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THE PHILOSOPHICAL MAGAZINE:

COMPREHENDING

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

NUMBER CI.
For OCTOBER 1806.

ILLUSTRATED WITH THE FOLLOWING ENGRAVINGS:

1. A Quarto Plate of M. LE ROY's Chronometer.
2. Mr. SYLVESTER's improved Air-Pump.

BY ALEXANDER TILLOCH,
HONORARY MEMBER OF THE ROYAL IRISH ACADEMY, &c. &c. &c.

LONDON:
Printed by R. Taylor and Co., 38, Shoe Lane, Fleet Street,
FOR A. TILLOCH:
And sold by Messrs. RICHARDSON; CADELL and DAVIES; LONGMAN, HURST, REES, and ORME; SYMONDS; MURRAY; HIGHLEY; VERNOR and HOOD; HARDING; London: BELL and BRADFUTE, Edinburgh; BRASH and REID, and D. NIVEN, Glasgow; and GILBERT and HODGES, Dublin.

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Vol. XXV. contains an Octavo Plate illustrative of Experiments by Sir JAMES HALL on the Effects of Compression in modifying the Action of Heat—A Quarto Plate on the same Subject—A Quarto Plate to illustrate Sir JAMES HALL's Paper on the Effects of Heat modified by Compression—Mr. SALMON's improved Geometrical Quadrant, Level, and Calculator, for ascertaining inaccessible Distances on Land or at Sea.—M. MONTGOLFIER's new Calorimeter, for determining the comparative Quantities of Caloric evolved by the Combustion of different Kinds of Fuel—Mr. GILBERT GILPIN's improved Crane with Flexible Chains—Mr. HERBERT's Book-case Bolt, and Mr. LE CAAN's Check for Carriage Wheels on Rail Roads—SALMON's Improvement on Canal Locks—Figures to illustrate M. GROTTHIUS's Memoir on the De composition of Water by Galvanism—A Bust in Silex, found coated over with Chalcedony.

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NUMBER CII.
For NOVEMBER 1806.

*** The Plates intended to have accompanied our present Number relate to M. le Roy's Chronometer; but Mr. Lowry, who was executing them, having been taken suddenly ill, and being at this Moment confined to his Bed, we are obliged to publish this Month without any Engravings.

BY ALEXANDER TILLOCH,
HONORARY MEMBER OF THE ROYAL IRISH ACADEMY, &c. &c. &c.

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ENGRAVINGS.

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NUMBER CIII.
For DECEMBER 1806.

ILLUSTRATED WITH THE FOLLOWING ENGRAVINGS,
BY LOWRY:

1. A large Plate relating to Le Roy's Chronometer.
2. An Octavo Plate on the same Subject.
3. The Irish Canal Track-Boat.

BY ALEXANDER TILLOCH,
HONORARY MEMBER OF THE ROYAL IRISH ACADEMY, &c. &c. &c.

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NUMBER CIV.
For JANUARY 1807.

ILLUSTRATED WITH THE FOLLOWING ENGRAVINGS,
BY LOWRY:

1. Mr. STEEVENS's Autocratic Cock for large Reservoirs.
2. Mr. WOOLF's improved Piston for Steam-Engines.
3. Another Plate relating to the same Subject.

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Vol. XX. is illustrated with a Quarto Plate to illustrate the Anatomy of the Rhinoceros—A Quarto Plate of an improved Malt Kiln—An Astronomical Chart, exhibiting the Path of the new Planet—A Quarto Plate of Fossil Teeth of the Rhinoceros: engraved by LEE—A Plate relating to the Principles of Pump-work: engraved by LOWRY—Mr. KNIGHT's improved WOULF's Apparatus—A Quarto Plate to illustrate Mr. MARTIN's Paper on the Principles of Pump-Work—Mr. STEEVENS's Instrument for equalizing the Efflux and Pressure of non-elastic Fluids—A View of a Water-Spout, taken from Nature.

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

VOL. XXVI.

For OCTOBER, NOVEMBER, DECEMBER, 1806; *and JANUARY* 1807.

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CONTENTS

OF THE

TWENTY-SIXTH VOLUME.

I. *RECIPE for an elastic and permanent Varnish for Hats, or Helmets of Felt, Gaiters, or other Parts of Dress in Leather, as Boots and Shoes, and which may be also employed with Success in varnishing Cloth and Linen* .. 3

II. *Memoir upon Waters distilled from Plants described as being inodorous, upon the Distillation of Water intended for Chemical Experiments, and upon Alembics. With a Note subjoined by* M. DEYEUX *on the Subject. By* M. DESCROIZILLES *sen.* 6

III. *Extract of a Memoir, by* A. LAUGIER, *on a new Principle in Meteoric Stones. Read in the French National Institute March* 10, 1806 11

IV. *Observations upon the emetic Property of the ligneous Part of Gray Ipecacuanha, and Analysis of that Root. By* M. HENRY, *Professor of Chemistry in the School of Pharmacy, and Member of the Pharmaceutical Society of Paris* 13

V. *Memoir upon Coffee. By* C. L. CADET, *Apothecary in Ordinary to the Imperial Household* 17

VI. *Reply to certain Remarks made by a Writer in the* 5*th Number of the Retrospect of Philosophical, &c. Discoveries, on a Paper in the* 24*th Volume of the Philosophical Magazine. To which are added, Observations on Vision, when terrestrial Objects are seen through a Mist* 29

VII. *Analytical Essay on Asparagus. By* M. ROBIQUET *junior, Apothecary at Vale de Grace* 33

VIII. *Description of an improved Air-Pump. By* T. SYLVESTER, *Esq.* 38

IX. *A Memoir on the best Method of measuring Time at Sea, which obtained the double Prize adjudged by the Royal Academy of Sciences; containing the Description of the Longitude Watch presented to His Majesty the* 5*th of August* 1766. *By* M. LE ROY, *Clock-maker to the King.*

King. Translated from the French by Mr. T. S. EVANS, *F.L.S., of the Royal Military Academy, Woolwich* 40
X. *Extract of a Letter from M.* PROUST *to M.* VAUQUELIN, *upon Porcelain, and the nutritive Use of the Lichen islandicus* 68
XI. *Thirty-second Communication from Dr.* THORNTON, *relative to Pneumatic Medicine* 69
XII. *Observations of M.* BONEFOS, *Assistant Physician of the Infirmary at Perpignan, upon Fumigations with the Oxygenated Muriatic Acid* 71
XIII. *Memoir upon Animal Fat, and some Medicinal Preparations which are administered through that Medium. By M.* VOGEL 72
XIV. *Report made to the Class of Physical and Mathematical Sciences of the French Institute on the 6th of January* 1806, *by M.* PINEL, *upon the advantageous Results obtained by M.* DESGENETTES, *from the Use of Fumigations of Oxymuriatic Acid* 81
XV. *On the Acetic Acid and its Ether. Extracted from a Letter of M.* GEHLEN *to M.* GUYTON 85
XVI. *Observations on the two Preparations of Acetic Ether. By M.* HENRY, *Professor of Pharmacy in the Pharmaceutical School of Paris* 86
XVII. *Proceedings of Learned Societies* 88
XVIII. *Intelligence and Miscellaneous Articles* 90—96
XIX. *Memoir on the Saccharine Diabetes. By Messrs.* DUPUYTREN *and* THENARD 97
XX. *Memoir upon Animal Fat, and some Medicinal Preparations which are administered through that Medium. By M.* VOGEL 105
XXI. *Memoir upon the Acetic Acid. By M.* TROMSDORFF 111
XXII. *Account of a Discovery of Native Minium. In a Letter from* JAMES SMITHSON, *Esq. F. R. S., to the Right Honourable Sir* JOSEPH BANKS, *K.B. P.R.S.* 114
XXIII. *Analytical Essay on Asparagus. By M.* ROBIQUET *junior, Apothecary at Vale de Grace* .. 115
XXIV. *On the inverted Action of the alburnous Vessels of Trees. By* THOMAS ANDREW KNIGHT, *Esq. F.R.S. In a Letter to the Right Honourable Sir* JOSEPH BANKS, *K.B. P.R.S.* 120
XXV. *A Memoir on the best Method of measuring Time at Sea, which obtained the double Prize adjudged by the Royal Academy of Sciences; containing the Description of the Longitude Watch presented to His Majesty the 5th of August* 1766. *By M.* LE ROY, *Clock-maker to the King,*

