays have to pass through, and not from any new modification either in the crystal or the rays, it has surprised at least the early writers on optics not a little, that the two images of any object, viewed through Iceland crystal, should not be doubled when these rays have to pass through a second crystal before they enter the eye; and I believe it has never yet been satisfactorily explained, why sometimes there should be four images exhibited by the two crystals and at others only two. "It is wonderful,"

wonderful," says Huygens, (see the Edinburgh Philosophical Journal for January) " why the rays C E and D G, incident from the air upon the lower crystal, should not divide themselves like the first ray A B." In another place he calls it a wonderful phænomenon; and he accounts for there being sometimes only two and at other times four images, by supposing that the *waves* of light " have acquired a certain form or arrangement, in virtue of which, by meeting the texture of the second crystal in one position, they are capable of moving the two different matters which serve for the two kinds of refraction; while, by meeting the second crystal in another position, they have not the power of moving any of these matters. But in what manner," he says, " this happens, I have not been able to form any satisfactory conjecture."

Now this wonderful phænomenon is explained in a moment by considering that the crystals are placed in such a position, with respect to each other, that in one instance, the rays passing through two different surfaces in the lower crystal enter the eye, and consequently exhibit four images, while in another position, the only rays that can enter the eye are those that pass through the two parallel surfaces, and then only two images can be visible. Suppose two plates of glass to be placed, as I have represented them in fig. 4, and rays of light from A to be striking upon the first plate in every direction. Now by the common law of refraction *, the ray *a* would enter the eye at E; but *b c* and *d*, and all other rays from that object would be refracted away from the eye in different angles, and could not possibly enter it while it remained in that position; so that while the plates are placed in that position, with respect to each other and the objects, only one image can enter the eye; and all the difference between Iceland crystal and glass is, that the images are doubled. If the plates however, with respect to the object and the eye, should be placed as I have represented them in fig. 5, the ray *a*, from A, will enter the lower plate at one surface, and the ray *b* at another; and then two images will be visible at E, as I have represented them; and when the crystals possess a double refracting power, by the same rule there will be four.

6. The sixth peculiarity of Iceland crystal, that sometimes one image appears to move, and sometimes the other, when the crystal is moving on its axis, may be explained in this way: Whichever image we fix our eyes upon while the crystal is moving, will always keep one place on the retina, and consequently appear to

* In the two last figures, as well as in the third, I have made the angle of refraction to be half the angle of incidence: the effect, however, except that the eye must be placed in a different position, would be just the same let the angle of refraction be what it may.

be stationary; while the other must change its place on the retina, and of course will appear to be in motion. By the same rule the moon, in stormy weather, very frequently appears to be in motion; whereas, if we fix our eyes steadfastly upon it, we perceive at once that the moon is stationary, and that it is the clouds that are in motion. Whenever we are moving, objects that are motionless change their position with respect to one another; and whichever of two objects we look directly at, will appear to keep its position, and the other to be in motion; and upon the same principle we may account for this phænomenon.

7. The reason why the images of objects appear to be fainter in Iceland crystal than in glass is this: Whenever an object is viewed through a plate of plain glass, there are very near as many rays from that object enter the eye, as there are when the glass is out of the way: but in all double refracting substances, half the rays that would enter the eye under any other circumstances, are bent out by the extraordinary refraction, the rays that do enter the eye are so much the thinner, and the light in consequence is proportionally fainter.

It affords very strong grounds for the presumption that an hypothesis is true, when it accounts for phænomena by principles that are either univerally received, or are known to exist in nature. If pieces of glass, cut in the form of Iceland crystal, will exhibit the phænomena I have supposed in figures 3 and 5, (and if they will not, the principles of refraction laid down by the writers on optics cannot be true,) then the only difference between glass and Iceland crystal is, that the latter possesses a double refracting power which the other has not; and it surely is more philosophical to suppose that this secondary power is owing to a secondary substance, when the existence of a first substance possessing the power of refraction has been fully proved, than to imagine, with Newton, that the particles of light have two sides of different power by which they are differently refracted, according as either side strikes against the medium; or, with Huygens, that the waves of light are capable of moving the matter, which produces the refraction in a medium, when it is in *one* position, without having any such power when it is in *another*. Newton accounted for the phænomenon of double refraction by supposing an original difference in the rays of light* by [illegible] of which "some rays are constantly refracted after the usual manner, and others constantly after the unusual manner." But if the phænomenon of double refraction depended solely upon this difference of properties in the rays, the phænomenon would be exhibited in glass as well as in Iceland crystal, and in neither of them

* He asks (Query 26) Have not the rays of light several sides, endued with several original properties?

with

with any regularity, because it must depend upon the mere accident of the rays coming down with one or the other side foremost. The phænomenon could not be constant in Iceland crystal, and never in glass, if the cause did not in great measure reside in the crystal; and then there can be no necessity to suppose any such difference in the properties of light; for, if we admit that there is one substance in Iceland crystal that has the power of turning the rays round, and another that has not (and we can only account for the phænomenon upon his principle by supposing this), there can be no difficulty in supposing that there are two distinct substances in the crystal possessing a greater and less power of refraction; and as the rays strike upon one or other of these substances, they are more or less refracted.

Huygens accounted for this phænomenon in a very different way: Light, according to him, instead of being an effluvium of particles propagated in straight lines from a luminous body, and proceeding in all directions, consists in undulations of an ethereal medium. "He supposes the ordinary refraction to be produced by *spherical* undulations, propagated through the crystal, while the extraordinary refraction arises from *spheroidal* undulations*." Here I must confess that I have never read any of the arguments by which this theory of undulations is supported; and therefore I do not pretend to controvert them: but as I cannot form in my mind any idea by which it can account for the phænomena of light, at least, without the violation of some of the known laws of nature, I shall propose certain objections to it, which, if the hypothesis be founded upon true principles, can very easily be answered: if they cannot, it must be evident that it rests upon no other authority than the fancies of a lively imagination, and will bear no comparison with an hypothesis that is founded upon principles which are known to exist in nature, and which, as far as I have met with them, is capable in itself of explaining all the phænomena of light, without borrowing from any other system.

I shall say nothing about these undulations of light being spherical and spheroidal, just as it suits the convenience of the theorist, because I do not know in what way Huygens accounted for it; but it certainly is not saying much for a system, that the inventor of it can only explain himself by the use of principles which he denies to be in existence. Huygens, in describing the phænomena of light passing through Iceland crystal, does not say a word about undulations, but speaks of the *rays*, being divided and variously affected by the crystal, in the same terms that Sir Isaac Newton would have used; and yet, immediately afterwards, it appears that these rays have no existence, but that

* See Edinburgh Philosophical Journal.

light

light consists in undulations of an ethereal medium*. It may be said, perhaps, that Huygens made use of these terms in condescension to the want of capacity in ordinary readers, who would not have understood him had he explained himself in any other way: but a philosophy which can be comprehended only by a few, is rather metaphysical than solid; it can answer no general purpose of usefulness, and, in all probability, is not founded upon any solid principles.

By supposing light to consist of separate particles, it is easy to conceive how they may cross each other, as they are continually doing; but there is no analogy in nature to support the opinion that undulations of a fluid can cross each other without one or the other, or both, being thrown into confusion. If we throw two stones into water at a little distance from each other, two circles of undulations are produced, proceeding from two centres; but the moment they meet they are broken and thrown into confusion, and the undulations only proceed where there is no interruption.

If it is difficult to conceive how it is possible for undulations to cross each other without being broken, it is also equally difficult to conceive how these undulations can pass through a medium without being separated into particles, and then the hypothesis is destroyed: a stone thrown into water easily passes through, because the particles of water give way to the greater force of gravity by which it is impelled; and a particle of light may do the same: but it is impossible for a fluid to pass through another, without one or the other being broken and dispersed; and the water evidently is not, or we should see it in commotion. If we hold a tumbler upside down and immerse it in water, we see that the air still remains at the bottom of it, and cannot escape through the water: And is it not, then, too much to suppose that any other fluid can pass through, without either being thrown into confusion or separated into particles?

I can readily conceive that particles of light and colour, entering the eye in different directions, may impinge on the retina distinct figures and colours, and that the mind may distinguish these figures and colours by the sense of seeing, as it is commonly understood, and not, as the philosophers will have it, by vibrations on the nerve: but I can imagine nothing but confusion, when I am told that light is nothing more than undulations of an ethereal

* See the Edinburgh Philosophical Journal. In another part of it we read, "The phænomena of inflection are considered by M. Fresnel to be inexplicable on the Newtonian theory of the emission of luminous particles; while almost all of them may be directly deduced by the Huygenian theory of *undulations*." I am fully persuaded myself, and I think I shall be able to prove it, that inflection is a property that does *not* belong to light.

medium;

medium; nor can I conceive how it is possible for these waves to give the mind any distinct idea of figure or colour, either by the sense of seeing, or, as it seems to be the mode, by the sense of feeling.

LXX. *Catalogue of Ancient Eclipses, with the Dates of their corresponding Eclipses at one and two Periods Distance. With Remarks.* By Mr. THOMAS YEATES.

[Continued from p. 347.]

To Mr. Tilloch.

SIR, IN my Remarks on the Ancient Eclipses, page 348 of your Magazine, I have suggested the idea of the whole ecliptic phænomena being comprehended in the space of 912 solar years, and have produced some convincing examples that the hypothesis is not improbable: these examples are submitted with deference to such of your readers as are practitioners in these calculations, and who are disposed to listen to useful discussion.

In the next place I propose to show, that not only *the same eclipses* return at the expiration of one or more complete periods, but also the same *series of eclipses* returns in true succession. This being a very important point to examine, it constitutes the subject of this paper: I take it for granted, that the great astronomical year commencing with the Nabonassarean æra is a true epoch for computing the Julian equation according to Rem. XI. page 345, and that the dates of those eclipses subsequent to that period must be corrected on that principle.

The manner proceeded on is simply this; 1st. The first column gives the historical dates as per table, Phil. Mag. p. 244. 2d. The second column shows the equation to be added according to the rate of 11 minutes per annum from the aforesaid epoch. 3d. The third column shows the equated or true time of the said eclipses; and, 4th. The fourth column shows the corresponding dates. The Julian equation at the rate of seven days for one period, and 14 days for two periods; less by four days for one, and eight days for two periods; which leaves six days for two periods to be added for dates preceding the Gregorian and New style, is added afterwards for want of room.

Before Christ.		Dates of Eclipses.				Julian Equation.			+ Equated Time.				Corr. Eclip.
			D.	H.	M.	D.	H.	M.		D.	H.	M.	A.D.
721	☽	March	19	10	34	0	4	46	March	19	14	20	1104
720	☽	March	8	11	56	0	4	57	March	8	16	53	1105
720	☽	Sept.	1	10	18	0	4	57	Sept.	1	15	15	1105

621

Before Christ.		Dates of Eclipses. D.	H.	M.	Julian Equation. D.	H.	M.	+ Equated Time.	D.	H.	M.	Corr. Eclip. A.D.
621	☽ April	21	18	22	0	23	6	April	22	17	28	1204
523	☽ July	16	12	47	1	17	6	July	18	5	53	1302
502	☽ Nov.	19	12	21	1	20	55	Nov.	21	9	6	1323
491	☽ April	25	12	12	1	22	54	April	27	11	6	1334
431	⊙ August	3	6	35	2	9	54	August	6	16	29	1394
425	☽ Oct.	9	6	45	2	11	0	Oct.	11	17	45	1400
424	⊙ March	20	20	17	2	11	11	March	23	7	28	1401
413	☽ August	27	10	15	2	13	12	August	29	23	27	1412
406	☽ April	15	8	50	2	14	31	April	17	23	21	1419
404	⊙ Sept.	2	21	12	2	14	53	Sept.	5	12	5	1421
403	⊙ August	28	5	53	2	15	4	August	30	20	57	1422
394	⊙ August	13	22	17	2	16	43	August	16	15	0	1431
383	☽ Dec.	22	19	6	2	22	44	Dec.	25	17	50	1442
382	☽ June	18	8	54	2	22	55	June	21	7	49	1443
382	☽ Dec.	12	10	21	2	22	55	Dec.	15	9	16	1443
364	⊙ July	12	23	51	2	22	13	July	15	22	4	1461
357	⊙ Feb.	28	22	0	2	23	30	Mar.	3	21	30	1468
357	☽ August	29	7	29	2	23	30	Sept.	1	6	59	1468
340	⊙ Sept.	14	18	0	3	2	33	Sept.	17	20	33	1485
331	☽ Sept.	20	20	29	3	3	6	Sept.	23	23	35	1494
310	⊙ August	14	20	5	3	8	7	August	18	4	12	1515
219	☽ March	19	14	5	4	0	48	March	23	14	53	1606
218	☽ Sept.	1	rising		4	0	59	Sept.	5	0	59	1607
217	⊙ Feb.	11	1	57	4	1	1	Feb.	15	2	58	1608
203	⊙ May	6	2	52	4	3	44	May	10	6	36	1622
202	⊙ Oct.	18	22	24	4	3	55	Oct.	23	2	19	1623
201	☽ Sept.	22	7	14	4	4	6	Sept.	26	11	20	1624
200	☽ March	19	13	9	4	4	17	March	23	17	26	1625
200	☽ Sept.	11	14	48	4	4	17	Sept.	15	18	25	1625
198	⊙ August	6	0	0	4	4	39	August	10	4	39	1627
190	⊙ March	13	18	0	4	6	7	March	18	0	7	1635
188	⊙ July	16	20	38	4	6	29	July	21	3	7	1637
174	☽ April	30	14	33	4	9	0	May	3	23	33	1651
168	☽ June	21	8	2	4	10	6	June	25	18	8	1657
141	☽ Jan.	27	10	8	4	15	6	Feb.	1	1	14	1684
104	⊙ July	18	22	0	4	21	53	July	23	19	53	1721
63	☽ Oct.	27	6	22	5	2	4	Nov.	2	8	26	1762
60	⊙ March	16	rising		5	2	37	March	21	0	37	1765
54	⊙ May	9	3	41	5	3	43	May	14	7	24	1771
51	⊙ March	7	2	12	5	4	16	March	12	6	28	1774
48	☽ Jan.	18	10	0	5	4	49	Jan.	23	14	49	1777
45	☽ Nov.	6	4	0	5	5	52	Nov.	11	9	52	1780
36	⊙ May	19	3	52	5	10	21	May	24	10	21	1789

Before

Before Christ.		Date of Eclipses. D.	H.	M.	Julian Equation. D.	H.	M.	+ Equated Time.	D.	H.	M.	Corr. Eclip. A.D.
31	☉ Aug.	20	setting		5	11	16	August	25	12	0	1794
29	☉ Jan.	5	4	2	5	11	38	Jan.	10	15	40	1796
28	☉ June	18	23	48	5	11	49	June	23	23	48	1797
26	☉ Oct.	23	4	16	5	12	10	Oct.	28	16	16	1799
24	☉ April	7	4	11	5	12	33	April	12	16	44	1801
16	☉ Nov.	1	5	13	5	14	1	Nov.	7	19	14	1809
2	☉ Feb.	1	20	8	5	16	35	Feb.	7	12	43	1823

In comparing the above historical dates with the equated times, the calendar of the ancients appears to have differed upwards of five whole days from the astronomical time in the space of about 750 years. The dates of the corresponding eclipses from A.D. 1104 to A.D. 1823 are of three denominations in the Tables: 1. Such as are set down prior to the correction of the Gregorian calendar. 2. Such as are registered or computed according to that calendar: and, 3. Such as come under the New style. Those set down in the tables according to the New style very nearly agree with the equated dates; this is manifest by inspection: those set down according to the Gregorian style do not so well correspond; which may be attributed perhaps to the defect of that calendar: but those eclipses set down prior to the year 1582 require the equation of two whole periods to be added, because they were recorded before any such correction took place: which will discover two things necessary to be inquired into; first, whether the solar equation allowed for each period is correct; and, secondly, whether those eclipses have been registered by the same or different calendars.