King. Translated from the French by Mr. T. S. EVANS, F.L.S., of the Royal Military Academy, Woolwich 129
XXV. *Chemical Examination of the native Cinnabars of Japan, Newmarktel, and Idria.* By M. KLAPROTH 146
XXVI. *Upon the Affinities of Bodies for Light; and particularly upon the refractive Powers of different Gases* 151
XXVII. *Memoir upon living and fossil Elephants.* By G. CUVIER 158
XXVIII. *Letter of Dr.* DE CARRO *to Professor* PICTET *of Geneva, on the Mineralogy of the Island of Ceylon* 169
XXIX. *On Music.* By Mr. JOHN FAREY 171
XXX. *Notices respecting New Books* 176
XXXI. *Proceedings of Learned Societies* 179
XXXII. *Intelligence and Miscellaneous Articles* 189—192
XXXIII. *A Memoir on the best Method of measuring Time at Sea, which obtained the double Prize adjudged by the Royal Academy of Sciences; containing the Description of the Longitude Watch presented to His Majesty the 5th of August* 1766. *By* M. LE ROY, *Clock-maker to the King. Translated from the French by* Mr. T. S. EVANS, F.L.S., *of the Royal Military Academy, Woolwich* 193
XXXIV. *Memoir upon living and fossil Elephants.* By M. CUVIER 203
XXXV. *Memoir upon a Process employed in the ci-devant Maçonnais of France, to avert Showers of Hail, and to dissipate Storms.* By M. LESCHEVIN, *chief Commissary for Gunpowder and Saltpetre at Dijon* 212
XXXVI. *On Canal Track-Boats.* By ROBERTSON BUCHANAN, *Esq. Civil Engineer, Glasgow* 219
XXXVII. *Analysis of the Substance known by the Name of Turquoise.* By M. BOUILLON LAGRANGE .. 220
XXXVIII. *The Bakerian Lecture on the Force of Percussion.* By WILLIAM HYDE WOLLASTON, *M.D., Sec. R.S.* 229
XXXIX. *History of Astronomy for the Year* 1805. *By* JEROME DE LALANDE 237
XL. *Report of Surgical Cases in the Finsbury Dispensary from the* 1st *of September to the* 1st *of November* 1806; *with Observations on two Cases of Hernia which proved fatal. Communicated by* JOHN TAUNTON, *Esq. Surgeon to the City and Finsbury Dispensaries, and Lecturer on Anatomy, Surgery, &c.* 253
XLI. *Thirty-third Communication from Dr.* THORNTON, *relative to Pneumatic Medicine* 257
XLII. *Notices respecting New Publications* 264

XLIII. *Pro-*

CONTENTS.

XLIII. *Proceedings of Learned Societies* 266
XLIV. *Intelligence and Miscellaneous Articles* 276—288
XLV. *Discovery of a new Vegetable Principle in Asparagus* (Asparagus sativus *of Linnæus*). *By M.* VAUQUELIN 289
XLVI. *Upon the Affinities of Bodies for Light; and particularly upon the refractive Powers of different Gases* 292
XLVII. *Memoir upon living and fossil Elephants. By M.* CUVIER 302
XLVIII. *Description of an Autocratic Cock, useful in Breweries, Distilleries, &c. By* JOSEPH STEEVENS, *Esq.* 314
XLIX. *Description of Mr.* ARTHUR WOOLF'S *improved Piston for Steam-Engines* 316
L. *On the Food of Plants. By the Rev.* JOSEPH TOWNSHEND, *Rector of Pewsey, Wilts* 317
LI. *Letter from* THOMAS KEITH, *Esq. Secretary to the Master of His Majesty's Household, &c., respecting Mr.* BONNYCASTLE'S *Treatise on Plane and Spherical Trigonometry* 329
LII. *On the Cultivation of the Poppy. By* T. COGAN, *M.D.* 331
LIII. *History of Astronomy for the Year* 1805. *By* JEROME DE LALANDE 347
LIV. *Method of ascertaining whether Wines are adulterated with Litharge. By M.* NAUCHE, *Physician* .. 362
LV. *Proceedings of Learned Societies* 363
LVI. *Intelligence and Miscellaneous Articles* .. 373—375

THE

THE

PHILOSOPHICAL MAGAZINE.

I. *Recipe for an elastic and permanent Varnish for Hats or Helmets of Felt, Gaiters, or other Parts of Dress in Leather, as Boots and Shoes, and which may be also employed with Success in varnishing Cloth and Linen*.*

First Operation.

It is necessary, in the first place, to free the hats, or other articles of felt, from all the gum which they may contain. This may be easily effected by washing them in warm water, and afterwards pressing them. Before they are perfectly dry, they must be placed on moulds in order that they may be preserved in their proper shape, and be without wrinkles, —a very essential requisite. New leather, as well as old, must be scraped in order to clear its superficies from the wax or grease with which it is impregnated. Colophony, or resin in powder, laid upon a coarse brush, also removes the grease perfectly well.

Second Operation.—All felt hats have a kind of down or nap, of which they must be cleared, when dry, by means of pumice-stone; and every part of the hat where the varnish is to be applied must be smoothed in this manner. Leather must be smoothed in the same manner also to remove all inequalities, and even the marks of the scraper.

The same method must be pursued with cloths or linens.

Third Operation.—The down being removed in the manner above described, a coat of the black varnish, to be afterwards mentioned, must be laid on the articles to be varnished. They must be allowed to dry well upon their moulds, that they may not assume any wrinkles, which prevent the proper distribution of the varnish.

* From *Bibliothique Physico-Economique,* May 1806.

Fourth Operation.—This first coat of varnish being perfectly dry, the pumice-stone must be again resorted to, in order to remove any small inequalities which may remain.

Fifth Operation.—When the air is dry and warm, a second coat of the black varnish must be applied, and also polished with the pumice-stone.

Sixth Operation.—The finishing hand must now be put to the article by laying on the varnish to be afterwards described, taking care to employ for this purpose a small and compact pencil, in order to spread the varnish uniformly and equally.

When the first coat of varnish is well dried, it must be sprinkled with pumice-stone reduced to fine powder, and then rubbed all over with a wet sponge, or a piece of fine linen rag also wetted, in order to render the varnish perfectly smooth; or in place of pumice-stone, with tripoli soaked in oil and rubbed with the palm of the hand. As to the second and last coat of varnish, it must be polished when well dried, by sprinkling it with starch and rubbing it with a piece of old linen rag, which will give it a very fine lustre.

In the event of the varnish being tarnished, or losing its lustre by long usage, in order to restore it, place the articles of felt or leather in boiling water for a minute, then let them dry thoroughly, sprinkle them with starch, and rub them with a piece of dry linen, and they will resume their former lustre.

Preparation of Linseed Oil, under the Denomination of Oil of Marmite.

Take Linseed oil	-	15 pounds.
Umber	-	4 ounces.
Red lead	-	1 pound 8 ounces.
White lead	-	2 pounds 4 ounces.

Put the whole in a pot placed upon a coal fire; boil it for 36 or 40 minutes; stir it from time to time with a wooden spatula; and care must be taken that it is neither too little boiled, nor viscous from being too much.

Upon taking the pot off the fire, throw in a piece of bread, both

both crust and crumb, of the size of a small loaf. Cover it, and let it cool for 24 hours. The oil thus prepared is made use of for various purposes.

Composition of the Black Varnish.

1. Take of black umber 2 pounds 13 ounces; cut it into small pieces, and place them in a frying-pan upon a very brisk fire, and roast it like coffee for about three quarters of an hour; bruise it afterwards upon a marble slab, by mixing it in the manner of painters, with a little boiled linseed oil, and keep it in a stone pot.

2. Take three pounds of verdigrise; reduce it to an impalpable powder; mix it with the boiled linseed oil; then put it into the stone pot which contains the umber.

3. Take of lamp-black one pound, mix it also with boiled linseed oil, and after putting it also into the stone pot, blend the whole well together.

This is the mixture made use of to varnish articles of felt, cloth, or leather, observing that when leather is to be varnished it is essential to give it previously two or three, and sometimes even six coats of linseed oil; it must be well dried each time, in order to extract the grease from the leather, wax, or fish oil, in order that the varnish may incorporate with the leather more easily. This precaution must be made use of with soft boots, when placed upon moulds or boot-trees; and, without even taking them off, as many coats of varnish may be laid on as necessary.

Method of preparing the Varnish.

Take of Prussian blue	-	12 ounces.
Indigo	- - -	12

Bruise these two separately upon a marble slab; mix them up with a little oil, and put them in a pot by themselves.

Afterwards take of gum-copal	-	8 ounces.
Prepared nut oil		5
Spirit of turpentine		14

Put the gum-copal, bruised in a matrass with a large neck, upon a strong fire, but not flaming, taking care to stir it often, and to keep it uncovered. We know that the gum is totally dissolved when the smoke has entirely abated in the matrass; pour into it, by little and little, prepared nut oil, stirring it in order to incorporate the whole completely. Afterwards, and in the same manner, the spirit of turpentine is poured in, and the mixture is then taken from the fire, filtered, and cooled; it is then made use of to grind with the Prussian blue and indigo in small quantities at a time, and the whole is well mixed together.

This mixture forms the fine varnish for the purposes indicated.

II. *Memoir upon Waters distilled from Plants described as being inodorous, upon the Distillation of Water intended for Chemical Experiments, and upon Alembics. With a Note subjoined by M.* Deyeux *on the Subject. By M.* Descroziilles *sen.**

Among those whose professions lead them to the preparation and application of medicines, there are various opinions upon the efficacy of waters distilled from plants described as inodorous. I shall by no means decide upon the question in a medical point of view; I shall only request that before entering on the discussion the question be stated in precise terms.

I was induced last summer to repeat some experiments on the distillation of inodorous plants, in consequence of reading some observations on the subject in a former number of the *Annales de Chimie* †.

The observations of the learned author in the above memoir lead me to offer some hint which may tend to give a new direction to his useful researches.