A. D.		Date of Eclipses. D.	H.	Days.	Equated Time.	D.	H.	D.	H.		Corr. Eclip. A.C.
1104	☽ Mar.	13	3	A.+6	Mar.	19	3 A.	19	10	34	721
1204	☽ April	15	12	39+6	April	21	12 39	21	18	22	621
1302	☽ July	10	4	A.+6	July	16	4 A.	16	12	47	523
1323	☽ Nov.	13	2	A.+6	Nov.	19	2 A.	19	12	21	502
1334	☽ April	19	10	33+6	April	25	10 33	25	12	12	491
1394	☉ July	28	2	A.+6	Aug.	3	2 4	3	6	35	431
1400	☽ Oct.	3	2	A.+6	Oct.	9	2 A.	9	6	45	425
1401	☉ Mar.	15	2	M.+6	Mar.	21	2 M.	20	20	17	424
1412	☽ Aug.	22	6	M.+6	Aug.	28	6 M.	27	10	15	413
1419	☽ April	10	8	M.+6	April	16	8 M.	15	8	50	406
1421	☉ Aug.	28	9	M.+6	Sept.	3	2 4	2	21	12	404
1422	☽ Aug.	2	11	A.+6	Aug.	8	11 A.	28	5	23	403
1431	☉ Aug.	8	4	M.+6	Aug.	14	4 M.	13	22	17	394
1442	☽ Dec.	17	3	59+6	Dec.	23	3 59	22	19	6	383
1443	☽ June	12	2	A.+6	June	18	2 A.	18	8	54	382

A.D.	Date of Eclipses.	Days.	Equated Time. D. H.	D. H.	Corr. Eclip. A.C.
1443 ☽	Dec. 7 6 M.	+6	Dec. 13 6 M.	12 10 21	382
1461 ⊙	July 7 10 M.	+6	July 13 10 M.	12 23 51	364
1468 ☽	Aug. 4 2 M.	+6	Aug. 10 2 M.		357
1485 ⊙	Sept. 9 2 M.	+6	Sept. 15 2 M.	14 18 0	340
1494 ☽	Sept. 14 19 45	+6	Sept. 20 19 45	20 20 29	331
1515 ⊙	Aug. 9 9 A.	+6	Aug. 15 9 A.	14 20 5	310

From these examples it appears that the assumed equation above given very nearly reduces the dates to the same day, and time of the day within a few hours, although it must be remarked the distance of time is no less than 1824 years. To proceed on any nicer calculation would be prolix, but not unprofitable in a work devoted to the subject of the lunar astronomy.

Again: If you add to the above equated dates the Julian equation, these equated dates will correspond with the equated dates of those eclipses which happened for seven hundred years before the Christian æra, and the result will amount to the same measure of time, or very nearly: I shall produce an example of this sort.

		D.	H.	M.			D.	H.	M.
(1.) Before Christ	331 ☽ Sept.	20	20	29	A.D. 1494 ☽ Sept.		14	19	45
	Equation +	3	3	6		+	6	0	
						Sept.	20	19	45
						+	3	3	6
						Sept.	23	22	51
	Eq. time	23	23	35		Eq. time	23	23	35

(2.) Eclipse A.C. 331	Table time Sept.	20	20	29
Eclipse A.D. 1494	Table time Sept.	14	19	45
	Equation +	6	0	0
	Sept.	20	19	45
(3.) Eclipse A.C. 331	Equated time Sept.	23	23	35
Eclipse A.D. 1494	Equated time Sept.	23	22	51
	Diff.			44

Having now produced the principles upon which the eclipses of the ancients may be verified without the tedious method of calculation by tables and anomalies, and reduced their dates by a most simple method to a common standard, agreeing with modern observations, I proceed on other remarks.

The year A.D. 1801 appears to have been remarkable for the places of the sun and moon in the autumnal equinox, and an *epoch*

epoch most convenient for calculating the lunar periods and cycles to any extent. The place of the moon's ascending node and apogee in September of that year coincided with points in the heavens not occurring during many ages, at least for upwards of nine centuries.

In 912 solar years there are 48 lunar cycles of 19 years, and exactly 49 revolutions of the nodes: therefore if you divide this period by 48, the precise measure of one cycle is obtained; and if you divide thé same period by 49, you will ascertain the period of one revolution of the nodes. The whole of these calculations depend upon the just quantity of the solar year, any augmentation or diminution of which, beyond or short of its true and absolute measure, affects all the lunar calculations founded on it. A difference of one minute in the quantity of a solar year will amount to 15 hours 12 minutes for a whole period, and a difference of but one second in a lunation will amount to three hours in that space of time; a difference of eight seconds will amount to one day; and one hour and a half in a cycle will amount to three days.

In 912 solar years there are 228 bissextiles and seven intercalary days, in all 235 days; and in one lunar cycle are $12 \times 19 = 228$ lunations, and seven intercalary; in all 235 lunations: also in a lunar cycle are 940 changes of the moon, and in 912 solar years are 940 lunations: this harmony really subsists, and is confirmed by every evidence of observation.

The lunar cycle is computed at 19 solar years, and constitutes one fundamental principle of the Paschal Tables published in all editions of the book of Common Prayer. The fractional quantities belong to the pure astronomy, and which can only affect the ecclesiastical and calendar reckoning in a lapse of some centuries. This cycle comprehends a regular series of eclipses; and when expired, a new series begins, with a variation which requires a whole period to exhaust; so that forty-eight such cycles must elapse before such period terminates. This arises from the motion of the moon's ecliptic points or nodes, whose revolutions produce these admirable variations.

The precise measure of this cycle is variously estimated, but the following method will nearly approximate to the truth.

Days.	H.	M.		Days.	H.	M.	
365	0	0	× 19 =	6935	0	0	
	6	0	× 19 =	4	18	0	
				6939	18	0	Julian time
	0	11	× 19 –		3	29	
				6939	14	31	Solar time.

 There-

Therefore 19 solar years contain 6939 days 14 hours 31 minutes, at the rate of 11 minutes per annum for the Julian excess; and by deducting three whole days in 912 years the moon completes her course within one hour and a half of the sun, and consequently the cycle contains 6939 days 13 hours one minute, which makes a difference of three whole days in 912 solar years.

If the moon anticipates the sun three days in 912 solar years, it is evident the moon's motion must be completed in less time than the sun, and therefore the quantity of one hour and a half must be deducted from the solar time as above stated: but the question is, whether this equation for the lunar anticipation is any real element in the lunar astronomy; or whether it may not be accounted for on the unknown measures of the moon's true motions. The concurrence of so many examples of eclipses of the sun and moon at fixed periods, stated in the preceding remarks, affords so many convincing testimonies for the coincidence of their motions, as seems to exclude the idea, that any positive anticipation of either the sun or moon really exists; and for this plain and self-evident fact, that every eclipse is caused by a unison and concurrence of their motions.

The true measure of a lunar cycle of 19 years may, it is presumed, be best determined from a comparative statement of the motions of the sun and moon with the elements of each at that interval, as computed in the British Nautical Almanack, which is the highest authority to be consulted in so exact a determination.

			1801.		1820.
			D. H. M.		D. H. M.
Sept. 21	☽	Full moon	21 19 24	☽	21 18 48
21	☽	Eclipse	21 19 12	☽	21 18 41
		⊙ Longitude.			
			S. D. M. S.		S. D. M. S.
21			5 27 59 17		5 28 22 34
22			5 28 58 2		5 29 21 19
		⊙ Right ascension.			
21			11 52 37 0		11 54 2 5
22			11 56 12 6		11 57 38 1
		⊙ Declination north.			
			D. M. S.		D. M. S.
21			0 48 4		0 38 48
22			0 24 41		0 15 24
23			0 1 16		0 8 0 s.

☽ Longi-

		1801.				1820.			
		☽ Longitude.							
		S.	D.	M.	S.	S.	D.	M.	S.
Sept. 21	Noon	11	17	13	35	11	17	14	23
	Midn.	11	24	23	28	11	24	50	25
22	Noon	0	1	29	37	0	2	26	18
	Midn.	0	8	31	28	0	10	0	49

☽ Latitude.

		D.	M.	S.		D.	M.	S.	
21	Noon	1	10	2	s.	0	31	28	s.
	Midn.	0	30	46	s.	0	10	45	N.
22	Noon	0	8	38	N.	0	52	44	N.
	Midn.	0	47	30	N.	1	33	39	N.

☽ Right ascension.

		D.	M.	D.	M.
21	Noon	348	43	348	28
	Midn.	355	3	355	11
22	Noon	1	19	1	53
	Midn.	7	31	8	35

☽ Declination.

		D.	M.		D.	M.	
21	Noon	6	8	s.	5	32	s.
	Midn.	2	42	s.	1	53	s.
22	Noon	0	44	N.	1	47	N.
	Midn.	4	7	N.	5	24	N.

☽ Semidiam.

		M.	S.	M.	S.
21	Noon	16	18	16	44
	Midn.	16	14	16	45
22	Noon	16	10	16	44
	Midn.	16	4	16	42

☽ Hor. Paral. and Propor. Log.

		M.	S.		M.	S.	
21	Noon	59	49	4784	61	20	4676
	Midn.	59	35	4801	61	21	4675
22	Noon	59	18	4822	61	18	4678

Place of Moon's node.

	S.	D.	M.		S.	D.	M.
19	0	0	4	Sept. 19	11	22	33
25	11	29	45	25	11	22	14

Meridian passage.

	H.	M.		H.	M.
21	11	45	Sept. 21	11	44
22	12	33		12	36

[To be continued.]

LXXI. *On*

LXXI. *On the Distribution of the Magnetic Fluids in Masses of Iron: and on the Deviations which they produce in Compasses placed within their Influence.* By Mr. Charles Bonnycastle.

It having been observed by modern navigators, that the iron which enters so largely into the construction and equipment of their vessels, is capable of producing a very sensible effect upon the needle; much attention has lately been turned towards explaining and correcting the error thus produced. With this view, Mr. Barlow of the Royal Military Academy, Woolwich, has, not long since, published a Memoir, containing the results of a numerous set of experiments, made principally upon spheres of iron: and a few pages on this subject have also been written by Dr. Young, and printed by order of the Board of Longitude.

The latter of these productions, from its nature and the assumptions on which its investigations are founded, can only be considered as affording a probable approximation to the truth; and the former, independently of its impeaching in some degree the received theory of magnetism, is professedly experimental; and for this reason requires some further evidence to prove that the conclusions of its author, drawn as they are from a restricted case, may be safely employed under the variously modified actions which arise from the irregularity of the ferruginous masses contained in a ship, and the alterations in the intensities and directions of the magnetic fluids, which take place with every change of position on the surface of the globe.

To remove these difficulties, and to investigate from the theory of Coulomb the magnetic attractions of masses of iron of all figures, and for all places, is the object of the following pages: in which if I have succeeded, I flatter myself I shall have added something to the stability of the theory I have employed, by showing how ready an explanation it affords of many magnetic phænomena, to the investigation of which it has hitherto been applied without success*.

The principle upon which it is my intention to found this inquiry, is an extension of the law that regulates the action of elec-

* Mr. Barlow, although led by his results to entertain some doubts of Coulomb's theory, has observed, "If therefore, when the mass of iron is great, and the distance at which it acts considerable, the laws which I have developed should be found to be the necessary consequence of the hypothesis to which I have alluded, (Coulomb's,) the agreement will furnish one of the best proofs that has yet been given of the accuracy of the deductions upon which that hypothesis is founded; and, I should hope, without detracting in any manner from the value of the experimental results detailed in the foregoing pages of this work.—(Essay on Magnetic Attractions, page 121.)

trified

trified bodies upon conductors; which was first given by M. Poisson in the Memoirs of the Institute for 1811, and employed by him to determine the development of the electric fluids in spheres that mutually act on each other; an investigation to which, I believe, it has been heretofore entirely restricted.

Confining ourselves in this place to magnetic forces, the law which I have here alluded to may be briefly deduced as follows:

Conceive every particle of the given mass of iron, as containing equal quantities of the magnetic fluids; it is then manifest that they will, by their mutual actions, neutralize each other; and the magnetism, thus reduced to a latent state, will be incapable of producing any external effect by which its presence might be detected.

But on the approach of a magnet, this state of equilibrium is immediately disturbed; the different fluids are repulsed or attracted according as the predominant pole is of the same or a contrary denomination; the liberated fluids in their turn cause a further displacement; and a series of these successive actions will go on until the developed magnetism is so distributed that its action upon any particle is equal, and opposite, to that of the disturbing magnet.

In the case we are considering, the disturbing force is the latent magnetism of the earth; which, from the distance of its poles, may be considered as acting in parallel lines, and with equal forces on all the particles of the mass: hence the condition which we have deduced for the equilibrium of the fluids may be enunciated as follows:

If the latent magnetism of a mass of iron be developed by the action of the earth, it will recede to the surface, and will there form a very thin shell, whose nature is such, that its action in a given direction is always equal to a certain constant quantity.

To determine from this law the thickness of the shell at any required point, is, in most instances, a problem that seems nearly to surpass the powers of analysis: when however the given solid is a sphere, a case which is immediately connected with our present inquiry, the solution may readily be obtained as follows:

Let A N B S (fig. 6) be the given sphere, of which the centre is in O; through O draw N S in the direction of the dip, and conceive an equal sphere A N′ B S′ to have its centre at O′; which is in the line N S, and at an infinitely little distance from O.

Then the force with which any particle p, which is common to the two spheres, is urged towards O, by the attraction of the sphere A N B S, will vary as p O; and towards O′, by the attraction of A N′ B S′, will vary as p O′. Resolve this last force into the two $p\,i$, i O′, at right angles to each other, and conceive

ceive the sphere A N′ B S′ to be subtracted from the sphere A N B S; then the attractions which will act upon p will be $-Oi$ and $-iO'$.

But the attraction of the sphere A N B S, minus the attraction of the sphere A N′ B S′, is the same as the attraction of the solid A N B N′ A, together with the repulsion of the solid A S B S′A. Hence, if the space occupied by the first of these solids be filled with a magnetic fluid of either denomination, and that occupied by the second with the opposite; the effect of their actions upon the magnetism, in any particle p of the sphere, will be as iO and $O'i$.

Now let the force with which the magnetic action of the earth attracts p be pe; resolve this into the two pk and ke; of which pk is in the direction pO, and ke is at right angles to it: also pk, ke are opposite and proportional to $o'i$, iO; it will therefore follow that, if the forces which solicit p destroy each other, they will also destroy each other for every other particle of the sphere: or the fluids will be distributed according to the condition required.

This result may be readily expressed analytically. Take any point n in the surface of the sphere; join nO, and let it cut the interior surface of the shell in m. Then if we join mO', it will be equal to mi; but nO is equal to mO'; therefore mn is equal to Oi: or putting ϕ for the angle NOn, T for the polar thickness, and t for the thickness at the point n; we derive the formula $$t = T . \cos\phi.$$

The law which regulates the equilibrium of the magnetic fluids in spheroids, may also be readily deduced from a similar mode of reasoning to that which we have employed for spheres.