Waters distilled from a great quantity of inodorous plants yield a smell so much the stronger, the oftener that the produce of the first distillation has been cohobated upon fresh quantities of the same plant; but I do not think it has been yet demonstrated that such waters are susceptible of putre-

* *Annales de Chimie*, tome lvii. p. 175.

† See page 3, of our last volume.

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faction when they have been distilled in B. M.; and I hope to be able to prove by and by, that the putrefaction to which even waters distilled from aromatic plants are subject, is owing to a portion of the decoction of the plants being extravasated, or forced into the tubes of the apparatus, when distillation in the open fire is made use of.

Before proceeding to explain some of the probabilities upon which I rest my opinion, that down to the present moment the greater part of such waters as are distilled from plants in the open fire have been adulterated with their respective extracts, I consider it my duty to say a few words on the subject of alembics. My hints are the results of a course of reflections and experiments on the improvement of the above apparatus, in which I have been constantly occupied since the year 1775, a space of more than 30 years.

The old chemists had certainly observed, that during the ebullition of water a part of that fluid is thrown to a height of more than 30 centimetres in a multitude of small drops or globules, which can with difficulty be perceived by the naked eye. They must have also observed, that during the time of distillation the decoction of plants has the property of swelling up when boiling, and of being raised with the greatest facility into the neck of the cucurbits; they consequently contracted their distilling apparatus to moderate the tendency of the boiling liquor to bubble up, and they interposed between the cucurbit and the head cylindrical tubes, either straight or spiral, or zigzag, &c., and of a length equally inconvenient and unreasonable.

Baumé, to whom, in spite of his obstinacy in rejecting the theory of pneumatic chemistry, we are indebted for a great number of useful observations, has particularly contributed to the reform of the construction of alembics; but in bringing the head so near the body of the alembic as he has done, he has fallen into an opposite extreme, the inconvenience of which did not occur to me until within these two years. At the above period I interposed between the body and the head of my alembic a cylindrical funnel 40 centimetres high. Besides, I gave an inverse inclination the length of a decimetre to the beak, at the place where it is soldered to

 the

the head of the alembic. I had a long time before suppressed the ridge or furrow in the latter.

By means of these precautions, and of some others which I am about to communicate, I obtained distilled waters, which never putrefied although the plants were distilled in the open fire, and with a strong ebullition; and although I afterwards put these waters into bottles with ground-stoppers exposed to the rays of the sun, and in an apartment constantly heated by a stove, they preserved all the delicacy of their aroma and their limpidity.

We know that the orange flower water, prepared at Paris with great care, ought to be kept in a cool place, and in the shade, in vessels not closely corked, if we wish to prevent their undergoing any alteration; but we know also that the orange flower water, which comes from our southern departments in large bottles of very thin copper, is preserved a much longer time without alteration, although they are well corked. I have reason to believe that this difference arises from the distillers of Provence making use of alembics of the old form with long necks, while those of Paris employ the modern flat alembics. I know it will be said that a little spirit is mixed with the Provence orange water; but I do not think, if that were the case, that the quantity of spirits thus added is sufficient to stop, for so long a time, the putrefaction of a distilled water, which contained a little of the extract from which it was produced.

Some chemists complain that pure water, when distilled once, still shows traces of the muriatic acid when tried by the nitrate of silver; they consequently prescribe that this water should be redistilled. I request them to take the precautions I have here suggested, and they will obtain at the first distillation purer distilled water than they could otherwise do by two distillations.

Although these observations are very succinct, I flatter myself they will excite the attention of such chemists or artists as make use of alembics; and the question of the properties of waters distilled from plants being placed in this new point of view, there must consequently be more certainty in the observations of such medical gentlemen as have a partiality for these kinds of medicines.

Before

Before concluding my observations on distillation by alembics, I shall briefly notice the various improvements I have successively made on these articles, a great number of which I have made both for myself and my friends.

Since the year 1775, I have used cylindrical instead of serpentine tubes, sometimes straight, flattened, inclined, or vertical, but always so disposed as to accelerate the distillation greatly by their largeness; and being perfectly well polished, I could easily clear them, after every new operation, from every foreign taste or smell.

Before any thing had been announced relative to what might be made of the heat contained in the vapours which form by their condensation the distilled liquids, I presented to the National Institute an alembic, where this heat was rendered profitable in producing the rectification of the produce of an anterior distillation, and in preparing the subsequent distillation.

The following are the principal advantages of the alembic of which I am about to publish a description; and in the construction of it I have observed every possible precaution, in order to œconomize the heat and to facilitate the operations.

Every part of it must be of pewter.

We may distil in it, without inconvenience, 25 given measures of liquid, or only a single 25th part of any measure, and afterwards reduce it to a fourth part by evaporation.

Each distillation, without augmenting the expense of fuel, and without delay, may be accompanied with a rectification; and immediately after this double operation, the cucurbit being emptied by a syphon, a cock adapted to it evacuates the liquor intended for a subsequent distillation in a state very near the boiling temperature; the subsequent distillation is consequently effected much more quickly, and with much less fuel.

By means of four watch-glasses and three small tubes, every thing is seen which passes inside the body of the still and its cylinder, the rectifying vessel, and the preparatory one.

After

After the most nauseous distillation, all the parts of the alembic may be so cleaned in a quarter of an hour, that the most delicate liqueur may be immediately distilled in it.

Such is the result of the experiments of more than thirty years. I alone know what this experience has cost me; I shall, however, consider myself in some measure indemnified for my labours, if these hints shall be of any service to the progress of the arts and sciences.

Note by M. Deyeux.—I do not rightly understand what the author means in the beginning of the above memoir. He seems desirous that the question relative to the efficacy of waters distilled from inodorous plants should be stated in precise terms. It appears to me that this question cannot be presented in the form of a problem, since it has been proved, by a multiplicity of experiments, that it is highly improper to doubt the efficacy of waters distilled from these kinds of plants.

As to the observations relative to the necessity of distilling these waters in B. M., or at least of rectifying them in this manner, I am not only of opinion, that in some cases such a process may be useful, but I also think that the above method is not always sufficient to obtain distilled waters in a state which may bid defiance to all alteration. I have often had occasion to distil waters in B. M.; and in spite of this precaution I have remarked that some of these waters become turbid at the end of a month, and that they often acquired a disagreeable smell and taste.

To conclude: As the author announces a work upon distillation in general, and as he also promises us some details upon the best form of alembics, I shall wait for the publication of his memoir, which will, without doubt, contain the results of experiments made with that care and acuteness which characterize every production of M. Descroizilles.

III. *Extract of a Memoir, by* A. Laugier, *on a new Principle in Meteoric Stones. Read in the French National Institute March* 10, 1806 *.

Since the period when Mr. Howard called the attention of the learned to the substances called meteoric stones, every chemist who repeated his experiments has met with the same results. All agree that they had discovered in these stones, at whatever time or place they had fallen to the ground, the same principles; viz. silex, iron, magnesia, sulphur, nickel, and accidentally traces of lime and alumine. Upon comparing the results of their analyses, we find that the above principles were found very nearly in the same proportions in each experiment. M. Proust has latterly discovered the existence of manganese in such meteoric stones as he analysed; and the fact has since been confirmed by other chemists.

M. Laugier, assistant-professor of natural history, is charged with the analyses for the Museum of Natural History. Upon analysing a meteoric stone which had fallen at Verona in 1663, he discovered a principle hitherto unknown to exist in these substances. This new principle, which is chrome, is the subject of M. Laugier's memoir.

" It is probable," he says, " that I should not have discovered the chrome, had I not made use of a mode of analysis totally new. Hitherto the acids have been always resorted to; and this is, perhaps, the most natural and convenient process; on the present occasion, however, I employed caustic alkali. This has the peculiar advantage of showing the presence of chrome, however small in quantity; whereas it is almost impossible to perceive it when it is dissolved in the acids, particularly since it is then mixed with a great quantity of iron, manganese, &c."

M. Laugier had recourse, therefore, to the following process for separating the chrome; and he determined its proportions pretty exactly: he melted one part of the stone

* From *Annales de Chimie*, tome lviii. p. 261.

with

with three parts of caustic potash; he diluted the mass with distilled water, which became of a yellow or greenish yellow colour on account of the manganese; the latter was precipitated on being allowed to rest, and the solution then resumed its pure yellow colour. This solution, joined with the water of the washings, and sufficiently diluted in water, which is necessary to prevent the precipitation of the silex, is hypersaturated with a slight excess of nitric acid. He poured into this solution nitrate of mercury at the *minimum*, recently prepared; there was immediately formed an orange red precipitate or chromate of mercury, which he allowed to rest until the evening of next day. He decanted the liquor, and added several fresh waters in order to wash the precipitate, and when the latter had become quite tasteless he poured the whole into a platina crucible; the water was evaporated, the chromate of mercury became dry, and was soon decomposed into a green oxide of chrome, the quantity of which amounted to one hundredth of the stone employed.

As this Verona stone is similar in its physical properties to all the other meteoric stones, the author of the memoir thought himself bound to examine if the latter contained chrome as well as the former. He successively examined fragments of stones which had fallen at Ensisheim, l'Aigle, Barbotan near Bourdeaux, and at Apt; and he also found chrome in these four stones.