Thus let N A S B (fig. 7) be the given spheroid, whose major axis N S is in the direction of the dip. Conceive also N′A S′B to be a similar and equal spheroid, whose centre O′ is in the line N S, and at an infinitely little distance from O. Then it is manifest that the magnetic fluids when they are in equilibrio, will be bounded by the surfaces N A N′B N and S A S′B S. For the attraction of any particle p will vary as pm and pn, which are respectively perpendicular to the minor and major axes (Maclaurin's Fluxions, No. 634; or Phil. Trans. 1809): and as this is true for each spheroid, the difference of their attractions will be zero for the direction pn, and the constant quantity mm' for the direction pm.

The same solution also extends to spheroids, when the dip is not in the direction of either of the axes. For making the construction in fig. 8, where N S is the direction of the dip; the difference of the attractions pm, pm' is mm'; or drawing mh parallel and hp perpendicular to N S, the difference of these attractions

attractions will be as $m i$ and $h k = (i m')$; which, putting the angle N O C equal a, is as cos a and sin a. In the same way it appears that the difference of the attractions $p n$, $p n'$ is as cos a and $-$ sin a: the attractions therefore perpendicular to N S destroy each other; whilst in the direction of that line they are equal to a constant quantity; and as N S is the direction of the dip, it is manifest that this distribution of the fluid is agreeable to the law that we have laid down.

Let ϕ be the angle which the conjugate diameter of any point in the surface makes with the meridian; t the thickness of the shell at the given point; and T the polar thickness. Then from the foregoing demonstration we readily derive the following analytical expression for the thickness of the shell at any point of the surface, viz. $t = T. \sin \phi$.

It would be foreign to our present purpose to enter any further into the investigation of particular cases; all that we have proposed to investigate may be derived either from what has preceded, or from the following general theorem, which it will be our next object to demonstrate.

In any mass of iron, whether regular or irregular, if t_1, t_2, t_3 *be the respective thicknesses of the magnetic fluid at any point, when the dip is directed according to three rectangular co-ordinates* a_1, a_2, a_3; *then the thickness* t *at the same point, when the dip is in a direction that makes with* a_1, a_2, a_3 *the angles* a_1, a_2, *and* a_3, *will be expressed by the formula*

$$t = t_1 \cos a_1 + t_2 \cos a_2 + t_3 \cos a_3.$$

The truth of this theorem may be readily established; for, conceiving the force in the direction of the dip to be unity, the forces in the directions a_1, a_2 and a_3 will be respectively cos a_1, cos a_2, and cos a_3. But it is manifest that the thickness at any point is, *ceteris paribus*, as the disturbing force; and it therefore follows, that if the force in the direction a_1 were alone concerned, the thickness at the given point would be t_1 cos a_1: moreover, the fluid distributed according to this law would produce an attraction in the direction a_1, equal to cos a_1; whilst at right angles to that direction its effect would become zero.

Hence, as a similar observation applies to the distributions of the fluids by the actions of the forces cos a_2 and cos a_3, we may infer that if these three developments had place at the same time, the effects they would produce in the directions a_1, a_2 and a_3 would be respectively cos a_1, cos a_2 and cos a_3. But the forces cos a_1,

cos

$\cos \alpha_2$ and $\cos a_3$ acting in the directions of the axes a_1, a_2 and a_3 are equivalent to a force represented by unity, and acting in the direction we have assumed for that of the dip. Whence we conclude that the distribution of the fluid assigned to any point t by the theorem, is such as would produce a force equal and opposite to that of the terrestrial magnetism; and consequently such as will fulfil the conditions to which we have arrived in page 447.

Having thus deduced the laws which regulate the accumulation of the magnetic fluids in masses of iron of all forms; it will now be merely an affair of analysis to determine the deviations which their attractions are capable of producing upon compasses whose situations are known with respect to the given body.

When the given body is of an irregular form, the deviations produced can only be obtained by knowing the attractions for three other directions of the dip: but if the mass be either spherical or spheroidal, the effect may be calculated by a direct process. Thus; let AGDS (fig. 9) be the given sphere; where O is the centre; N S the direction of the dip; and O′ the centre of the second sphere (page 447-8); let also C be the place of the compass, and join C O, C O′; the first of which cuts the sphere in some point P; join N P, A P; and produce the latter to meet the horizontal circle G H P in H, through which point draw O C′ meeting the vertical C C′ in C′: then the necessary construction will be finished by drawing in the vertical plane the axe OA, and the rectangular axes O G, O D in the plane of the horizon, OG being also parallel to the magnetic meridian.

From what has been proved in page 447-8, it will then follow that the attraction of the sphere to the compass at C will be the result of a positive and a negative force, that vary respectively as $\frac{1}{CO^2}$ and $\frac{1}{CO'^2}$. Or resolving the last into the rectangular forces Ca, aO', and assuming $CO = d$, $NOP = \phi$, and $OO' = e$; the attraction in the direction C O will vary as

$$\frac{1}{d^2} - \frac{d + e\cos\phi}{\left\{d^2 + e^2 + 2ed\cos\phi\right\}^{\frac{3}{2}}}.$$

And in the direction O a, as

$$\frac{e.\sin\phi}{\left\{d^2 + e^2 + 2ed\cos\phi\right\}^{\frac{3}{2}}}$$

But since e is infinitely small, these expressions will reduce to the two following respectively,

$$\frac{2e.\cos\phi}{d^3} \quad \ldots\ldots\ldots\ldots \quad (1)$$

$$\frac{e.\sin\phi}{d^3} \quad \ldots\ldots\ldots\ldots \quad (2)$$

It

In this instance it will be convenient to transform the latter of these expressions into another which shall represent the force in the direction O N: this will readily be effected by adding to it a force equal to $\frac{e. \cos \varphi}{d^3}$, acting in the direction a C, and then subtracting the same quantity from the first expression, that the effect upon the whole may not be altered. We shall then have for the forces in the directions C O and O N respectively,

$$\frac{3e. \cos \varphi}{d^3} \quad \ldots \ldots \ldots \ldots \quad (3)$$

$$\frac{e}{d^3} \quad \ldots \ldots \ldots \ldots \quad (4)$$

To estimate the effect of these forces upon the compass, it will be necessary to reduce them to the horizontal plane; for which purpose put G H $= i$; H P $= k$; and G N $= d$. Then the forces in the respective directions G O, D O will be represented by

$$\frac{e}{d^3} \left\{ 3 \cos k \cos i \cos \phi - \cos d \right\} \quad \ldots \ldots \quad (5)$$

$$\frac{3e}{d^3}. \sin i \cos k \cos \varphi \quad \ldots \ldots \ldots \quad (6)$$

Where, if i and k are known, φ may be found by the formula

$$\text{Cos}\ \varphi = \sin d. \sin k + \cos d. \cos k. \cos i.$$

Let δ be the deflection caused in the direction of the needle by the action of these forces: then since the force by which the terrestrial magnetism endeavours to restore it to the meridian varies as $\sin \delta$; we shall have the following equation:

$$m. \sin \delta = \frac{e}{d^3} \left\{ 3 \cos k. \sin i. \cos \varphi. \cos \delta - (3 \cos k. \cos i. \cos \varphi - \cos d) \sin \delta \right\}$$

Whence by reduction

$$\tan \delta = \text{A}. \frac{\sin i. \cos k. \cos \varphi.}{d^3 + a \left\{ 3 \cos k. \cos i. \cos \varphi - \cos d \right\}} \quad \ldots \ldots \quad (7)$$

This is the rigorous formula that expresses the relation between the place of the compass with regard to the sphere, and the consequent deviation produced in the direction of its needle: when however δ is small, an approximate expression may be found by neglecting, in the right hand number of the first of the above equations, the coefficient of $\sin \delta$, and substituting in the place of $\sin i. \cos k$, their equals $\sin \phi. \cos l$; where l is the distance of the meridian N P measured from D, on a great circle at right angles to N S. Our expression (7) will then become

$$\tan \delta = \text{A} \frac{\cos l. \sin 2\varphi}{2d^3} \quad \ldots \ldots \ldots \quad (8)$$

which is the formula derived by Mr. Barlow in his Essay on Magnetic Attractions.

To determine the deviations produced when a dipping needle is substituted for the compass at C, it will be merely necessary to resolve the forces (3) and (4) into two others which are in the meridian, and respectively parallel and perpendicular to the direction of the dip.

But the force (3) when estimated in the direction AO is equivalent to

$$\frac{3e. \cos\phi. \sin k}{d^3}.$$

And when estimated in the direction GO is equivalent to

$$\frac{3e. \cos\phi. \cos k. \cos i}{d^3}.$$

Therefore the total force in the direction at right angles to NO is as

$$\frac{3e}{d^3} \cos\phi \left\{ . \sin k. \cos d + \cos k. \cos i. \sin d \right\}.$$

And in the direction NO the force is evidently as

$$\frac{e}{d^3}. \left\{ 3\cos^2\phi - 1 \right\}.$$

Hence we have for the equation that determines the deviation δ'

$$m'. \sin\delta' = \frac{e}{d^3}. \left\{ \cos\phi\,(\sin k. \cos d + \cos k. \cos i. \sin d). \cos\delta' - (3\cos^2\phi - 1)\sin\delta'). \right\}$$

Or reducing as before,

$$\tan\delta' = A'. \frac{\cos\phi. (\sin k. \cos d + \cos k. \cos i. \sin d)}{d^3 + d'. (3\cos^2\phi - 1)}.$$

Or substituting for $(\sin k. \cos d + \cos k. \cos i. \sin d)$ its equal $\sin\phi. \sin l$,

$$\tan\delta' = \frac{A'}{2}. \frac{\sin l. \sin 2\phi}{d^3 + d' (3\cos^2\phi - 1)} \quad \ldots \ldots \ldots \ldots \quad (9)$$

From which we may derive, as before, the approximate expression

$$\tan\delta' = \frac{A'}{2}. \frac{\sin l. \sin 2\phi}{d^3} \quad \ldots \ldots \ldots \ldots \quad (10)$$

Having given the deviations in the horizontal plane, those in the vertical may be deduced very readily from them; for $A = \frac{A'}{\cos d}$; whence by substituting in (8) and (9), and equating the results,

$$\tan\delta' = \tan\delta. \cos d. \frac{\sin l}{\cos l} = \tan\delta. \cos d. \tan l.$$

As the dip at London is 70° 30′ this equation will become for that place,

$$\tan\delta' = .3338 \tan l. \tan\delta.$$

From which expression we learn that δ' will be less than δ from $l = o$ to $l = 71° 32'$. And as we know from our expressions (8) and (9) that the maximum value of δ is to that of δ' as 1 to 3333; we may conclude that the deviations produced in the vertical

vertical plane are much less than those in the horizontal; a circumstance that was noted by Mr. Barlow, although that gentleman has not given the law by which the former of these actions is regulated*.

Many curious corollaries may be deduced from the formulæ given in the preceding pages. Thus from our expressions (8) and (9) it may be shown that there are *three planes of no attraction in every sphere;* one of which has reference to the dipping needle; one to the horizontal compass; and one is common to both: these planes are manifestly determined by making $\cos l = o$; $\sin l = o$; and $\sin 2\phi = o$; and are therefore respectively parallel to the meridian; at right angles to the meridian; and at right angles to the dip.

Were the needle acted on by no other attraction than that of the sphere, its position would be such that

$$\tan \delta' = 2 \cos \phi;$$

where δ' in this case represents the dip. This formula follows immediately from the equations (1) and (2); for from them we have

$$2. \cos \phi : \sin \phi :: 1 : \cos \delta'.$$
$$\cos \delta' = \tfrac{1}{2} \tan \phi.$$
$$\tan \delta' = 2 \cot \phi.$$

The law here given is that which Dr. Young has assumed, as regulating the position of the needle on the earth's surface: it was first deduced by a foreign mathematician, from a much more complicated expression of Biot's: and it is not a little singular that that philosopher should have missed it, considering how very readily it follows from the principles he has employed.

I shall conclude this part of the subject with a deduction that follows from our preceding investigations; which serves strongly to mark the analogy between the natures of magnetism and electricity; and is at the same time attended with rather a paradoxical circumstance. I allude to the fact, that *the quantity of magnetic fluid developed in a sphere or spheroid, is as the surface; and is therefore the same within certain limits, for spheroidal or spherical shells, as for the whole solids; but that nevertheless the attraction to those solids varies as the cubes of their diameters* †.

* The relations here deduced between A and A′, δ and δ', must be considered as relating only to the approximative expressions 8 and 10; and not to the rigorous formulæ 7 and 9.

† A view of the subject something similar to what I have here given, confirmed by some experiments on the attractions of iron plates, had led me to perceive the first part of this law, about the same time that Mr. Barlow discovered the second. The two facts appeared so completely opposed, that I found it difficult to persuade that gentleman (unable as I then was to account for the anomaly) of the truth of my deduction: an accidental experiment has since however convinced him of the fact.

The

The same law probably obtains for other solids, but nothing that we have yet demonstrated will enable us to draw so general a conclusion: that it is true for spheres and spheroids, may be demonstrated as follows. Since we have seen (page 447-8) that the development of the magnetic fluids which produces on any particle p of the sphere an attraction equal and contrary to that of the terrestrial magnetism, takes place at the surface; it follows that if we were to take away all that interior portion of the mass which is bounded by a concentric sphere, having the radius p O, the equilibrium of the fluids in p would still be maintained; and the distribution and consequent attraction of the magnetism in the shell thus produced, would be the same as for the whole sphere.

Since moreover the attraction of each of the equal spheres A N B S, A N′ B S′, varies as either of their masses; the difference of their attractions, which is proportional to the magnetic attraction of the first sphere, must also vary as the same mass, or as the cube of the diameter.

The same demonstration will evidently extend to spheroids.

Hence, to complete the investigation which we have proposed to ourselves, it will now be necessary to examine the attractions of irregular bodies; which may be effected by the following theorem, analogous in its nature and demonstration to that given in page 449.

If a, a′, a″ *be three rectangular co-axes; and* a_0, a_1, a_2 *be the attractions of any mass of iron according to each of these axes respectively, when the dip is in the direction* a; a_0', a_1', a_2' *the same attractions when the dip is in the direction* a′; *and* a_0'', a_1'', a_2'' *when the dip is in the direction* a″: *Then will*

$$a_0 \cos a + a_0' . \cos a' + a_0'' \cos a''$$
$$a_1 \cos a + a_1' \cos a' + a_1'' \cos a''$$
$$a_2 \cos a + a_2' \cos a' + a_2'' \cos a''$$

be the attractions according to each of the axes a, a′, a″ *respectively, when the dip makes with them the respective angles* a, a′ *and* a″.

This theorem may be readily demonstrated, as we have observed above, from that in page 449. For since we there learn that the thickness of the shell of magnetism arising from the new position of the dip, is the aggregate of the thicknesses in its former directions, multiplied respectively by the cosines of the angles made with those directions; we conclude that the part of the magnetic force which acts in the direction a, will produce the several attractions a_0, cos a, a_1 cos a and a_2 cos a, which act according to the axes a, a' a'' respectively; that the attractions in the same directions

directions arising from the part of the force which is parallel to a', will be a_0' cos a', a_1' cos a', and a_2' cos a'; and so on: whence the theorem is evident.

If it is wished to apply this theorem to practice, it will be convenient to assume the axes a and a' in the horizontal plane; in this case assuming e for the angle made by the projection of the needle in that plane with the axe a, we shall have $\cos e . \sin a'' = \cos a$; and $\sin e . \sin a'' = \cos a'$; whence it follows that the horizontal forces, estimated at right angles to the deflected needle, are respectively,

$$\left\{ a_0 \cos e + a_0' . \sin e + a_0'' \cot a'' \right\} . \sin a'' . \sin (e - \delta)$$

$$\left\{ a_1 \cos e + a_1' \sin e + a_1'' \cot a'' \right\} . \sin a'' . \cos (e - \delta).$$

Whence may be readily derived an expression for the deviation δ; which however for want of room I must omit. We may however observe, that this expression applied to the experiment detailed in page 60 of the Essay on Magnetic Attractions, will show very clearly the reason of the results there obtained agreeing so nearly with those deduced from the formula employed, (which is the same as our 8th,) although that expression is not generally applicable to the attractions of irregular masses.

From the preceding expression we are also enabled to calculate what would be the deviation produced by the attraction of the iron in a vessel, at any place, and with the ship's head directed to any point of the compass; provided we know the intensity of magnetism; and the angle of the dip at that place; and the effect produced by the same mass, in three different positions of the vessel's head, at any other place.

But one of the principal uses of the last theorem will be found to consist in the easy demonstration it affords of Mr. Barlow's very simple practical method of determining the attraction of the iron in ships; a point which it is of much importance to establish, and the more so as its truth has hitherto been allowed to rest wholly on analogy; the only attempt at proof being derived from the manifest fact, that both the iron in the vessel, and the plate employed to ascertain its effect, must have a resultant of force: but no sufficient evidence has been adduced to show that the resultants of these differently-shaped masses will continue the same for all directions of the meridian and dip*; a point which was indispensably requisite, (in that mode of demonstration,) to prove

* That no sufficient proof is derived from the experiment with the twenty-four pounder we have already seen; and the example of the attractions on board the Isabella is manifestly too restricted; since the dip, which was from 74 to 86, could be but little affected by a motion in the horizontal plane.

the

the efficacy of the method. The truth of the latter, however, follows immediately from our theorem; for since the attraction of any mass of iron, whose parts are similar and equal on each side the meridian, passing through the compass, can be found in all positions, the compass and mass retaining their relative situations, from the attractions in any three; it follows, that if we can find a mass of iron whose effect shall double that of the ship in three directions of the vessel's head; it will also, *cæteris paribus*, double it for every other position of the vessel; either with regard to the points of the compass, or the situation on the globe.

It was my intention to have added some further applications of the method I have followed in this inquiry; but I have already surpassed the bounds to which I had proposed to restrict myself: and I can therefore only add my sincere wish that the hitherto greatly neglected subject of magnetism may continue to attract the attention which it has lately received from Government; and by which alone we can hope to see explained many of those complicated phænomena of variation that, from the widely extended observations they require, would perhaps for ever baffle the most arduous exertions of individual talent.

Woolwich, June 13, 1820. CHARLES BONNYCASTLE.

LXXI. *Facts respecting the comparative Strength of Chain Cables on the Construction of Capt.* SAMUEL BROWN, *and the Patent Chain Cable of* THOMAS BRUNTON, *Esq.; and of Iron Cables compared with Hempen Cables: as elicited on the Trial of the Cause* BRUNTON *against* HAWKS *and others, before the Lord Chief Justice* ABBOTT *and a Special Jury, 25th May* 1820.

THIS was an action for infringing a patent granted to the plaintiff in the year 1813, for certain improvements in the construction, making, or manufacturing of ships' anchors and windlasses, and chain-cables, or moorings. The cause was of considerable importance, and occupied the attention of the Court for upwards of five hours. The case for the plaintiff was opened with great ability by Mr. Scarlett, and was confined to an infringement of one part only of the patent, namely, the chain-cables: but it was contended by the Solicitor-General for the defendants, that, as the patent was not for one, but three articles, if any one of them were not new at the time of obtaining the patent, or not useful, the patent would be void.

The Judge was not disposed to admit this: it was possible that the oldness or inutility of any one of the three articles embraced in a grant, might render the grant void as to that one, without destroying

destroying the right to the other two; and therefore his Lordship would reserve the point of law on this head, if defendants insisted on entering into any other question than the infringement complained of, should the jury not find for the plaintiff upon this point. It was finally so settled; the defendants insisting on inquiring into the novelty of plaintiff's mode of manufacturing ships' anchors. They admitted the novelty of the windlass.

On the part of the plaintiff, the utility of the chain cable was proved by Captain Johnston, the acting manager of the house of Messrs. Buckles and Co. eminent ship-owners in the city, who stated, that all the ships belonging to their house were furnished with the patent chain-cables and anchors; that the hempen cables used frequently to give way, the iron ones never; and that in a voyage to the West Indies their ships used to break on an average two of the common anchors every voyage—sometimes three; but that they have never yet broken one of the patent anchors. The actual infringement of the patent was then proved by a gentleman who bought a chain cable from defendants, impressed or marked with the name of Hawks on every one of the links, a piece of which was exhibited to the Jury.

Dr. Gregory and Mr. Barlow (of the Royal Military Academy, Woolwich), Mr. Brunel and Mr. Donkin (both eminent engineers), gave evidence to the perspicuity of the specification, and the scientific merits of the chain-cable and anchor. The improvements in the former they stated to be, the placing the whole material of the links in the same sectional plane, the introducing a broad-ended stay into each link to prevent the sides from collapsing when exposed to a strain; the making the ends of this stay remain in their place by lapping upon and surrounding a considerable portion of the opposite curved parts of the opposite sides of the link, and the making those parts of each link which interpose between the central stay and the neighbouring interposed links at each end, as straight as possible, that they might not be solicited to change their figure when exposed to a strain—every change of the figure being apt to injure the fibre of the iron: and these improvements were pointed out as being manifest and self-evident, by comparing the patent chain-cable with the chain-cables before in use, which were manufactured by Captain Brown.—In these cables the links had stays, but instead of being broad-ended, they were sharp-pointed; and instead of lapping round and embracing the opposite inner sides of the link, were secured in the links by holes punched in these to receive the pointed ends of the pins or stays—a mode of combining them that tended to weaken the link and endanger its rupture, by breaking it over the points of the stay as fulcrums, when exposed to a strain; and besides, instead of presenting the substance of the link in the same sectional plane, Capt. Brown's links are made of a twisted form, for the avowed purpose of giving them elasticity—a form extremely injurious to the strength of the link, as the first effort of tension must be to draw the link out of its twist, and bring those portions of it which lie between the points of strain into straight lines.

John Knowles, Esq. secretary to the surveyors of the navy, gave a most interesting detail of experiments made by order of the Navy-office, to ascertain the comparative merits of Brown's chain-cables and those of the patentee. In every experiment, Brown's twisted links, with sharp-ended stays, were ruptured and broken, while the plaintiff's remained uninjured. In consequence, orders were issued that no more chain-cables on Brown's construction should be admitted for the service of the navy; but only and exclusively, chain-cables with broad-ended stays in the links, and the links having their substance in the same plane.

Mr. Knowles read the minutes of the experiments alluded to, and which were signed by the commissioners of the navy, as follows:

"At a trial of chain and hempen cables on the 23d of May, 1816, at the manufactory of Messrs. Brunton and Co., in the Commercial Road. Present:

The Comptroller of the navy.

Commissioners. { Sir H. Peake. Seppings. Bowen. Fraser. Cunningham.

And the following were the results:—

Experiment 1. "Chain-cables of 1½ inch diameter* manufactured from the same iron on Capt. Brown's twisted principle, and on Messrs. Brunton, Middleton, and Company's plan, were attached together. Capt. Brown's chain measured 36 feet, Messrs. Brunton and Company's 33 feet. At a strain of 50 tons the former had stretched 12 inches, the latter 6. On a strain of 60 tons having been applied, the former had stretched 24 inches, and the latter 12. Capt. Brown's was broken in the body of the iron by a strain of 65 tons; the iron was of a very good quality: the stays of the twisted chain were generally crooked."

Experiment 2. "Capt. Brown's 1⅝ inch chain, 36 feet long, was attached to Messrs. Brunton and Company's 1½ inch that had before been tried [in Exper. 1.]. Capt. Brown's was broken by a strain of 76 tons, a second link was fractured, and most of the stays crooked."

Lord Chief Justice Abbott.—"The other stood? A. "Yes, my lord."

* That is the diameter of the iron of which the links were made.

Expe-

Experiment 3. "A 12 inch hempen cable was attached to a length of chain-cables on Messrs. Brunton and Company's principle, of the respective diameters $\frac{7}{8}$ of an inch, 1 inch, $1\frac{1}{8}$, $1\frac{1}{4}$, $1\frac{3}{8}$, $1\frac{1}{2}$, and $1\frac{5}{8}$ in diameter, in order to determine which sized chain was equal in strength to the hempen cable. A strain of 28 tons broke the $\frac{7}{8}$ chain. The hempen cable was measured after a strain of 25 tons had been applied; it had stretched 12 inches in 10 feet. The hempen cable was broken by a strain of 30 tons."

Experiment 4. "A 24 inch hempen cable was attached to a $1\frac{3}{4}$ inch iron cable of Capt. Brown's, and a $1\frac{1}{2}$ inch iron cable of Messrs. Brunton and Company's manufacture. After a strain of 30 tons the cable measured 22 inches: when 76 tons had been applied, it was reduced in circumference: the cable was broken in the nip at 91 tons."

Experiment 5. "A length of Capt. Brown's twisted chain of $1\frac{7}{8}$ inch, that he had supplied to Deptford yard, was attached to one of $1\frac{1}{2}$ inch manufactured by Messrs. Brunton and Company. The link of Capt. Brown's was broken by a strain of 64 tons. It was again tried, and another link broke at 70 tons. The iron was found to be of a bad quality."

"After the first experiment, Capt. Brown doubted the accuracy of the results given by the hydraulic engine, [the testing machine is made on the ingenious principle of Mr. Bramah's hydraulic press] imagining that the strain was nearly one-half more than the balances indicated: but the truth of the engine appears to have been proved by the strain on the 12 inch hempen cable; as it bore, according to that machine, about 2 tons more than was expected."

Witness.—"I have also the trials of the hempen cables with Mr. Brunton's, made in order to ascertain what they would bear, and thereby to be better able to apportion the chain cables to be substituted for them."

Mr. Solicitor General (for the defendants).—"We do not mean to offer any evidence that these chains are not stronger than Brown's twisted chains."

Mr. Scarlett.—"What are the experiments as to the hempen cables?"

The witness attended previously at Messrs. Brunton and Company's manufactory, between the 23d of May and the 17th of June 1816, to see that the ropes were properly stretched, without any member of the board; but the trial took place on the 17th of June. Present:

Commissioners. { Sir H. Peake.
J. Tucker.
P. Fraser. }

Experiment 1. "A 14 inch cable was tried against chains of

1, $1\frac{1}{8}$, $1\frac{1}{4}$, $1\frac{3}{8}$, $1\frac{1}{2}$, $1\frac{5}{8}$, $1\frac{3}{4}$, and $1\frac{7}{8}$ inches: at a strain of 40 tons the cable was reduced to 12 inches: the strain of 40 tons was continued, and the cable was broken."

Experiment 2.—"A 16 inch cable was attached to the chains; the rope was reduced at 40 tons to $13\frac{1}{2}$ inches: the 1 inch chain was broken by a strain of 44 tons."

Experiment 3.—"The cable was then attached to the chain from $1\frac{1}{8}$ to $1\frac{7}{8}$ inch inclusive: the cable was broken at 47 tons."

Experiment 4.—"The chains were then put to an 18 inch cable, and a further piece of chain of $1\frac{1}{4}$ inch was attached. At 55 tons the rope was reduced to $16\frac{1}{2}$ inches, the $1\frac{1}{8}$ inch chain was broken at a strain of 55 tons: the cable, which then measured $15\frac{1}{4}$ inches, was affixed to the other remaining chains, and was broken by a strain of 59 tons."

Experiment 5.—"A 20 inch cable was tried. The chain of $1\frac{1}{4}$ inch broke at a strain of 62 tons. Upon affixing the cables again, a piece of $1\frac{3}{8}$ inch chain broke at a strain of 66 tons: the iron of which it was made was good, but very malleable.

"When 71 tons had been applied, the cable was reduced to $17\frac{1}{4}$ inches. At a strain of 75 tons the second piece of $1\frac{1}{4}$ inch chain was broken; the cable was reduced in size to 17 inches, and was broken at a strain of 76 tons; the smallest size of chain in the machine being $1\frac{3}{8}$ inch."

Experiment 6.—"A 24 inch cable was put into the machine. The $1\frac{3}{8}$ inch chain was broken at a strain of 80 tons: the rope was then attached to the other chains: when 92 tons had been applied, the cable measured 21 inches; when 105 tons were put upon it, the cable had evidently suffered very much, and measured $20\frac{3}{4}$ inches: it was broken with a continuance of the strain of 105 tons."

Lord Chief Justice.—"And the chain remained?" A. "Yes, my lord."

"As it was considered that the 18 inch cable had not borne a sufficient strain (comparatively with the other sizes) to draw any accurate conclusion of the strength of ropes of this size, a duplicate of the 18 inch cable was put into the machine, and was broken by a strain of $63\frac{1}{2}$ tons, which is about the force that it was considered should break an 18 inch cable."

Mr. Scarlett.—"And the result was an order for the chains of Mr. Brunton?"—"Yes; by sharing it between him and Brown."

"The twisted cable was consequently dismissed?"—"Yes."

"And has been used since?"—"Those in store were necessarily used, but they were not further ordered."

Cross-examined by Mr. Gaselee.

"These experiments were against the twisted chains only?—"Yes; Capt. Brown requested, in a letter of the 4th of April 1816, 1816, that not only the triangular chain but the parallel chain should be tried against them."

"Brunton's was the angular?"—"Yes."

"And Capt. Brown desired some experiments to be made between the elliptical and angular chains?"—"Yes."

"Did he (Brunton) admit that the plan of Brown's was strongest?"—"No; he admitted that it was as strong, and it would be only trying the excellency of the workmanship and the iron against workmanship and iron."

"Mr. Brunton admitted that the elliptical was as strong as the angular form, supposing they were made the same way, and not that the elliptical link with the pointed stay was as strong?"—"No; that was not spoken of. Capt. Brown wished to have manufactured chains for the purpose of trial, with links that had their sides nearly as a straight line, considering that the more it deviated it became the weaker. Brown wished it to be done; but the Navy Board thought it unnecessary to enter into it, as it was admitted to be as strong."

The plaintiff next called Mr. Bramah; but the Judge here intimated, that already the evidence for the plaintiff appeared so plain, that he should like to learn from defendants' counsel what answer they could give to so strong a case, without now taking up the time of the court in examining more witnesses for plaintiff, till it should be seen, from the nature of the defence, whether it would be necessary?—in which event, he would not preclude defendant from calling more witnesses.

The counsel for plaintiff said, they had many scientific men yet to be examined, if necessary*; but had no objection to what had been recommended by his lordship.

Defendants' counsel contended, that neither the chain-cable nor the anchor were new. Chains having oval links, very similar in form to those described by the patentee, had been in use for ages, therefore he could have no exclusive right to the use of such chains; and Capt. Brown had used stays, therefore he could have no exclusive right to them. It was also stated, that evidence would be offered to prove that links having their substance in the same plane, and having what might be called broad-ended stays (though not so broad, nor precisely of the form of plaintiff's stay), had been in use long before the date of the patent.