It is also remarkable that the Verona stone in which he first discovered this metal contained the least of the whole five; the others contained one hundredth part, while that only contained half that quantity of chrome.

M. Vauquelin has made a very favourable report of the above memoir. According to its author, M. Laugier, the following conclusions may be drawn:

1. That the five meteoric stones which fell at Verona, Barbotan, Ensisheim, l'Aigle, and Apt, contain the metal called chrome in the proportion of about one hundredth part, exclusive of the other principles already ascertained by other chemists.

2. That it is very probable that all the meteoric stones contain-

contain this principle, since they resemble each other so much in their chemical and physical properties, and as it seems they have all the same origin.

3. That in most cases it is indispensable, in making a correct chemical analysis, to treat the same substance both by the acids and the alkalis, since it has been demonstrated by experiments that a principle which the acids could not discover, may be ascertained by means of the alkalis.

IV. *Observations upon the emetic Property of the ligneous Part of Gray Ipecacuanha, and Analysis of that Root. By M.* HENRY, *Professor of Chemistry in the School of Pharmacy, and Member of the Pharmaceutical Society of Paris*.*

AMONG those authors who have written on the subject of ipecacuanha, we may reckon Adrien-Helvetius, Boulduc, Geoffroy, Neumann, Cartheuser, Lewis, and more recently Lassone jun. and Cornette, who, in the Memoirs of the Royal Society of Medicine of the 31st of August 1779, have given analyses of this root, and have demonstrated that "the ligneous part is very nearly as emetic as that which is separated from it."

Last of all, Murray, in the first volume of his Materia Medica (*Apparatus Medicaminum*), p. 804, relates, without asserting any thing positively, "that, lately in France, (he quotes Lassone and Cornette,) it has been remarked that the ligneous part of this root was equally efficacious with the cortical part; that it contained as much resin and extract; and that an equal dose of it excites vomiting and allays dysentery."

In spite of the experiments of Lassone and Cornette, it seems that the ligneous part is never used, and apothecaries are condemned who are not careful of rejecting it from their preparation of the ipecacuanha powder.

Nobody, so far as I know, has repeated the experiments

* From *Annales de Chimie*, tom. lvii. p. 28.

announced

announced by Murray, and extremely well described in the memoir of Lassone jun. In order to set aside every doubt which may be yet raised, I shall give an account of the experiments lately made in the hospitals of Paris.

I made choice of gray ipecacuanha, the ligneous part of which I separated carefully, so as to leave no doubt of any of the cortical part remaining. I collected 500 grammes (one pound), which I pulverized. The powder was whitish: I sent portions of it to the different chief apothecaries of the Hotel-Dieu, the Hospital of St. Anthony, and the Hospice de la Maternité.

The experiments made in these different places proved that this part was, as announced by Murray, equally emetic and purgative. M. Chaussier, professor in the School of Medicine and physician to the Hospice de la Maternité, assured me that the wood of ipecacuanha had succeeded every time he employed it.

The experiments were pursued for a month at least, and the results were constantly the same.

Messrs. Latour and Morisset, of the Hotel-Dieu and the Hospital of St. Anthony, deserve my warmest thanks; they traced the effects of ligneous ipecacuanha with the utmost precision, and they assured me they were constantly similar.

In confirming the experiments of Messrs. Lassone and Cornette, which establish the emetic property of the ligneous part of ipecacuanha, I do not mean to announce any new fact; but, as I had often heard of its inefficacy, I thought the best method would be to analyse ipecacuanha carefully, in order to put an end to all doubts on the subject. I shall now proceed to communicate my analysis.

After having carefully separated the cortical and ligneous parts, I treated each separately, by sulphuric ether, rectified alcohol, and water.

Ten grammes of the cortical part, digested in 150 grammes of ether, slightly coloured the liquor: the ether evaporated to dryness; there remained upon the sides of the vessel seven decigrammes of resinous matter, inflammable and insoluble in water, and presenting all the characters of the resins.

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An equal quantity of the cortical part yielded by the medium of rectified alcohol six decigrammes of resin.

This same part of the root in the same quantity, digested in 200 grammes of distilled water, furnished by evaporation a dry extract weighing one gramme eight decigrammes. This extract is entirely soluble in water, of a citrine colour, and of a slightly bitter taste.

Lastly, the same substance, treated by boiling water in the same proportions, yielded a greater quantity of extractive matter by nearly two grammes five decigrammes.

I now operated upon the ligneous part in the same manner with the same agents, and I obtained the following results:

1st, By the sulphuric ether, three decigrammes of resin.

2d, By rectified alcohol, two decigrammes five centigrammes of resin.

3d, By cold water, one gramme four decigrammes of dry extract.

4th, By boiling water, two grammes and eight decigrammes of extractive matter.

It often happens that alcohol dissolves more soluble matter than ether; this difference is owing to the fluid taking off with the resin a small quantity of extract, but it is easy to separate it.

In order to ascertain which of those two parts excited vomiting, or if both of them had equally an emetic and purgative property, I tried each of them separately in the hospitals of Paris.

From the observations of medical practitioners it results that the resinous part, in a dose of four grains, possesses a decisive emetic and purgative property.

The method of administering it consists in mixing the resin with a small quantity of sugar, and adding thirty or fifty grammes of water.

As to the extractive part, the effects were the same; but the dose was greater, namely, about six or eight grains.

After this short explanation it is easy to see, that if the cortical part of the root of ipecacuanha and the ligneous part both contain the same principles, they should enjoy the

same

same properties, although there is a very small difference between the proportions of resin and of extract they furnish.

The cortical part, exposed to the action of boiling water, swells up like Bassora gum; treated with the nitric acid, in the proportion of six parts of acid, of thirty degrees, to one of the bark, there is a good deal of nitrous vapours liberated, and malic acid is obtained as the residue: this leads us to regard this matter not as a resin, but as a particular gum mixed with resin and extract, which alone give it an emetic property; for I am certain that this matter has no property at all, after having experienced the action of alcohol and water.

But there is a fact of which no author, to my knowledge, has spoken. Every time we boil the cortical part, the decoction becomes turbid, and a kind of cloud is formed in the liquor: if it is filtered a whitish matter is deposited, which at first I took for glue, but when well examined it presented some of the properties of caoutchouc. It becomes coloured after some time without experiencing any alteration; is easily dissolved in ether and alcohol at 60 degrees. This particular matter, which I am certain is an elastic gum, retains a great quantity of feculum.

An infusion of the cortical part yields by the re-agents the following results:

It strongly reddens the colour of turnsole.

The solution of glue does not change the transparency of the liquor.

The sulphuric acid makes it very turbid.

The nitrate of silver forms a white precipitate.

The muriate of barytes forms no precipitate.

The tartrites of potash and of antimony form no precipitate.

The oxalate of ammonia furnishes a slight precipitate.

The same results are got from an infusion of the ligneous part.

Decoctions of the ligneous and cortical parts act in the same manner.

These experiments seem to prove that the root of ipecacuanha contains, 1st, free acid of a vegetable nature decomposable

posable by the action of caloric; 2d, different salts with a base of lime: in short, that the cortical and ligneous parts contain very nearly the same materials; which confirm the experiments of Lassone and Cornette, and those recently made in the hospitals of Paris.

This root, distilled in the naked fire in a small glass retort well luted, yielded the following products; water, oil, acetous acid containing oil, and elastic fluids.

In my last analysis I incinerated the root of ipecacuanha: thirty grammes of it placed in a crucible were reduced to a small quantity of ashes, which, being leyed, yielded very little saline matter, about fifteen centigrammes of sulphate of lime, mixed with a small quantity of a muriate of a kind I did not know.

Such is the result of my experiments. I hope they will remove all uncertainty, and be productive of some utility.

V. *Memoir upon Coffee.* *By* C. L. CADET, *Apothecary in Ordinary to the Imperial Household**.

CHEMICAL researches are often directed to the analyses of substances more curious than useful; while those which are familiar to every one, and in daily use, are but too much neglected. Such were the considerations which induced me to undertake the following experiments upon that salutary article of nourishment—coffee.

When we reflect that this colonial produce takes out of France more than thirty millions annually, and that it occasions an immense consumption of sugar, always to the advantage of foreigners, it certainly becomes chemists to examine its nature and explain its medicinal virtues.

Bourdelin, Geoffroy, Rihiner, and some others, have already published analyses of coffee; but their labours have taught us nothing, because science, at the time they wrote, was not far enough advanced, and they wanted the most useful re-agents. Without thinking myself wiser than they,

* From *Annales de Chimie*, tom. lviii. p. 266.

I shall perhaps be more fortunate in throwing light upon this substance, in a manner as yet new.

Examination of dry Coffee.

Dry Coffee treated with Water.—When boiling water is poured upon dry coffee, such as we meet with in the shops, the water becomes yellowish green *. If the action of heat is continued, the decoction becomes brown, and a slight scum is formed, which remains insoluble; when filtered it passes very clear, and becomes turbid upon cooling. A little caustic potash poured into this decoction makes it more brown. Ammonia produces the same effect. Lime water produces an abundant flaky precipitate; sulphate of iron converts it into a black ink. Solution of gelatine does not become turbid upon being mixed with this decoction. The oxymuriatic acid only partly discolours it; and if an alkali is added to the mixture it becomes red.