* We have seldom or never seen in any cause such a phalanx of scientific and practical men as were subpœnaed for the plaintiff. Besides those already mentioned, we observed among them, Professor Millington, of the Royal Institution, Messrs. Mawdeslay, Wilson Lowry, Ostell, Galloway, Tilloch, Keir, Collinge, John Farey jun., Hawkins, Gill, Joseph Farey, T. Jones (instrument-maker to the Board of Longitude), and others; besides Master ship-builders, Sea Captains, and people versant with naval matters, but whose names we could not learn.

Mr.

Mr. Solicitor General.—"That there is a great resemblance between our links [Hawks's] and fig. 5. [Brunton's] is because it resembles the common oval link. We are not precluded, I submit, from the use of the oval link, because he [the patentee] has specified something like it?"

Lord Chief Justice.—"Unless you have evidence to show that the broad-headed stay is either old or useless, they will most likely succeed.—Any evidence you can call to show that it is not the invention of the plaintiff, or useless, will be of importance."

Mr. Jessop was called for defendants. He was of opinion that a little curvature in the sides, was rather better than having them straight between the points of strain, as making it better able to stand the effect of a sudden jerk. But on a question from Mr. Scarlett, whether the iron cable by which a ship rides, could be brought into a straight line, so as to be jerked? he stated his meaning to be, "that mere positive strength applied to iron does not apply to the same material when used against a jerk."

A link of a chain, not of the form of plaintiff's, but having stays with ends that might be called comparatively broad, and secured by lapping round the inner part of the link where interposed, like the plaintiff's, was now put in, and sworn to by one of Mr. Rennie's workmen, as having been (or rather similar links) made and used some months before the date of the patent: but on cross-examining the witness, it was proved that they were not made for a chain, properly so called; that only four of them were made in all; and that they were applied to the joining of two bolts in some building.

Some old men (nail-makers) were called to prove, that among parcels of old iron, which they were in the habit of working up into nails, they had frequently, many years ago, met with pieces of chain having stays in them, which were not sharp-pointed; and some persons belonging to the arsenals were brought to prove the recent sale of similar pieces of chain among the old stores. Various pieces of the kind of chain alluded to were produced and sworn to by the witnesses: but it turned out on examining them, that the stays did not lap round the substance of the link, leaving the substance unhurt, but, on the contrary, were retained in their places by being received into notches chiselled out of the substance of the link; and it was also proved, even by defendants' witnesses, that the stays were not put into the links to strengthen them against a strain, but merely to prevent the chain from *hinking*—that is, two or more of the links getting entangled; and that they were made solely for pump chains.

Defendants' counsel next put in what is commonly well known by the name of a mushroom-anchor, and another known by the name of the adze-anchor, both having their names from their respective forms. The head of the former, instead of having arms and

and flukes, has a large round head, shaped like a dish, or a portion of a sphere: the latter is like a cooper's adze. They are both made of cast iron, with a shank of malleable iron riveted into them: this the counsel insisted was the same method as that described by the plaintiff, and therefore his anchors were not new.

The answer was, That these were not the kind of anchors claimed or described by the patentee, whose anchors had arms and flukes, and were described as *ship's* anchors, which these were not. It was proved, even by the witnesses for defendants, that these anchors were employed only for *stationary* moorings —and not for ships' anchors used on voyages.

It was now put to the jury, whether there was any necessity for the plaintiff's counsel to take up their time? and they stated, that they thought it not necessary, as they were perfectly masters of the whole case.

The Chief Justice now addressed the jury. It was not necessary that he should recapitulate the whole evidence respecting the particular infringement complained of, as the plaintiff's counsel had not been called upon to address them. He wished, however, in giving their verdict respecting the infringement on the chain-cable, to be also informed by them, whether they found the anchor to be new and beneficial?

The jury found for the plaintiff upon all the points: namely, that the specification was sufficient, the chain-cable and anchor new and beneficial, and that the defendants had infringed the chain-cable. Nominal damages alone were sought for in this action, the question of *Right* being all that was to be tried, as the defendants were to account, under a previous suit in Chancery, for the profits they had made by the use of the invention. The jury accordingly awarded One Shilling damages, besides costs of suit.

The verdict seemed to give very general satisfaction to a crowded court.

LXXIII. *Notices respecting New Books.*

Lately published,

Travels in Sicily, Greece and Albania, illustrated with numerous Engravings. By the Rev. T. S. Hughes, Fellow of Emanuel College, Cambridge.

An Essay on Involution and Evolution; containing a new, accurate and general Method of ascertaining the numerical Value of any Function of an unknown Quantity; particularly applied to the operation of extracting the Roots of Equations, pure or adfected. With an Appendix. By Peter Nicholson. 8vo.

New

New Method of solving Equations with ease and expedition: by which the true value of the unknown Quantity is found without previous reduction. With a Supplement, containing two other Methods of solving Equations, derived from the same principle. By Theophilus Holdred. 4to.

Preparing for Publication.

Travels in Syria and Mount Sinai; consisting of, 1. A Journey from Aleppo to Damascus. 2. A Tour in the District of Mount Libanus and Antilibanus. 3. A Tour in the Hauran. 4. A second Tour in the Hauran. 5. A Journey from Damascus, through Arabia Petræa and the Desert El Ty, to Cairo. 6. A Tour in the Peninsula of Mount Sinai. By the late John Lewis Burckhardt.

LXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

We have this month the painful task of announcing the suspension of the meetings of the Society in consequence of the death of its venerable President Sir Joseph Banks. He departed this life at his house Spring Grove, near Hounslow, on the 19th June. He was in his seventy-seventh year, having been born 13th December 1743.

Sir Joseph had been long a severe sufferer by the gout, and during the last sixteen years he was carried about, having lost the use of his limbs; but although so far advanced in years and oppressed by infirmities, he preserved to the last the same cheerfulness and liberality by which during a long life he was so eminently distinguished.

The loss to science by the demise of this excellent man and liberal patron will be long and severely felt. "His time," to use the words of a contemporary work*, "his wealth, his influence, his talents; an incomparable library of science and art; knowledge and judgement to advise; affability to conciliate and encourage; generosity to assist; all in short of which he was possessed, and it was all something either of goodness or greatness, he made the patrimony of the studious and learned, not of his own country alone, but of the whole world." His fortune was princely; and besides what he devoted to the encouragement of science, he expended a large portion of it in works of public and private benevolence.

* Percy Anecdotes of Science.

ASTRO-

ASTRONOMICAL SOCIETY OF LONDON.

June 9. This was the last meeting for the present sessions, and was very numerously attended. A paper by Mr. F. Baily was read, on the method of adjusting a transit instrument by observing the passage of two stars, differing considerably from each other in declination: and a new Table was given, whereby the deviation of the instrument and the error of the clock might be readily determined without the trouble of computation. Portable transit instruments are now made with great neatness and accuracy; and are a valuable acquisition to every œconomical observatory, and to such persons as are travelling with a view to improve the connected sciences of astronomy and geography. Instruments of this kind are often fixed in situations which do not command a view of the pole star: and under such circumstances, the table, above alluded to, is very desirable. Indeed M. Delambre prefers this mode of adjusting a transit instrument, to the observation of the circum-polar stars: he considers the results as more correct, and obtainable in much less time.—Sir H. Englefield requested permission to address the meeting, and called their attention to the new bodies which have lately been discovered, and which are supposed to be of a cometary nature. He noticed the discoveries of Cassini and Short, supposed to be of new planets, which have not since been observed; and wished some of the members who had time and inclination for the purpose, would submit the observations of those two celebrated astronomers to the test of analysis, in order to determine whether those bodies assimilated in their motions to those which have been lately discovered.—The Society afterwards adjourned till the 10th of November.

LXXV. *Intelligence and Miscellaneous Articles.*

NOTE ON THE DIVINING ROD, BY DR. CHARLES HUTTON.

To Mr. Tilloch.

Sir,—In the last No. (44) of the Quarterly Review, several small mistakes have accidentally crept into the short account relating to the alleged property of the divining rod, for the discovery of water, when used by certain persons. And as it must be desirable that every work should be as correct as possible, I communicate for your consideration the following particulars relating to the note in pp. 373 and 374, concerning some small inadvertencies that have occurred through forgetfulness, or by mistakes in reports passing through two or three different persons, as will appear by comparing the note with the full account in the 4th vol. of the new edition (an. 1814) of Dr. Charles Hutton's translation of Montucla's Philosophical Recreations, as given by the lady herself, whose name was then Lady M. but is now Lady N.

The note begins thus: "Jan. 21st, 1818: it is just 50 years since Lady N.'s attention was first called to this subject; she was then 16 years of age." Which must therefore have been in the year 1768; whereas Lady M.'s first letter to Dr. Hutton stated, that the transaction was in 1772 (if not 1773), when she was 19 years old. But these are only trifling discrepancies.

The remainder of the note differs not materially from the statement in Lady M.'s letter in Dr. Hutton's book above mentioned, till we arrive at the paragraph in the middle of the 374th page, viz. "A few years afterwards she went, at Dr. Hutton's particular request, to see him at Woolwich (Common), and she then showed him the experiment, and discovered a spring in a field which he had lately bought near the New College, then building. This same field he has since sold to the College, and for a larger price in consequence of the spring."

Now, Sir, this paragraph consists of a mass of inaccuracies. The fact is, that Dr. H. having about 15 or 16 years before purchased the field alluded to, bounded close upon the east side of Woolwich Common, and being convenient for erecting dwelling-houses, he immediately commenced a series of operations of that kind, and in the first year (1790) built the first house, which he occupied with his family during several years afterwards This house has now been used for the last 14 years as the hospital of the Military Academy or College, and goes by the name of the Cube House; standing on a square of 40 feet each side, and the same in height. During the building of this house, as it had always been reported and understood that no water was to be found on the neighbouring common, for his satisfaction Dr. H. procured a set of iron boring rods, with which he bored the ground to the depth of 50 feet, where he found a copious spring of water, and then dug his first well, 16 years before Lady M. did him the honour of her visit.

From that time Dr. H. continued similar operations, till, in the couse of 15 years, he had erected almost 20 new and genteel houses, part of them of 4 stories, and the others of 5 stories high, and had sunk wells to them, of the same depth as the first; having taken down about half-a-score cottages of two stories high, which had before been on the estate, to make room for his new buildings.

About this time the Board of Ordnance had in contemplation to erect somewhere a new College, the old one in the Warren (now called the Arsenal) being found too small, and the situation too confined: various situations were examined for that purpose; but

but on account of Dr. H's new residence, and of his having discovered plenty of water, his situation was preferred: accordingly the whole of Woolwich Common was purchased from the parish, and the new college was placed very near the first or cubical house built by Dr. H.

After the new college was completed, the King came down to view it, having a good taste in architecture, having indeed himself made choice of the drawings for the front of it, being in the handsome Gothic style. It happened that when standing before the building, and looking down towards the beautiful Military Barracks, at the lower or opposite end of the common, he perceived that some new buildings partly intercepted the view of the barracks. His Majesty immediately remarked the impropriety, and said that the obstructing buildings might be removed. He was then told that it was not in their power to remove them; that they were private freehold property, and belonged to Dr. H. the head master of the Academy. "But," said His Majesty, "they may be bought; let the whole be purchased, and the obstructing parts removed."

This was an order not to be neglected by the Board of Ordnance; and accordingly Dr. H. soon received a message from them, desiring him to transmit what price he demanded for his whole estate, houses and land. Dr. H. replied, That though he well knew they must have the property at any rate, yet he scorned to take any advantage of that circumstance, desiring not to have any thing beyond the bare and fair value of the estate; and agreeably to that resolution he requested the Board to send down their own surveyor to set a value on the estate, and whatever that valuation might be, he would accept it. The Board replied; There could be nothing more fair and liberal on his part. Accordingly, very soon after they sent down their own surveyor, Mr. Wyatt, for that purpose; who examined the property, both houses and land, said the buildings were very well executed, asked all the tenants what rent they paid; then adding all the rents together into one sum, multiplied it by 20: thus allowing twenty years purchase for the whole, both fields and land, which will certainly not be thought a high valuation. The surveyor reported to the Board his estimate, with which they immediately acquainted Dr. H., who said he was quite satisfied; and the business was concluded, with thanks and compliments to Dr. H. for his liberal conduct. Immediately six of the largest obstructing houses were taken down.

After all this was done, and the estate conveyed and paid for, Lady M. honoured Dr. H. with her visit; not to find him a spring of water, for that he had found himself long before, but to satisfy his curiosity as to the manner in which her ladyship

 performed

performed the experiment, and which she certainly did, to the satisfaction of himself and family, and of other friends who witnessed it.

See the whole of Lady M.'s correspondence on the subject, in the fourth volume of Dr. H.'s Philosophical Recreations above referred to.

NEW ELECTRICAL BATTERY.

Dr. Dana, of Harvard University in America, has constructed an electrical battery of plates, extremely portable and compact, and from his experiments appearing to be very powerful. It consists of alternate plates of flat glass and tin foil, the glass plates being on all sides two inches larger than those of foil. The alternate plates of tin foil are connected together, i. e. the 1st, 3d, 5th, 7th, &c. on one side, and the other series, or 2d, 4th, 6th, 8th, &c. on the other side, slips of tin foil extending from the sheet to the edge of the glass plates for that purpose. These connexions unite together all the surfaces, which, when the battery is charged, take by induction the same state. A battery constructed in this way contains, in the bulk of a 4to volume, a very powerful instrument; and when made of plate glass, it is extremely easy, by varnishing the edges, to keep the whole of the inner surfaces from the air, and to retain it in a constant state of dry insulation.

MORPHIA.

Dr. Thomson has given the following as the easiest method for obtaining pure morphia: Into a strong infusion of opium pour caustic ammonia. Separate the precipitate by the filter. Evaporate the solution to about a sixth, mix it with more ammonia, and a deposit of impure morphia is obtained. Collect the deposit by filter, and wash it with cold water. When well drained, pour a litle alcohol on it, and let the alcoholic liquor pass through the filter; it will carry off a good deal of colouring matter, and very little of the morphia. Dissolve the impure morphia in acetic acid, and mix the deep-brown solution with ivory black. Agitate the liquor frequently for 24 hours, filter it, and the liquid will pass through colourless. If ammonia be now dropped into it, pure morphia falls in the state of a white powder. If this precipitate be dissolved in alcohol and slowly evaporated, the morphia is obtained in crystals (they are rectangular prisms) perfectly white, of a pearly lustre, destitute of smell, and of an intensely bitter taste. Pure morphia, passed slowly through red hot peroxide of copper, is converted entirely into carbonic acid and water.—*Annales*, *No.* 90.

AMBER.

Dr. Brewster maintains, from a number of experiments and their results, that amber is an indurated vegetable juice.

BURNING

BURNING OF SMOKE.

We mentioned in our last that Mr. M. A. Taylor had brought this subject before the House of Commons, by a motion for a committee to consider of the practicability of compelling persons using furnaces, to erect them on a construction less prejudicial to health than the methods hitherto pursued. The motion having been agreed to, there has been no little stir in consequence among those intetested in the discussion. The method alluded to by Mr. Taylor as having been successful at Warwick, is not the only, and we are much inclined to believe not the best, that may be adopted for causing furnaces to burn their own smoke; and we should be sorry to see Parliament adopting any measure, which, even by implication, might be construed into an act of partiality. A committee reported last year on this very subject; and in their report, which was an able one, various furnaces were described, no way inferior, if not superior, in efficacy to the one so strongly recommended by Mr. Taylor. Among others, those of Messrs. Gregson, Losh, Wakefield, Brunton, and Walker, were noticed, and illustrated by means of engravings. The same parties, and other ingenious individuals, have been or will be before the present committee, and we may hope that much practical useful information will result from the investigation.