Distillation.—I distilled eight pounds of water over a pound of dry coffee. I obtained a very aromatic water, on which floated some drops of a concrete oil like that of the *myrica cerifera.* The decoction remaining in the alembic was viscous. I diluted it a little with water, and poured alcohol into it; an abundant matter was precipitated, which being collected on the filter was soluble in water, and had all the characters of a mucilage. The coffee from which the water had been distilled, dried in a stove, and digested in alcohol, furnished a tincture which precipitated by means of water.

The watery decoction of dry coffee does not redden the blue vegetable colours: it even gives a green colour to turnsole tincture. Every chemist who has analysed coffee before my time has said that the decoction holds a free acid suspended in it, which reddens the blue vegetable colours. Geoffroy has even gone the length of asserting that water distilled from coffee in B. M. became very acid. I have tried five different varieties of coffee, and repeated my ex-

* When the coffee is newly gathered, its decoction is of a superb emerald green. A lac may be then made of it. M. Dupont de Nemours assured me that he made use of it as one of the tints for colouring maps.

perimente

periments more than twenty times, but the decoction never was acid.

It decomposes sulphate of alumine and precipitates the earth from it, which it colours feebly.

Dry Coffee treated by Alcohol.—An infusion of alcohol and dry coffee, even in the cold, becomes slightly coloured, and holds in solution a very abundant extracto-resinous principle. If water is poured in, the solution becomes milky, and the resin is precipitated of a dirty white. With a solution of sulphate of iron the precipitate is green; with the muriatic acid it is fawn-coloured. Coffee, when exhausted by alcohol and afterwards treated with water, still furnishes some extract and mucilage.

It may be concluded from these first experiments that dried coffee contains, 1st, An aromatic principle soluble in water: 2d, A very small quantity of essential oil: 3d, An abundant resin: 4th, A gum in greater quantity: 5th, Gallic acid, but no tannin: 6th, Extractive matter: and, 7th, A little alumine.

Observations.

If the warm filtered decoction becomes turbid on cooling, it is because it holds in solution, on account of the heat, a little resin. The alkalis make it brown; the usual effect of these re-agents upon vegetable decoctions. Lime water precipitates it, because, on the one hand, gallate of lime is formed; and, on the other hand, the extracto-gummous matter unites itself to the earth and takes it down with it. There is even sulphate of alumine in it. Spirit of wine separates the mucilage from it, because the gums are not soluble in alcohol; and water precipitates the alcoholic tincture, because water does not dissolve resins. This precipitate becomes white when water is used, on account of its extreme minuteness; green by the sulphate of iron, because it is mixed with the gallate of iron; fawn-coloured by the oxymuriatic acid, because oxygen acting on resin sets free a little carbon. The insoluble scum which is formed upon the decoction is a vegetable albumen coagulated by boiling water. To obtain this it is necessary that the water stand some time cold upon the coffee before heating it.

General Proportions of the above constituent Parts.—Although it may be of little service to ascertain the proportions of the immediate principles of coffee, since these proportions must vary on account of the greater or less maturity or richness of the article, yet I considered it my duty to estimate these proportions as nearly as possible. After several comparative experiments, I found that eight ounces of coffee yielded about one ounce of mucilage, one drachm of resin, one drachm of extracto-colouring matter, 3½ drachms of gallic acid, five ounces 3½ drachms of *parenchyme*, and ten grains of vegetable albumen.

I compared the decoctions and tinctures of the three dried coffees of Bourbon, Moka, and Martinique. Bourbon and Martinique coffee seemingly furnished the same principles in the same proportions. That of Moka differs essentially from the rest. Its decoction was much less saturated, its alcoholic tincture was higher coloured than similar tinctures of the Bourbon and Martinique coffees; it contains less gum, less gallic acid, more resin, and more aroma than the others.

Roasted Coffee.

In order to ascertain the changes produced upon coffee by roasting, I examined the phænomena which took place while it was burnt in the open air.

At first it augments in volume as it is penetrated by caloric; it crackles and becomes fawn-coloured; the pellicle which envelops the bean is detached; as it is very light and slender, it flies off with the least breath. The coffee then emits a very agreeable aromatic flavour. This vapour increases in its intenseness, the bean smokes and becomes brown: the smell then changes and becomes slightly empyreumatic; the coffee exudes and becomes oily at its surface*; it ceases to smoke; and if the action of the fire is continued the coffee is charred.

* M. Parmentier enveloped roasted coffee, when in a state of exudation, in filtering paper, which imbibed the oil, and remained greasy and transparent upwards of a year: this supposes the existence of a fat oil in this grain. I could not obtain any such oil, however, in a separate state, either by expression, ebullition, or the caustic alkalis.

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The interval between roasted coffee becoming higher coloured and its being charred, is so long, that it is extremely difficult to determine the point when it is necessary to stop, in order to preserve the agreeable properties of the beans; but, in order to approximate to this point, so important to ascertain, I reckoned three distinct epochs in the roasting: 1st, The bean loses its natural colour and passes to that of bread-raspings or dry almonds: 2d, The coffee becomes of the brownish red colour of Indian chestnuts: 3d, Although almost black, it is not, however, charred.

I took six ounces of Martinique coffee and divided them into three parts, which I roasted separately, and each of them to one of the above three degrees.

The two ounces, lightly roasted, of the colour of dry almonds, lost two drachms in the fire: this I shall call the first degree.

The two ounces roasted of the chestnut colour lost three drachms: this was my second degree.

The two ounces roasted black lost three drachms forty-eight grains: third degree.

No. 1. passed through the mill with difficulty *. A cold infusion of it contained tannin, and precipitated the solution of gelatine. Its taste was strongly aromatic †; its flavour was that of almonds; it had no bitterness, and had a decisive green colour. A warm infusion had the same aro-

* M. Cadet de Vaux, my uncle, has remarked, that to grind roasted coffee is not the best method, by bruising it in a mortar much more of the aroma is preserved.

† The desire of retaining the aroma which is dissipated at too strong heat has suggested two processes, not altogether useless; the one is in use in India and France, and consists in putting a little fresh beer upon the coffee when it begins to colour in roasting in the cylinder: as much beer must only be used as will slightly varnish the surface of the grains. The beer retains a part of the essential oil which would have been evaporated. This is not a bad method, but it sometimes gives the coffee a flavour which does not suit every one's palate. The other process consists in spreading the roasted coffee while hot and exuding, upon white paper, and then strewing it lightly over with sugar: the sugar absorbs the oil of the coffee and retains the aroma. This method, in my opinion, does not add to the agreeable qualities of the coffee, and makes one uncertain what quantity of sugar to put into a cup of coffee.

matic qualities; its taste resembled that of almond cake: it was not bitter, and was not so green as the former.

No. 2. was more easily ground. A cold infusion of it furnished less tannin, its aromatic flavour was weaker, its taste more sugary; it was neither bitter nor green. A warm infusion neither yielded more taste nor more aroma.

No. 3. was very easily powdered. A cold infusion was almost free from aroma; its taste was empyreumatic, and a little bitter; it formed a precipitate scarcely perceptible with solution of gelatine: the warm infusion was more bitter, more empyreumatic, and the aroma was more distinct.

All these infusions contained mucilage and gallic acid, but in an inverse progression to the tannin; because the proportions of gum and acid increased with the roasting, whereas the tannin diminished.

M. Bouillon-Lagrange, in a very excellent paper upon gall-nuts, has already considered the gallic acid as a modification of tannin: these experiments support his opinion.

Roasted Coffee.—As the immediate principles of coffee are not equally soluble or volatile, it was necessary to examine comparatively the cold and warm infusions of the three kinds of coffee, as well as their decoctions.

Infusion in cold Water.—I poured eight ounces of distilled water upon one ounce of roasted and ground coffee; I allowed it to infuse two hours, and I filtered it. The infusion was of a very clear brown; did not redden paper; became black by the sulphate of iron; and slightly precipitated the solution of gelatine. Alcohol separated from it a little mucilage, and gave the infusion the smell of juniper. Moka, Bourbon, and Martinique coffee, presented the same characters.

Infusion in warm Water.—I infused, for a quarter of an hour, one ounce of roasted and ground coffee in eight ounces of water at 70° (158° Fahr.). This infusion did not redden turnsole paper; did not precipitate solution of gelatine; and formed ink with sulphate of iron. Alcohol separated more gum from it than from the cold infusion. The three kinds of coffee acted in the same manner in these experiments.

Decoction.

Decoction.—I boiled two ounces of ground coffee in one pound of water for two hours. The decoction had an infinitely less agreeable and less aromatic smell than the infusion. It did not change the colour of blue paper; did not precipitate the solution of gelatine; and became black with the sulphate of iron. Alcohol separated much more mucilage from it than is found in equal proportions of the infusion. The three kinds of coffee yielded the same results.

If we boil, for a long time in the open air, a filtered and limpid decoction of coffee, it becomes turbid and deposits a black powder, which has been sometimes taken for resin, but it is only a highly oxygenated extract. Physicians and apothecaries have not yet sufficiently well ascertained the action of the atmospheric air upon vegetable decoctions; they might derive some experience of the greater or less energy of certain remedies from experiments on this subject.