TEST FOR WHEAT FLOUR.

D. Tadei in the course of various researches respecting the fermentation of grain, has ascertained that the gluten of wheat is composed of two substances perfectly distinct from each other, one of which he has named *gloiodine*, and the other *zimome*. The first of these gives to gluten its elasticity, and the second is the cause of the fermentation which takes place on the mixture of gluten with other vegetable substances.

Having had occasion to mix various gums with different kinds of flour, D. Tadei observed, that when the powder of guaiacum is mixed with flour, a blue tint is produced. When kneaded with any meal or flour containing but little gluten, the blue colour is very feeble: it acquires no blue tint with flour that has been much altered in its qualities: with gluten or zimome a very fine blue tint is instantly produced. Guaiacum, it is inferred, may therefore be employed as a test to detect adulteration of flour.—*Giornale di Fisica, Chemica, &c.* 1819.

VOYAGES, ETC.

The French Government is now preparing a voyage to Lapland. It is to proceed beyond the North Cape, into the Frozen Ocean, and is expected to terminate about the end of September this year.

M. the

M. the Count de Romanzow is fitting out at his own expense an expedition which is to set out from Tehouktches, so as to pass over the solid ice from Asia to America, to the north of Behring's Strait, at the point where Cook and Kotzebue were stopped.

The same gentleman is also fitting out an expedition which is to ascend one of the rivers which disembogue on the western coast, in Russian America, in order to penetrate into the unknown tracts that lie between Icy Cape and the river Mackenzie.

The French Minister of the Interior has engaged M. Dufour to make an excursion into such districts of the Pyrenees, as have not been explored by botanists, to augment the French Flora.

LIST OF PATENTS FOR NEW INVENTIONS.

To John Hague, of Great Pearl-street, Spital Fields, Middlesex, engineer, for his improvement in preparing the materials for making pottery-ware, tiles and bricks.—2d June 1820.—6 months allowed to inroll specification.

To William Bate, of Peterborough, Northampton, esq. for his combination of additions to machinery calculated to increase power.—3d June.—6 months.

To William Bate, of Peterborough, Northampton, esq. for certain improvements in preparing hemp flax or other fibrous material for spinning.—3d June.—4 months.

To Simon Teissier, of Paris, in the kingdom of France, but at present residing in Bucklersbury in the city of London, merchant, for certain improvements in propelling vessels, communicated to him by a certain foreigner residing abroad.—3d June.—2 months.

To Jacob Perkins, late of Philadelphia, in the United States of America, but now residing at Austin Friars in the city of London, engineer, for certain improvements in the construction of fixed and portable pumps, such as pumps fixed for raising water from wells and other situations, or ships' pumps, or for portable pumps which may be employed for garden engines, or in engines for extinguishing fires or other purposes.—3d June.—6 months.

To John Hague, of Great Pearl-street, Spital Fields, Middlesex, engineer, for certain improvements in the making and constructing of steam engines.—3d June.—2 months.

To John Wakefield, of Ancott's place, Manchester, engineer, for certain improvements in the construction of furnaces for boilers of various descriptions, and in the mode of feeding the same with fuel, which improvements are calculated to lessen the consumption

tion of fuel, and to burn the smoke, that the same are calculated to produce important benefit to the community.—6th June.—6 months.

To William Kendrick, of Birmingham, in the county of Warwick, chemist, for the manufacture of a liquid from materials now considered useless for that purpose, and the application of the same liquid to the tanning of hides and other articles requiring such process.—6th June.—6 months.

To Jonathan Brownill, of Sheffield, Yorkshire, for his method for better securing the blades of table knives and forks in the handles by means of caps being soldered upon the tangs, whether of iron, steel, or other material, after the handles are upon them. —8th June.—4 months.

To Samuel Parker, of Argyll-street, in the county of Middlesex, bronzist, for an improved lamp.—15th June.—2 months.

To William Erskine Cochrane, of Somerset-street, Portman-square, county of Middlesex, esq. for his improvement in the construction of lamps.—17th June.—6 months.

To Joseph Woollams, of the city of Wells in the county of Somerset, for certain improvements in the teeth or cogs formed on, or applied to wheels, pinions, and other mechanical agents for communicating or restraining motion.—20th June.—6 mo.

To John Butler Lodge, and John Belleston junior, both of the Strand, in the county of Middlesex, truss-makers, for certain improvements in the construction and application of spring trusses or bandages for the relief or cure of hernia.—20th June.—2 months.

To John Vallance, of Brighthelmstone, in the county of Sussex, brewer, for his method and apparatus for freeing rooms and buildings (whether public or private) from the distressing heat sometimes experienced in them, and keeping them constantly cool, or of a pleasant temperature, whether they are crowded to excess or empty, and also whether the weather be hot or cold.—20th June.—6 months.

To John Vallance, of Brighthelmstone, in the county of Sussex, brewer, for his method and apparatus for packing and preserving hops.—20th June.—6 months.

To John Shaw, of Mary-street, Fitzroy-square, in the county of Middlesex, watch-maker, for his new method of making bricks by machinery.—21st June.—6 months.

To James Harcourt, of Birmingham, for an improvement in castors applicable to tables and other articles.—21st June.—2 months.

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1820.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS			
May 15	3	68·0	29·50	Fine
16	4	66·	29·47	Rain all the morning
17	5	60·	29·43	Fine
18	6	54·	29·40	Heavy rain all the morning
19	7	58·	29·50	Showery
20	8	63·	29·90	Ditto
21	9	68·	30·	Fine
22	10	73·	29·75	Ditto
23	11	72·	29·58	Ditto
24	12	75·	29·34	Ditto
25	13	52·	29·40	Rain
26	14	63·	29·46	Fine
27	full	62·5	29·35	Rain A.M.
28	16	63·	29·15	Rain at night
29	17	56·	29·10	Showery
30	18	48·5	29·10	Rain
31	19	52·5	29·10	Cloudy
June 1	20	55·5	29·22	Stormy
2	21	60·	29·28	Fine—Hail-storm P.M.
3	22	58·	29·45	Cloudy
4	23	57·5	29·65	Ditto—rain in the morning
5	24	64·	29·70	Fine—rain all night
6	25	58·5	29·78	Ditto—rain A.M.
7	26	57·5	29·72	Cloudy
8	27	58·	29·70	Ditto—rain in the evening
9	28	52·5	29·50	Rain
10	new	56·	29·50	Cloudy
11	1	52·5	29·33	Rain
12	2	58·	29·53	Fine
13	3	52·5	29·66	Rain
14	4	56·5	29·87	Cloudy—rain A.M. and again in the evening.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For June 1820.

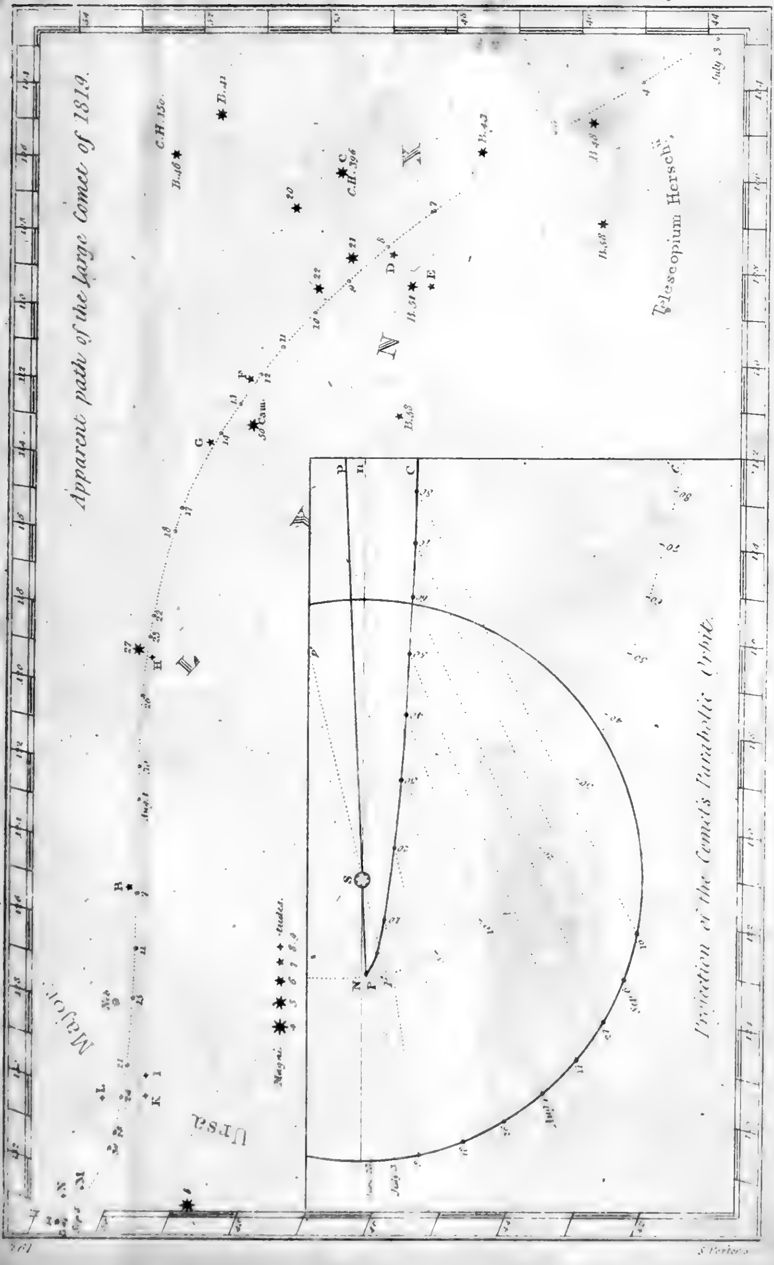
Days of Month. 1820.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Weather.
May 27	58	62	50	29·74	Showery
28	57	60	51	·67	Rain
29	50	58	46	·44	Showery
30	50	59	50	·46	Fair
31	52	56	50	·49	Showery
June 1	55	60	51	·62	Showery
2	56	60	50	·66	Stormy
3	50	57	49	·82	Cloudy
4	49	54	49	·99	Rain
5	49	61	48	30·04	Fair
6	49	55	49	·02	Rain
7	54	63	51	·06	Fair
8	52	61	52	29·95	Cloudy
9	52	60	54	·85	Cloudy
10	53	58	48	·82	Cloudy
11	49	53	50	·65	Rain
12	50	59	48	·87	Fair
13	50	53	50	·95	Thunder Storm,
14	51	58	52	30·15	Cloudy [with hail
15	53	58	53	29·94	Rain
16	54	62	55	30·04	Cloudy
17	56	65	58	·05	Fair
18	58	68	59	·10	Fair
19	56	67	56	29·86	Fair
20	56	58	57	·77	Rain
21	58	64	58	30·01	Fair
22	58	71	59	·19	Fair
23	60	76	60	·25	Fair
24	66	78	65	·35	Fair
25	67	83	74	·46	Fair
26	75	87	68	·54	Fair

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

INDEX TO VOL. LV.

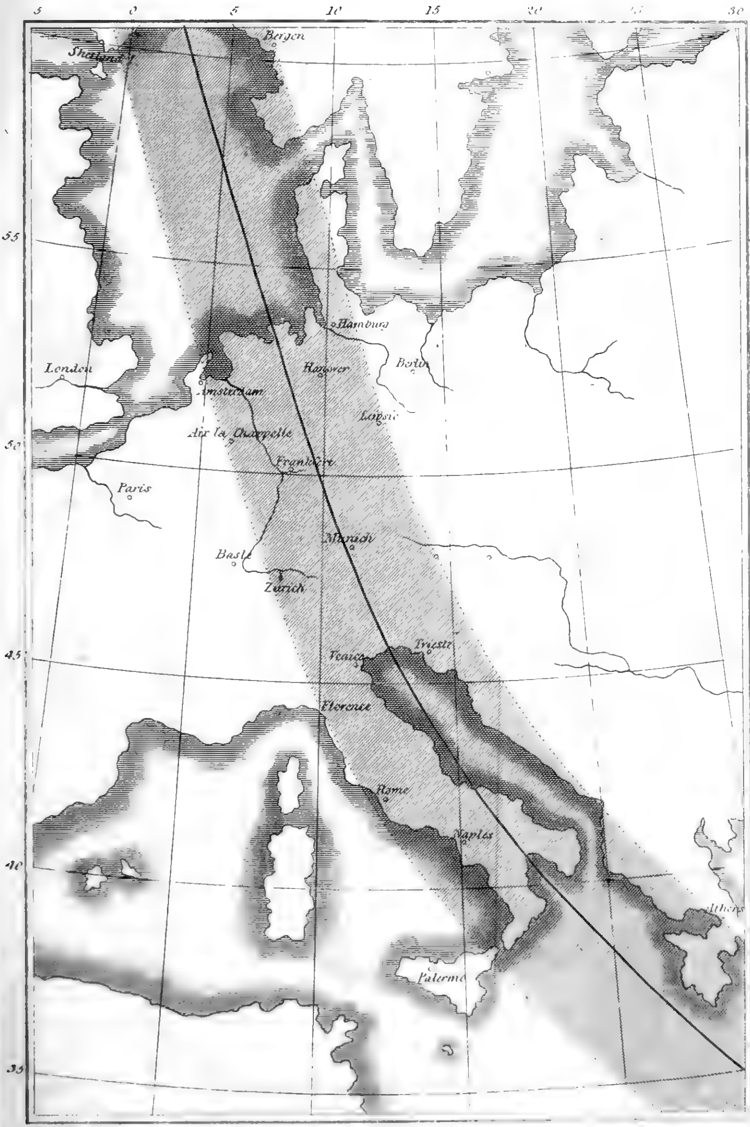
END OF THE FIFTY-FIFTH VOLUME.

Printed by R. and A. Taylor, Shoe Lane, London.


Apparent path of the large Comet of 1819.
Projection of the Comet's Parabolic Orbit.
Ursa Major
Telescopium Herschel
Magnitudes 4 5 6 7 8.9
C.H. 150
C.H. 396
50 Cam.
July 3
Aug. 1
S Porter sc.

EN

Print



Bergen
Shetland
London
Amsterdam
Aix la Chapelle
Paris
Hamburg
Hanover
Berlin
Leipsic
Frankfort
Munich
Basle
Zurich
Trieste
Florence
Rome
Naples
Palermo
Athens
Long East
from Greenwich

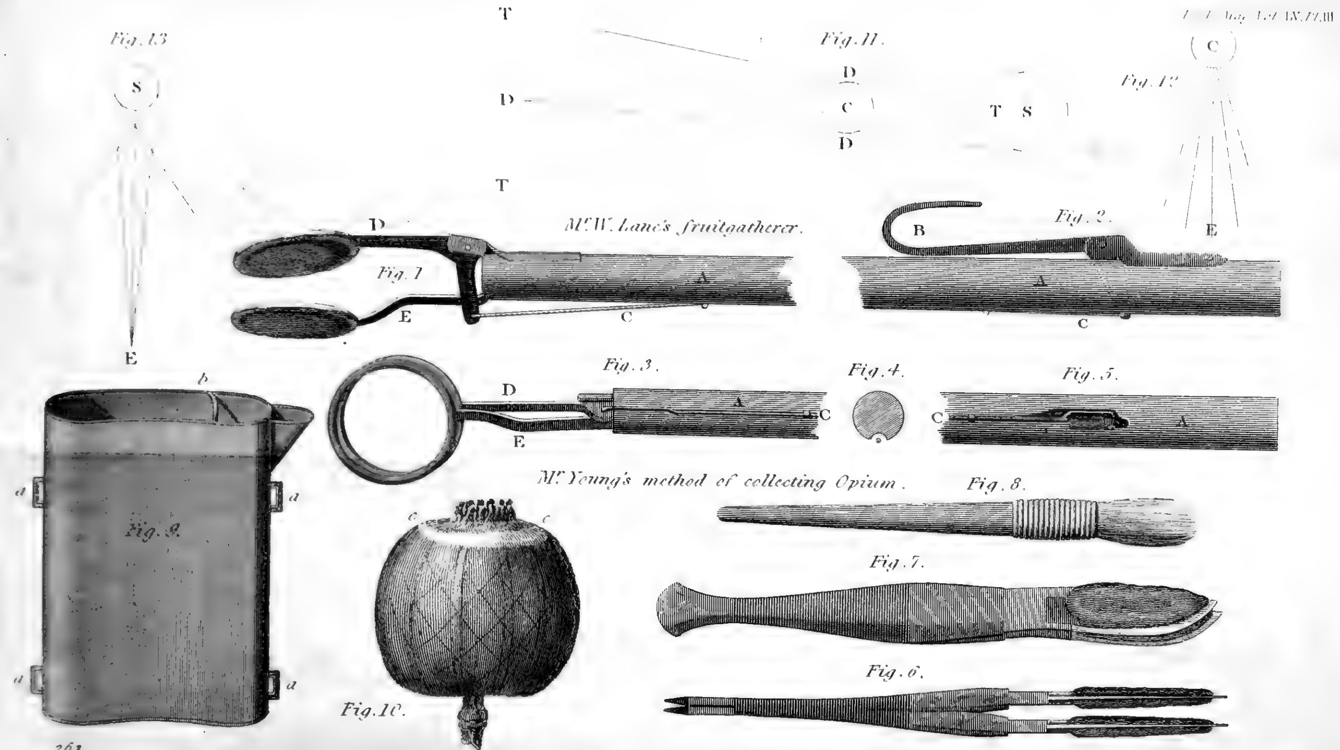
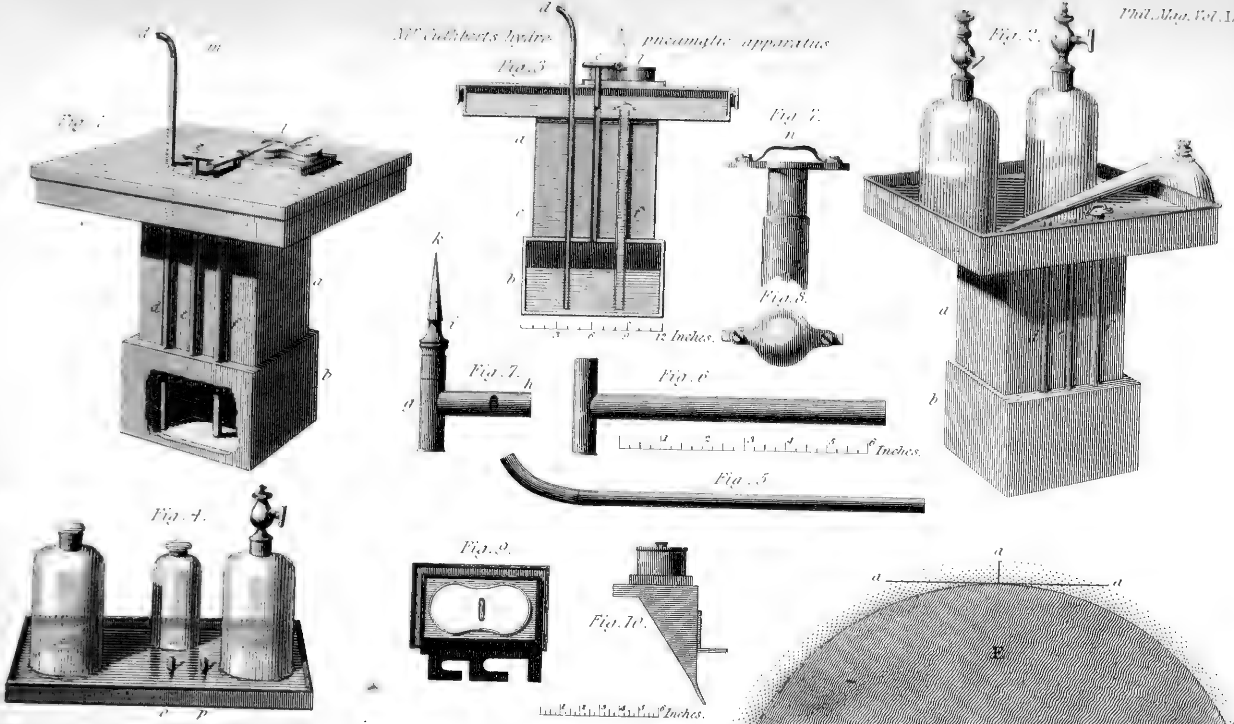


Fig. 13
S
E
T
D
T
Fig. 11.
D
C
D
T S
Fig. 12
C
E
Mr. W. Lane's fruitgatherer.
Fig. 1
D
E
A
C
Fig. 2.
B
A
C
Fig. 3.
D
E
A
C
Fig. 4.
Fig. 5.
C
A
Mr. Young's method of collecting Opium.
Fig. 8.
Fig. 7.
Fig. 6.
Fig. 9.
a
b
Fig. 10.
c

S. Porter. sc.

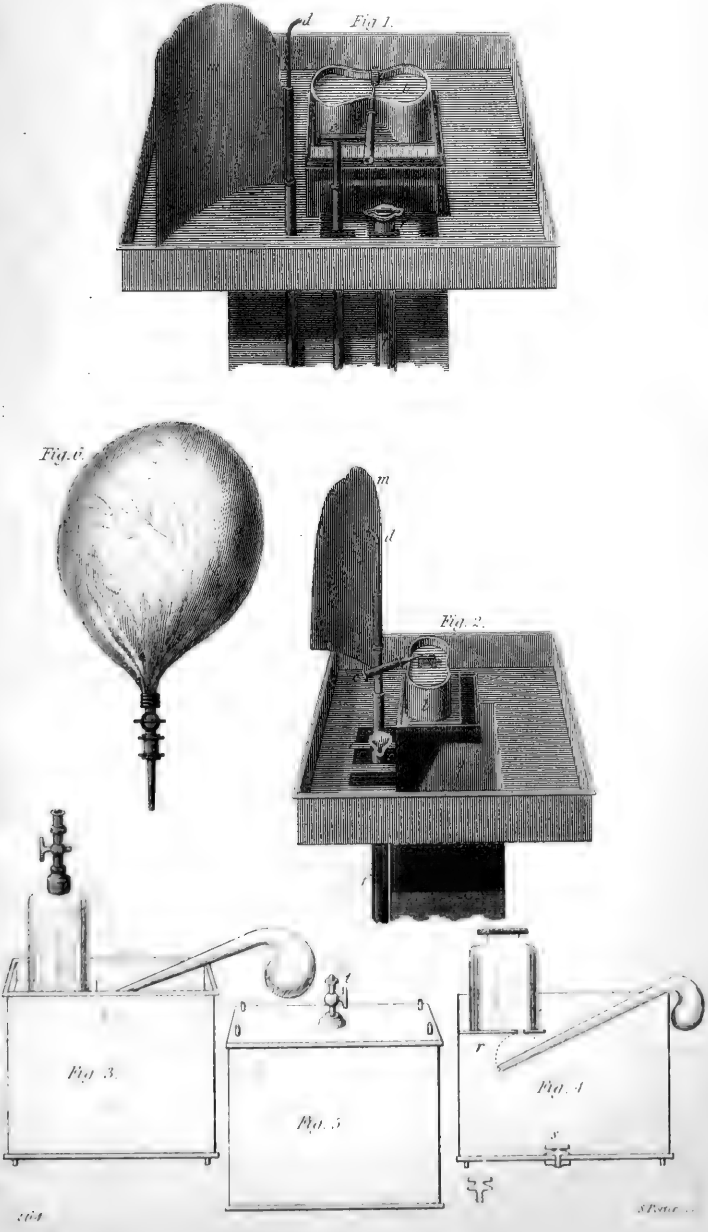
Mr. Cuthberts hydro-pneumatic apparatus



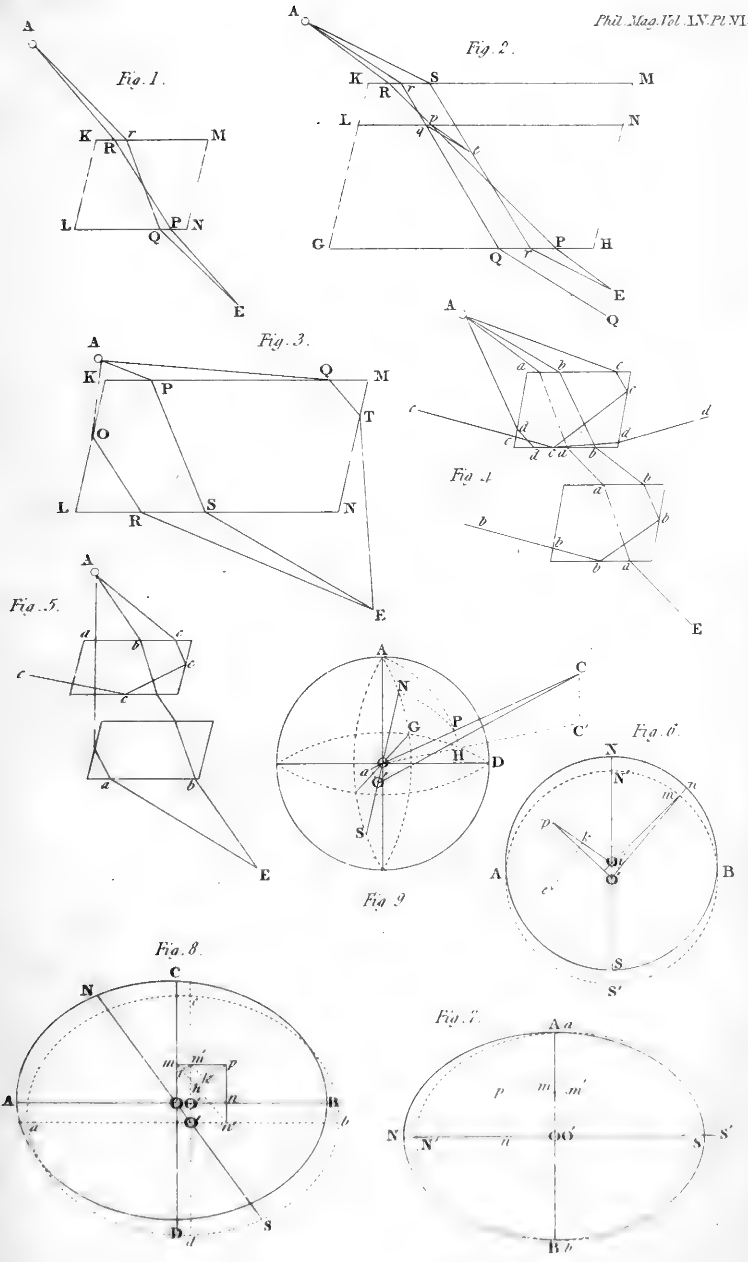
264

S. Porter sc.

Mr Cuthbert's hydro-pneumatic apparatus



S. Porter sc.



J. Porter, sc.

ENGRAVINGS.

Vol. LI. A Plate illustrative of Mr. CAPEL LOFFT's Paper on the Probability of Meteorolites being projected from the Moon.—Two Plates: one, of Mr. H. TRITTON's Improved Apparatus for Distillation; and another, of the Figures in BRADLEY's Gardening illustrative of the Article on the KALEIDOSCOPE.—A Plate illustrative of Mrs. IBBETSON's Paper on the Anatomy of Vegetables; and Mr. TREDGOLD's on Revetements.

Vol. LII. A Plate illustrative of Mr. UPINGTON's Electrical Increaser for the unerring Manifestation of small Portions of the Electric Fluid.—A Plate illustrative of Mrs. IBBETSON's Paper on the Fructification of Plants.—A Plate illustrative of the Rev. JOHN MICHELL's Theory of the Formation of the Earth.—A Plate illustrative of Capt. KATER's Article on the Pendulum; and New Apparatus for impregnating Liquids with Gases.—A Plate illustrative of Sir H. DAVY's Apparatus for Volatilization of Phosphorus, and Mr. SMITH's Essay on the Structure of the poisonous Fangs of Serpents.

Vol. LIII. A Plate illustrative of Dr. URE's Experiments on Caloric, Mr. LUCKCOCK's Paper on the Atomic Philosophy, and Mr. BOLTON's on the Purification of Coal Gas.—A Plate representing Mr. RENNIE's Apparatus employed in his Experiments on the Strength of Materials; and the Marquis RIDOLPHI's Improvement on the Gas Blow-pipe.—A Plate illustrative of Mr. MEIKLE's Paper on Calorific Radiation; Mr. LOWE's on the Purification of Coal Gas; and Mr. HUGHES's on ascertaining Distances. — A Plate illustrative of Dr. OLINTHUS GREGORY's Paper on the different Rates of PENNINGTON's Astronomical Clock at the Island of Balta, and at Woolwich Common.

Vol. LIV. A Plate illustrative of the MENAI BRIDGE.—A Plate illustrative of Mr. LOWE's Description of a Mercurial Pendulum.—A Plate illustrative of Mr. HARE's Calorimotor, a new Galvanic Instrument.—A Plate illustrative of Captain SABINE's Paper on Irregularities observed in the Direction of the Compass Needles of the Isabella and Alexander in the late Voyage of Discovery; and Mr. SCORESBY's Anomaly in the Variation of the Magnetic Needle as observed on Ship-board.

Contents of Number 261.

*** 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. XLIX. A Plate to illustrate Dr. EVANS's Communication on Terrestrial Magnetism; a new Electro-atmospherical Instrument; and Mr. ANDREW HORN's Paper on Vision.—A Quarto Plate to illustrate Mrs. IBBETSON's Physiology of Vegetables.—A Plate to illustrate the Solar Spots which appeared during the Year 1816;—and Mr. BEVAN's Improvement on the Sliding-Rule.—A Plate descriptive of Mr. EMMETT's Instrument for the Measurement of the Moon's Distance from the Sun, &c.; also a New Reflecting Goniometer.—A Plate to illustrate Chevalier BAADER's Method of communicating Rotatory Motion; Lieut. SHULDHAM's improved Method of working a Capstan; and STEELE's new Modification of NOOTH's Apparatus, &c.

Vol. L. A Plate to illustrate Sir HUMPHRY DAVY's new Researches on Flame, and Sir GEORGE CAYLEY's Paper on Aërial Navigation.—A Plate representing a Section of the Pneumatic Cistern, with the compound Blow-pipe of Mr. HARE; and a Sketch of a Steam-Vessel intended to run between London and Exeter.—Representation of Apparatus for Sublimation of Iodine—Model of a Safety Furnace by Mr. BAKEWELL—Apparatus for consuming Fire-damp in the Mine—and Apparatus for re-lighting the Miners' Davy.—A Plate illustrative of the New Patent Horizontal Water-Wheel of Mr. ADAMSON.—A Plate illustrative of Mrs. IBBETSON's Theory of the Physiology of Vegetables.—A Plate to illustrate Mr. DICKINSON's new System of Beaconing.