Extract of Coffee.—The decoction of coffee, filtered and evaporated to the consistence of an extract, has no longer the aromatic smell of the infusion; its taste is bitter; heated with alcohol the extract colours the liquor, but this colour is not precipitated by water. From this we may conclude that the decoction of coffee, when it is filtered or has rested some time, contains no resin.

Alcoholic Tincture of roasted Coffee.—Roasted coffee digested in alcohol yields a strong coloured tincture, which precipitates, by means of water, a greater quantity of resin as the coffee is dry or green. In green coffee the resinous matter is white; in the tincture of roasted coffee it is fawn-coloured.

Observations.

It results from these experiments that roasting develops in coffee odorous and resinous principles, and forms tannin, which is only soluble in cold water; a very singular phænomenon. The gallic acid manifests itself in coffee at all the temperatures of the water employed to dissolve it. The gum and the colouring extractive matter are more abundant in decoctions than in infusions; but the aromatic principle is more sensible and more agreeable in the latter.

Roasted Coffee distilled.—I distilled several litres of water from roasted coffee; this water was saturated with the aroma of the coffee, and it carried with it some particles of concrete essential oil, like that obtained from the distillation of dry coffee. The re-agents did not demonstrate the presence of any substance in solution in this water.

Infusions and Decoctions compared.—In order to ascertain the different solubility of the principles of coffee, it remained to submit the same roasted powder to the successive action of infusion and decoction. I therefore placed two ounces of coffee in a filter, I poured cold water upon it until the re-agents ceased to indicate to me the presence of any matters in solution. Sixty-eight ounces of cold water must have been employed to clear this coffee of its soluble matter. I divided these washings into seventeen portions of four ounces each, as they came through the filter. All these seventeen portions contained the gallic acid in proportion to their order of priority; the four first contained gum, and the first alone precipitated the solution of size, which showed the presence of tannin.

The coffee, withdrawn from the filter, was dried in a stove; I afterwards poured upon it eight ounces of water heated to 75° (167° of Fahr.); the smell of this secondary infusion was agreeable, but weaker than that of coffee which is prepared for the table: when examined by the re-agents it furnished a little mucilage and a good deal of gallic acid, I found neither tannin nor resin in it.

I took once more the same coffee, washed cold and infused warm as above described, and boiled it in six ounces of water until it was reduced to four. This decoction contained a great deal of gum and gallic acid, little aroma, and yielded by the re-agents no signs of tannin or resin.

Observations.

These experiments prove that cold water clears roasted coffee of the little tannin which it contains, of a part of its extractive matter, of a great part of its aroma; but it only takes off a small portion of its gallic acid and its gum. We have already seen that a warm infusion is more saturated with these last principles, but that its aroma is weaker. In short,

short, it will be recollected that the decoction, when prolonged, dissipates, in a great measure, the aromatic smell, and is highly charged with gum and gallic acid. If there is any resin in it, it is only in suspension; it injures the transparency of the liquor, and it is precipitated by repose.

Ashes of Coffee.—Although it is almost indifferent to know what coffee contains when reduced to ashes, I incinerated about half a pound of it: the ashes were light; washed in distilled water, they only presented a little lime to my analysis, and but very little potash. I sharpened this ley with a little nitric acid, and the filtered solution precipitated in a fine blue prussiate of potash, and was abundantly precipitated by the oxalic acid. Barytes did not alter it; it became white with the nitrate of silver. Thus the ashes of coffee are composed of charcoal, lime, and muriate of potash. I did not think it worth while to estimate the proportions.

I thought I had here terminated my analysis, but M. Parmentier has lately read in the Pharmaceutic Society a long essay upon coffee by M. Payssé, who has already published some very interesting works. It is said in this essay, 1st, That the precipitate formed by the mixture of the decoction of coffee with the sulphate of iron, is only soluble in the nitric, sulphuric, phosphoric, or oxalic acids: 2d, That coffee contains no gallic acid: 3d, That it contains a particular acid, *sui generis*, which the author calls coffic acid, and which he obtained by following the process of M. Chenevix, consisting in making a decoction of raw coffee, filtering it, precipitating it by the muriate of tin, and decomposing the precipitate by sulphuretted hydrogen gas.

The authority of the name of Chenevix, and the exactitude with which M. Payssé makes his observations, induced me to make several experiments to confirm the new facts they had announced.

I boiled, for two hours, two ounces of Bourbon coffee in half a pound of water; this decoction presented me with the same phænomena I had already seen; it assumed a yellowish green tint, which became more lively by the separation of a little albumen, and oxygenated extractive was precipitated. This

This decoction, when filtered, turned into green the aqueous tincture of turnsole.

I mixed a part of this decoction with a solution of sulphate of iron, and I obtained a precipitate of a very deep blue, drawing towards black; I redissolved this precipitate by the oxymuriatic acid, by weak and strong acetic acid, by the tartarous acid, the citric acid, and even by the benzoic acid.

The muriatic acid changed the liquor into yellow; and it resumed its transparency on depositing a heavy precipitate of oxygenated extractive. This precipitate, redissolved by ammonia, gave a fine red brown colour to the liquor.

The immediate precipitate of the sulphate of iron dissolved by the acetic acid acted like the former, the colour only excepted, which was violet blue; it was, besides, redissolved by ammonia. The other acids yielded nearly the same precipitate as the muriatic acid. Their action, in general, followed the ratio of acidities.

I treated in the same manner some precipitate of sulphate of iron obtained by the gallic acid, and the results presented no differences from the former.

I precipitated by the muriate of tin what remained of the coffee in decoction. This salt occasioned a very abundant deposit in the liquor. I washed the precipitate until the washings no longer exhibited any signs of acidity; I afterwards put this metallic compound in a Woolf's bottle, and I poured plenty of distilled water on it. I placed the apparatus of Woolf so as to cause sulphuretted hydrogen gas to pass over the precipitate. As soon as the first portions of gas began to pass, the mixture acquired a brown colour, which became darker in proportion as the liquor was saturated with sulphuretted hydrogen gas. The precipitate was decomposed; there was formed a hydro-sulphuret of tin, and the liberated acid passed into the liquor. This liquor, filtered, was evaporated at a gentle heat until it was reduced to one-eighth. This product, supposed by M, Payssé to be *coffic acid,* seemed to me to be nothing else than gallic acid. Not only did I submit it to the action of all the reagents, comparatively with the acid drawn from gall-nuts

by

by the ordinary method; but, in order to leave no doubt on this subject, gall-nuts by the same process. The muriate of tin formed a more abundant precipitate in the latter than in coffee; this precipitate, decomposed, like the former, by the sulphuretted hydrogen gas, yielded me an acid of the same colour and the same taste, enjoying the same properties, and presenting no difference except in the proportions. I then thought I might conclude that the *coffic acid* does not exist, but that coffee contains less gallic acid than gall-nuts.

It is possible that this gallic acid may present, in its combinations and compounds, some slight shades which make it differ a little from the acid produced from oak-galls, but it is not the less of the same nature. We know that the immediate materials of vegetables, although of the same species and perfectly analogous, are not rigorously identically the same; the gums and the saccharine matters present variations in their physical properties; nevertheless the saccharine substance and mucilage are the same when chemically considered *.

Proust has proved that tannin, obtained from several vegetables, presents differences. It is possible, therefore, that the gallic acid drawn from coffee is not absolutely the same as that of galls; but it is by no means a distinct acid.

Recapitulation.

It seems to be demonstrated by the above analysis that the coffee-berries (as known in commerce) contain mucilage in abundance, a good deal of gallic acid, a resin, a concrete essential oil, some albumen, and a volatile aromatic principle. To these principles we may add those which are found in most vegetables, viz. lime, potash, charcoal, iron, &c.† Roasting develops the soluble principles; but it ought to

* The feculum of potatoes does not resemble that of wheat; the latter differs from that of sago, arum, maize, &c.; nevertheless, all the chemists tell us that it is an amylaceous substance, and they acknowledge the same principal characters in all of them.

† The presence of iron in a vegetable is a common thing, but the presence of iron in a vegetable which contains plenty of gallic acid, without this acid

to be moderate if we wish to preserve the aroma, and not decompose the acid, the gum, and the resin.

Roasting adds a new principle, which is tannin, in very small quantity; the cold infusion is very aromatic, but a little charged with mucilage and gallic acid; the warm infusion preserves the aroma, and the principles dissolved are in such proportions as to please the palate; the decoction has little aroma, and is strongly saturated with gum and gallic acid; the resin itself may perhaps be suspended in it; it is less agreeable than the infusion.

Bourbon and Martinique coffee present no differences between them; but that of Moka, as already remarked, is more aromatic, less gummous, and more resinous. It is probable that the resin of coffee, like that of most of the astringent vegetables, has peculiar medicinal properties. As we cannot obtain it either by the infusion or the aqueous decoction, the habitual use of coffee can throw no light upon its action in the animal œconomy; it belongs to physicians alone to make useful experiments on this subject.

If I may be allowed to draw any precepts from this analysis applicable to the œconomical use of coffee, I should say that it is possible to drink most excellent coffee from all the kinds of it known in commerce, provided it is not adulterated. Amateurs require three qualities in the coffee they use; they wish to find an agreeable aroma in it, a slightly bitter taste, a fine colour, and a certain density which they call body*. In order to attain all these advantages, I think that good coffee ought to be had in the following manner:

1st, Choose a dry grain, which has no mouldy or sea taste.