Vol. LI. A Plate illustrative of Mr. CAPEL LOFFT's Paper on the Probability of Meteorolites being projected from the Moon.—Two Plates: one, of Mr. H. TRITTON's Improved Apparatus for Distillation; and another, of the Figures in BRADLEY's Gardening illustrative of the Article on the KALEIDOSCOPE.—A Plate illustrative of Mrs. IBBETSON's Paper on the Anatomy of Vegetables; and Mr. TREDGOLD's on Revetements.

Vol. LII. A Plate illustrative of Mr. UPINGTON's Electrical Increaser for the unerring Manifestation of small Portions of the Electric Fluid.—A Plate illustrative of Mrs. IBBETSON's Paper on the Fructification of Plants.—A Plate illustrative of the Rev. JOHN MICHELL's Theory of the Formation of the Earth.—A Plate illustrative of Capt. KATER's Article on the Pendulum; and New Apparatus for impregnating Liquids with Gases.—A Plate illustrative of Sir H. DAVY's Apparatus for Volatilization of Phosphorus, and Mr. SMITH's Essay on the Structure of the poisonous Fangs of Serpents.

Vol. LIII. A Plate illustrative of Dr. URE's Experiments on Caloric, Mr. LUCKCOCK's Paper on the Atomic Philosophy, and Mr. BOLTON's on the Purification of Coal Gas.—A Plate representing Mr. RENNIE's Apparatus employed in his Experiments on the Strength of Materials; and the Marquis RIDOLPHI's Improvement on the Gas Blow-pipe.—A Plate illustrative of Mr. MEIKLE's Paper on Calorific Radiation; Mr. LOWE's on the Purification of Coal Gas; and Mr. HUGHES's on ascertaining Distances. — A Plate illustrative of Dr. OLINTHUS GREGORY's Paper on the different Rates of PENNINGTON's Astronomical Clock at the Island of Balta, and at Woolwich Common.

Vol. LIV. A Plate illustrative of the MENAI BRIDGE.—A Plate illustrative of Mr. LOWE's Description of a Mercurial Pendulum.—A Plate illustrative of Mr. HARE's Calorimotor, a new Galvanic Instrument.—A Plate illustrative of Captain SABINE's Paper on Irregularities observed in the Direction of the Compass Needles of the Isabella and Alexander in the late Voyage of Discovery; and Mr. SCORESBY's Anomaly in the Variation of the Magnetic Needle as observed on Ship-board.

Vol. LV. A Plate exhibiting Sketch of the Comet's Path of July 1819.

Contents of Number 263.

*** 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.

Vol. XLIX. A Plate to illustrate Dr. EVANS's Communication on Terrestrial Magnetism; a new Electro-atmospherical Instrument; and Mr. ANDREW HORN's Paper on Vision.—A Quarto Plate to illustrate Mrs. IBBETSON's Physiology of Vegetables.—A Plate to illustrate the Solar Spots which appeared during the Year 1816;—and Mr. BEVAN's Improvement on the Sliding-Rule.—A Plate descriptive of Mr. EMMETT's Instrument for the Measurement of the Moon's Distance from the Sun, &c.; also a New Reflecting Goniometer.—A Plate to illustrate Chevalier BAADER's Method of communicating Rotatory Motion; Lieut. SHULDHAM's improved Method of working a Capstan; and STEELE's new Modification of NOOTH's Apparatus, &c.

Vol. L. A Plate to illustrate Sir HUMPHRY DAVY's new Researches on Flame, and Sir GEORGE CAYLEY's Paper on Aërial Navigation.—A Plate representing a Section of the Pneumatic Cistern, with the compound Blow-pipe of Mr. HARE; and a Sketch of a Steam-Vessel intended to run between London and Exeter.—Representation of Apparatus for Sublimation of Iodine—Model of a Safety Furnace by Mr. BAKEWELL—Apparatus for consuming Fire-damp in the Mine—and Apparatus for re-lighting the Miners' Davy.—A Plate illustrative of the New Patent Horizontal Water-Wheel of Mr. ADAMSON.—A Plate illustrative of Mrs. IBBETSON's Theory of the Physiology of Vegetables.—A Plate to illustrate Mr. DICKINSON's new System of Beaconing.

Vol. LI. A Plate illustrative of Mr. CAPEL LOFFT's Paper on the Probability of Meteorolites being projected from the Moon.—Two Plates: one, of Mr. H. TRITTON's Improved Apparatus for Distillation; and another, of the Figures in BRADLEY's Gardening illustrative of the Article on the KALEIDOSCOPE.—A Plate illustrative of Mrs. IBBETSON's Paper on the Anatomy of Vegetables; and Mr. TREDGOLD's on Revetements.

Vol. LII. A Plate illustrative of Mr. UPINGTON's Electrical Increaser for the unerring Manifestation of small Portions of the Electric Fluid.—A Plate illustrative of Mrs. IBBETSON's Paper on the Fructification of Plants.—A Plate illustrative of the Rev. JOHN MICHELL's Theory of the Formation of the Earth.—A Plate illustrative of Capt. KATER's Article on the Pendulum; and New Apparatus for impregnating Liquids with Gases.—A Plate illustrative of Sir H. DAVY's Apparatus for Volatilization of Phosphorus, and Mr. SMITH's Essay on the Structure of the poisonous Fangs of Serpents.

Vol. LIII. A Plate illustrative of Dr. URE's Experiments on Caloric, Mr. LUCKCOCK's Paper on the Atomic Philosophy, and Mr. BOLTON's on the Purification of Coal Gas.—A Plate representing Mr. RENNIE's Apparatus employed in his Experiments on the Strength of Materials; and the Marquis RIDOLPHI's Improvement on the Gas Blow-pipe.—A Plate illustrative of Mr. MEIKLE's Paper on Calorific Radiation; Mr. LOWE's on the Purification of Coal Gas; and Mr. HUGHES's on ascertaining Distances. — A Plate illustrative of Dr. OLINTHUS GREGORY's Paper on the different Rates of PENNINGTON's Astronomical Clock at the Island of Balta, and at Woolwich Common.

Vol. LIV. A Plate illustrative of the MENAI BRIDGE.—A Plate illustrative of Mr. LOWE's Description of a Mercurial Pendulum.—A Plate illustrative of Mr. HARE's Calorimotor, a new Galvanic Instrument.—A Plate illustrative of Captain SABINE's Paper on Irregularities observed in the Direction of the Compass Needles of the Isabella and Alexander in the late Voyage of Discovery; and Mr. SCORESBY's Anomaly in the Variation of the Magnetic Needle as observed on Ship-board.

Vol. LV. A Plate exhibiting Sketch of the Comet's Path of July 1819.—A Plate illustrative of the Annular Eclipse of the Sun on the 7th of September next.—A Plate illustrative of Mr. LANE's Instrument for gathering Fruit; Mr. YOUNG's Mode of preparing Opium from the *Papaver somniferum;* and of Captain FORMAN's Essay on a Property in

Contents of Number 264.

*** 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.

&c.; also a New Reflecting Goniometer.—A Plate to illustrate Chevalier BAADER's Method of communicating Rotatory Motion; Lieut. SHULDHAM's improved Method of working a Capstan; and STEELE's new Modification of NOOTH's Apparatus, &c.

Vol. L. A Plate to illustrate Sir HUMPHRY DAVY's new Researches on Flame, and Sir GEORGE CAYLEY's Paper on Aërial Navigation.—A Plate representing a Section of the Pneumatic Cistern, with the compound Blow-pipe of Mr. HARE; and a Sketch of a Steam-Vessel intended to run between London and Exeter.—Representation of Apparatus for Sublimation of Iodine—Model of a Safety Furnace by Mr. BAKEWELL—Apparatus for consuming Fire-damp in the Mine—and Apparatus for re-lighting the Miners' Davy.—A Plate illustrative of the New Patent Horizontal Water-Wheel of Mr. ADAMSON.—A Plate illustrative of Mrs. IBBETSON's Theory of the Physiology of Vegetables.—A Plate to illustrate Mr. DICKINSON's new System of Beaconing.

Vol. LI. A Plate illustrative of Mr. CAPEL LOFFT's Paper on the Probability of Meteorolites being projected from the Moon.—Two Plates: one, of Mr. H. TRITTON's Improved Apparatus for Distillation; and another, of the Figures in BRADLEY's Gardening illustrative of the Article on the KALEIDOSCOPE.—A Plate illustrative of Mrs. IBBETSON's Paper on the Anatomy of Vegetables; and Mr. TREDGOLD's on Revetements.

Vol. LII. A Plate illustrative of Mr. UPINGTON's Electrical Increaser for the unerring Manifestation of small Portions of the Electric Fluid.—A Plate illustrative of Mrs. IBBETSON's Paper on the Fructification of Plants.—A Plate illustrative of the Rev. JOHN MICHELL's Theory of the Formation of the Earth.—A Plate illustrative of Capt. KATER's Article on the Pendulum; and New Apparatus for impregnating Liquids with Gases.—A Plate illustrative of Sir H. DAVY's Apparatus for Volatilization of Phosphorus, and Mr. SMITH's Essay on the Structure of the poisonous Fangs of Serpents.

Vol. LIII. A Plate illustrative of Dr. URE's Experiments on Caloric, Mr. LUCKCOCK's Paper on the Atomic Philosophy, and Mr. BOLTON's on the Purification of Coal Gas.—A Plate representing Mr. RENNIE's Apparatus employed in his Experiments on the Strength of Materials; and the Marquis RIDOLPHI's Improvement on the Gas Blow-pipe.—A Plate illustrative of Mr. MEIKLE's Paper on Calorific Radiation; Mr. LOWE's on the Purification of Coal Gas; and Mr. HUGHES's on ascertaining Distances. — A Plate illustrative of Dr. OLINTHUS GREGORY's Paper on the different Rates of PENNINGTON's Astronomical Clock at the Island of Balta, and at Woolwich Common.

Vol. LIV. A Plate illustrative of the MENAI BRIDGE.—A Plate illustrative of Mr. LOWE's Description of a Mercurial Pendulum.—A Plate illustrative of Mr. HARE's Calorimotor, a new Galvanic Instrument.—A Plate illustrative of Captain SABINE's Paper on Irregularities observed in the Direction of the Compass Needles of the Isabella and Alexander in the late Voyage of Discovery; and Mr. SCORESBY's Anomaly in the Variation of the Magnetic Needle as observed on Ship-board.

Vol. LV. A Plate exhibiting Sketch of the Comet's Path of July 1819.—A Plate illustrative of the Annular Eclipse of the Sun on the 7th of September next.—A Plate illustrative of Mr. LANE's Instrument for gathering Fruit; Mr. YOUNG's Mode of preparing Opium from the *Papaver somniferum*; and of Captain FORMAN's Essay on a Property in Light which hitherto has been unobserved by Philosophers.—A Plate descriptive of Mr. CUTHBERT's improved Hydro-pneumatic Apparatus, &c.

Vol. 55. Philosophical Magazine. May 1820.

Contents of Number 265.

RICHARD AND ARTHUR TAYLOR, PRINTERS, SHOE LANE, LONDON.

Improvement on the Sliding-Rule.—A Plate descriptive of Mr. EMMETT's Instrument for the Measurement of the Moon's Distance from the Sun, &c.; also a New Reflecting Goniometer.—A Plate to illustrate Chevalier BAADER's Method of communicating Rotatory Motion; Lieut. SHULDHAM's improved Method of working a Capstan; and STEELE's new Modification of NOOTH's Apparatus, &c.

Vol. L. A Plate to illustrate Sir HUMPHRY DAVY's new Researches on Flame, and Sir GEORGE CAYLEY's Paper on Aërial Navigation.—A Plate representing a Section of the Pneumatic Cistern, with the compound Blow-pipe of Mr. HARE; and a Sketch of a Steam-Vessel intended to run between London and Exeter.—Representation of Apparatus for Sublimation of Iodine—Model of a Safety Furnace by Mr. BAKEWELL—Apparatus for consuming Fire-damp in the Mine—and Apparatus for re-lighting the Miners' Davy.—A Plate illustrative of the New Patent Horizontal Water-Wheel of Mr. ADAMSON.—A Plate illustrative of Mrs. IBBETSON's Theory of the Physiology of Vegetables.—A Plate to illustrate Mr. DICKINSON's new System of Beaconing.

Vol. LI. A Plate illustrative of Mr. CAPEL LOFFT's Paper on the Probability of Meteorolites being projected from the Moon.—Two Plates: one, of Mr. H. TRITTON's Improved Apparatus for Distillation; and another, of the Figures in BRADLEY's Gardening illustrative of the Article on the KALEIDOSCOPE.—A Plate illustrative of Mrs. IBBETSON's Paper on the Anatomy of Vegetables; and Mr. TREDGOLD's on Revetements.

Vol. LII. A Plate illustrative of Mr. UPINGTON's Electrical Increaser for the unerring Manifestation of small Portions of the Electric Fluid.—A Plate illustrative of Mrs. IBBETSON's Paper on the Fructification of Plants.—A Plate illustrative of the Rev. JOHN MICHELL's Theory of the Formation of the Earth.—A Plate illustrative of Capt. KATER's Article on the Pendulum; and New Apparatus for impregnating Liquids with Gases.—A Plate illustrative of Sir H. DAVY's Apparatus for Volatilization of Phosphorus, and Mr. SMITH's Essay on the Structure of the poisonous Fangs of Serpents.

Vol. LIII. A Plate illustrative of Dr. URE's Experiments on Caloric, Mr. LUCKCOCK's Paper on the Atomic Philosophy, and Mr. BOLTON's on the Purification of Coal Gas.—A Plate representing Mr. RENNIE's Apparatus employed in his Experiments on the Strength of Materials; and the Marquis RIDOLPHI's Improvement on the Gas Blow-pipe.—A Plate illustrative of Mr. MEIKLE's Paper on Calorific Radiation; Mr. LOWE's on the Purification of Coal Gas; and Mr. HUGHES's on ascertaining Distances. — A Plate illustrative of Dr. OLINTHUS GREGORY's Paper on the different Rates of PENNINGTON's Astronomical Clock at the Island of Balta, and at Woolwich Common.

Vol. LIV. A Plate illustrative of the MENAI BRIDGE.—A Plate illustrative of Mr. LOWE's Description of a Mercurial Pendulum.—A Plate illustrative of Mr. HARE's Calorimotor, a new Galvanic Instrument.—A Plate illustrative of Captain SABINE's Paper on Irregularities observed in the Direction of the Compass Needles of the Isabella and Alexander in the late Voyage of Discovery; and Mr. SCORESBY's Anomaly in the Variation of the Magnetic Needle as observed on Ship-board.

Vol. LV. A Plate exhibiting Sketch of the Comet's Path of July 1819.—A Plate illustrative of the Annular Eclipse of the Sun on the 7th of September next.—A Plate illustrative of Mr. LANE's Instrument for gathering Fruit; Mr. YOUNG's Mode of preparing Opium from the *Papaver somniferum*; and of Captain FORMAN's Essay on a Property in Light which hitherto has been unobserved by Philosophers.—A Plate descriptive of Mr. CUTHBERT's improved Hydro-pneumatic Apparatus, &c.

Vol. 55. Philosophical Magazine. June 1820.

Contents of Number 266.

*** 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.