2d, Divide the quantity to be roasted into two equal parts.

3d, Roast the first part merely until it has the colour of

acid being combined with it and giving a blue or black colour to the vegetable, is a very remarkable phænomenon. It seemed to me to be worthy of inquiry, and I made a comparative analysis of the ashes of gall-nuts, when I also found a remarkable quantity of iron.

* Some orientalists set so much value on this density that they reduce their coffee to a very fine powder, leave the meal in the infusion, and drink it thick like soup.

dry

dry almonds or bread-raspings, and has lost an eighth of its weight.

4th, Roast the second portion until it is of a chestnut-brown, and has lost a fifth of its weight.

5th, Mix these two parts together, and grind or rather bruise them in a mortar.

6th, Neither roast nor infuse your coffee until the very day you mean to drink it.

7th, Pour upon four measures of coffee* four cups of cold water: drain this infusion apart.

8th, Pour upon the same coffee three cups of boiling water, and mix the former cold water with this infusion. We ought to get six cups of good coffee from this.

9th, Heat this coffee briskly at the moment of taking it, but do not allow it to boil.

10th, Make use of a porcelain, an earthen, or a silver vessel for this infusion.

Such is the process recommended by theory; and I have experienced that it is also the most œconomical one.

VI. *Reply to certain Remarks made by a Writer in the 5th Number of the Retrospect of Philosophical, &c. Discoveries, on a Paper in the 24th Volume of the Philosophical Magazine. To which are added, Observations on Vision, when terrestrial Objects are seen through a Mist.*

To Mr. Tilloch.

SIR, Lynn, Oct. 8, 1806.

MY paper on the Theory of the horizontal Moon, in the 24th volume of the Philosophical Magazine, p. 240, has been reviewed in the 5th number of the Retrospect in a manner which shows how ill qualified the writer is to examine the "merits or defects" of "philosophical discoveries."

"Mr. Walker commences his paper," says this writer, "on this interesting topic, by some remarks on preceding hypotheses, delivered in a flippant style, but ill comporting

* A measure is half an ounce.

with

with the dignity of candid philosophical discussion." Then, after giving an imperfect account of my theory, he commences his observations thus:

"It will be seen, that, although Mr. Walker's hypothesis is supported by the authority of mathematical demonstration, it may, notwithstanding, be inadmissible, since there is a principle assumed in the demonstration which is not completely established. Is Mr. Walker certain that the figure of the crystalline, its position in the eye, the distance from the pupil to the retina at the back of the eye, or, in other words, the general dimensions and conformation of the eye, continue unchanged, while the magnitude of the pupil varies? If he be, he has attained conviction on a point respecting which Harris, Wood, Porterfield, and others of our best writers on vision, have still many doubts. Yet, unless this be a decided point, we conceive the whole of Mr. Walker's demonstration nugatory."

It does not appear, from the above observations, that the writer knows whether my theory is true or false; all that he has been able to discover is, (and that, it seems, not without some difficulty,) *that if my data be false, "he conceives the whole of my demonstration nugatory."*

Now, sir, my demonstration depends no more upon the string of questions mentioned by this writer, nor on the doubts which, he says, our best writers on vision have respecting them, than it does upon the shining of a *Troston glow-worm* *.

For all the data that I have used in my demonstration are well-known truths. As the crystalline lens converges the rays of light to a focus, not only when it is in the eye, but after it is taken out, it is evident, that when heterogeneal rays are thus collected by the crystalline, a circle of aberration is formed in its focus, which varies in magnitude with the pupil; for the same law of aberration obtains in all convex lenses composed of dense refracting mediums. Hence the image of an object in the focus of the crystalline lens increases with the pupil; and this is a property of the

* See Monthly Magazine for August 1806, p. 103.

eye that is incontrovertible. In short, these observations on my Theory, although they come in the borrowed dress of "candid philosophical discussion," are mere subterfuges opposed to mathematical demonstration.

Although the phænomenon of the rising moon has attracted the attention of men in all ages, yet a more extraordinary optical illusion, arising from the same cause, takes place when terrestrial objects are seen through a mist. Mr. Dunn, who has given a long dissertation on the phænomenon of the horizontal moon in the Philosophical Transactions, vol. lxiv. observes, that "mountains themselves, at a distance, sometimes appear larger than at other times. Cattle, houses, trees, all objects on the summit of a hill, when seen through a fog, and at a proper distance, appear enlarged.

The following article is taken from the *Ency. Brit.* vol. xvii. p. 681.

"The writer of this article was passing the frith of Forth at Queensferry, near Edinburgh, one morning which was extremely foggy. Though the water be only two miles broad, the boat did not get within sight of the southern shore till it approached very near it. He then saw, to his great surprise, a large perpendicular rock, where he knew the shore was low and almost flat. As the boat advanced a little nearer, the rock seemed to split perpendicularly into portions, which separated at a little distance from one another. He next saw these perpendicular divisions move; and upon approaching a little nearer, found it was a number of people standing on the beach, waiting the arrival of the ferry-boat."

The following extract of a letter, which now lies before me, is given as another instance of this property of vision. This letter I received soon after my papers on the Theory of the horizontal Moon were published in the Philosophical Journal, from a gentleman on whose veracity I can place the greatest confidence:

"I beg leave (says this gentleman) to offer my testimony to that part of your first letter respecting terrestrial objects seen through a mist. When I was a young man, I was, like

like others, fond of sporting, and seldom liked to miss a day if I could any way go out. From my own house I set out on foot and pursued my diversion on a foggy day; and after I had been out some time the fog or mist increased to so great a degree, that however familiar the hedges, trees, &c. were to me, I lost myself, insomuch that I did not know whether I was going to or from home. In W——n field, where I then was, I suddenly discovered what I imagined was a well known hedge-row, interspersed with pollard trees, &c., under which I purposed to proceed homeward; but to my great surprise, upon approaching this appearance, I discovered a row of the plants known by the name of *rag*, and by the vulgar *canker-weed*, growing on a meer balk dividing ploughed fields; the whole height of both could not exceed three feet or three feet and a half. It struck me so forcibly that I shall never forget it: this, too, in a field which I knew as well as any man could know a field."

The following account respecting the effects of mist on vision, was related to me on the spot where it happened:

A shepherd upon one of the mountains in Cumberland was suddenly enveloped with a thick fog or mist, through which every object appeared so greatly increased in magnitude, that he no longer knew where he was. In this state of confusion he wandered in search of some known object, from which he might direct his future steps. Chance, at last, brought this lost shepherd within sight of what he supposed to be a very large mansion, which he did not remember ever to have seen before; but on his entering this visionary castle to inquire his way home, he found it inhabited by his own family. It was nothing more than his own cottage. But his organs of sight had so far misled his mental faculties, that some little time elapsed before he could be convinced that he saw real objects. Instances of the same kind of illusion, though not to the same degree, are not unfrequent in those mountainous regions.

From these effects of mist on vision, it is evident that the pupil, and the picture of an object within the eye, increase at the same time.

The

The author of a paper in the Supplement to the *Ency. Brit.* vol. ii. p. 641, informs us that, "according as we look at an object more or less luminous, these variations (of the pupil) are so great, that in the observable variations of the human eye the aperture is thirty times as large at one time as at another."

Whence it may be easily understood, that a cottage, seen through a thick fog, may form as large a picture in the eye as a castle when strongly illuminated by the sun's rays.

I am, sir,

Your humble servant,

EZ. WALKER.

VII. *Analytical Essay on Asparagus. By M.* ROBIQUET *junior, Apothecary at Vale de Grace*.*

SOME time ago M. Parmentier engaged the apothecaries of the Military Hospital to repeat the experiments of M. Antoine on asparagus, for the sake of giving us a subject of study fit to exercise us. My colleagues gave it specially in charge to me to follow this labour; and I undertook the task with pleasure, conceiving, at first, that it would be an easy one. But as my first essays created some doubts in my mind respecting the truth of the facts which had been announced, and as there are besides many vegetable substances of which the distinguishing properties are but little known, I was necessarily involved in very considerable embarrassment. I strove, however, with redoubled efforts against these obstacles, till I collected the facts which I have the honour to present to you.

As in every analysis we should endeavour to isolate, as conveniently as possible, each of the substances which compose a whole; in place of following the process pointed out by M. Antoine, which prescribes making a strong decoction of asparagus, then to evaporate it to obtain the extract, &c., I thought it preferable to employ at first a mechanical analysis, that I might not be exposed to change certain sub-

* From *Annales de Chimie,* tom. lv. p. 152.

stances susceptible of being altered by heat: thus, after properly pounding some asparagus, I extracted the juice in the ordinary manner, and passed it through fine linen with the intention of entirely separating the gross feculence.

The juice in this state had an odour sufficiently strong, and as it were sourish; it was of a dirty yellow, inclining a little to green. I filtered it to separate the feculence: this being well washed retained the smell of asparagus, but more marked and more disagreeable; treated with boiling alcohol it was not entirely dissolved, and the residue, which had a grayish colour, presented itself under the form of light flakes, which, being collected with care, had the following characters: they were soft and somewhat unctuous to the touch; they acquired a horny consistency and much hardness, and turned almost black by desiccation. During combustion they gave out a smell like that of burnt horn; they diffused at the same time an oily smoke, and left a pretty compact coal. When submitted to distillation, they gave at first a yellowish phlegm of a milky consistence, afterwards carbonate of ammonia very abundantly, and towards the conclusion of the experiment a dark red oil in great quantity.

When left for some days in contact with cold water, the flakes we have mentioned were decomposed, grew mouldy, and contracted a fetid odour. When boiled with distilled water they were not sensibly altered.

The alkalis dissolve them entirely. Vinegar has also a marked action on this matter, which is insoluble in alcohol, especially if heat be employed. The solution in the acetous acid yields a deposit: by saturation with an alkali a great quantity of flakes are obtained similar to the former, if we except the colour, which becomes less intense. I am far from venturing to pronounce on the nature of this substance; and the few experiments I have been able to make from the small quantity of the matter I obtained, have only authorized me to consider it as very much animalized, and to believe that probably it contributes very much to the disagreeable odour which asparagus communicates to the urine of those who make use of it.

The alcohol which had been boiled upon the green feculent

lent matter was filtered while yet warm, and deposited, while cooling, some light jelly-like flakes, which in drying united in small yellowish masses. They became soft with heat, and diffused, when burning, an agreeable odour. They appeared to be true vegetable wax.

The alcoholic tincture, which became slightly turbid by the addition of water, left as the residue of its evaporation a green tenacious substance, of which the taste was at first a little sharp, slightly bitter, and very tart; it had a smell also peculiarly insipid, and even nauseous; properties which may create a presumption that if it is a resin it is not pure, and that it is probably united to a portion of volatile oil.

I think that those substances, although they are not of great importance, deserve to be mentioned in the análysis of asparagus. I shall pass, however, to the examination of the filtered juice: it had a colour nearly resembling that of whey, though inclining a little more to yellow, and reddening very sensibly the tincture of turnsole. I exposed it to the action of fire, to see if it contained a small portion of albumen. Before ebullition it showed some flakes, of which the number augmented considerably in proportion as the heat acquired intensity. The liquor, after having been boiled a certain time, was filtered, to separate from it the coagulated albumen.

My principal object being to obtain malic acid and gelatine, I evaporated (as M. Antoine had done) the liquor to the consistence of an extract, which I treated with alcohol at 39°; and after having washed the extract, obtained by several repetitions of the experiment, always with alcohol, I dissolved in water the portion that was insoluble in spirit of wine; but I did not obtain, as M. Antoine did, a residue which that gentleman designates by the name of an oxygenated extract. From the process he followed it was possibly owing to a little of the albumen remaining in the solution, which separated in the progress of evaporation. But this is only a conjecture.

I tried the alcoholic liquor, and was much astonished to obtain, by the acetate of lead, only a very slight flaky precipitate, which, according to M. Antoine, ought to be abundant, since he assures us that this alcohol contains malates of potash and of lime in manifest quantities. I observed, on the contrary, that the aqueous solution precipitated a larger quantity by the same reagent; and what surprised me much more was, that neither the one nor the other of these precipitates was resoluble in vinegar; from whence I was able to conclude that it was not malate of lead, or at least that it was not pure. It is true, however, that the animal matter might very much alter the result. I again consulted the memoir of M. Antoine, to know if he had made mention of this experiment; and I saw he had, but only to support his opinion, that malic acid exists in asparagus. I believe he has been deceived on this point; for we know that malate of lead dissolves easily in vinegar, and that it is even a test employed to detect the presence of sulphate of lead in vinegar.

Suspecting that there existed in the juice of asparagus a little earth, I treated a portion of the alcoholic liquor, properly evaporated, with the sulphuric acid diluted with three parts of water. I proceeded to distillation, and there passed into the receiver an acid liquor, which did not precipitate the metallic solutions nor those of barytes, &c.; in a word, easy to be recognised as vinegar.

The ashes of this part of the extract, which was soluble in alcohol, were composed, in a great measure, of potash and a little lime; whence it seems natural to conclude that the acetic acid was combined with the potash, and that the supposed malic acid was combined with the lime; for I ought here to mention that this calcareous salt, whatever it may be, has the property of dissolving in acetous acid, and that the alcohol always preserved a portion of it free.

The acid contained in the juice of asparagus appearing to have some properties different from those assigned to known acids, I directed my attention chiefly to this point, and endeavoured to procure it in a state of purity. For this purpose I took some juice of asparagus filtered and coagulated by heat; I poured upon it the acetate of lead until it ceased to form any more precipitate: this, being collected and well washed, was treated with a third of its weight of concen-

trated

trated sulphuric acid diluted with four parts of water. Some moments after mixture the mass became more liquid, and took a beautiful rose colour. I heated it slightly to facilitate the decomposition; and after some hours digestion the liquor was filtered: it still contained a little sulphuric acid, which I separated by means of caustic barytes. I again filtered it, and after having evaporated it to the consistence of thick syrup, I treated it with alcohol, fearing that this acid had carried along with it in solution a little animal matter (mucilage, or of some other substance) extraneous to its nature, particularly a calcareous salt which was deposited in small, fine, and white needles. I presume that this acid is one of its constituent principles. I afterwards evaporated the alcohol without making it boil, managing the fire very carefully towards the close of the experiment. I repeated this operation until, by again dissolving it in spirit of wine, it did not form any precipitate at all: a precaution necessary to have this acid in the purest possible state. We might suspect that the acetate of lead had precipitated at the same time with the acid a great quantity of the matter which M. Antoine regards as of an animal nature, and that consequently the sulphuric acid had set free a good deal of the acetous acid which was mixed with that of the asparagus; but it is to be remarked that this pretended gluten is not precipitated, at least for the greater part, until the juice has acquired a certain degree of concentration. The experiment I am about to relate will demonstrate this very distinctly: I poured into the juice so great a quantity of the acetate of lead as must have separated all the acid. I afterwards treated the liquor with sulphuretted hydrogen. It formed an abundant black precipitate of sulphuret of lead, which I separated by the filter; I evaporated it, and after some time I again obtained a precipitate by the same reagent.

The principal properties which I have recognised in the acid I have mentioned are: a colour more or less brown according to the degree of concentration, and very acid, leaving in the mouth a peculiar taste very disagreeable. It also gives out during its heating a very pungent odour, which afterwards becomes blended with another extremely fetid, and which has much resemblance to that of burnt onions. M. Antoine, however, compares it to the odour of caramel, The alkalis form with it soluble salts, and that of soda, among others, of a fresh savour, slightly bitter, and which leaves in the mouth a taste like that of green nuts.

Barytes, strontian, or lime, united to this acid give very insoluble salts, susceptible of solution in an excess of acid, but difficultly: all of them present themselves under the form of flakes more or less light. It possesses the remarkable property of decomposing, in general, all the earthy acetates without having recourse to double affinities: it also decomposes several metallic solutions, and in a remarkable manner those of iron, which it precipitates white though at the maximum; those of copper it precipitates of a blueish green. The acetate of lead is also precipitated in white flakes, which are insoluble in vinegar.

If in place of employing this free acid we take one of its alkaline combinations, precipitates are obtained, in the solutions, of aluminous and magnesian salts; but it is necessary that the latter be very much concentrated, without which the precipitates are redissolved immediately.

[To be continued.]

VIII. *Description of an improved Air-Pump.* By T. Sylvester, *Esq.*

To Mr. Tilloch.

SIR,

One of the principal causes of the imperfections in the air-pump arises from the difficulty in opening the valves at the bottom of the barrels, which imperfection was thought to have been remedied by the introduction of stop-cocks; but experience shows that, however accurate they may be when new, after a little use they became faulty.

I here send you a description of an air-pump, which I lately executed, without either valves or stop-cock. I have supplied their place by a slide, which is shifted at every motion of the piston, in a similar manner to that of a stop-cock, which requires no pressure of the air to open it. It moves between two facings of leather, which lessens friction

tion and procures a close fitting of the parts; and if after use it should at any time be found not air-tight, the defect is instantly remedied by means of four screws; which is a considerable advantage over stop-cocks. The manner of its application is shown in fig. 1. (Plate II.); the perspective is considerably strained for the purpose of rendering the construction more intelligible. AB is the barrel, having a strong plate of brass, CDEF, firmly fixed to the bottom with two small holes, G and H, that enter the inside; but near the edge of the barrel IKLM is another plate of brass similar to the former, having two holes, N and O, that correspond with G and H. In the hole N is closely screwed the duct P, which leads to the receiver of the pump; and in the hole O is screwed the duct Q, through which the air is discharged. The plates, being ground smooth, are faced with leather, pierced opposite the holes, and smeared with grease; then the polished brass slide RS, fig. 2, is placed between them. In the slide is a hole T, which is put over the hole N. The plates are then pressed together by the four screws V, U, W, X, which screw into nuts that are fixed into the bottom part of the machine; they also serve to direct the motion of the slide, which is drawn backwards and forwards. When the hole T is between N and G, the piston Y is drawn up, which causes the air to rush out of the receiver through the duct P into the barrel; the slide is then drawn by the ring Z, for the hole T to be between O and H. The communication between the barrel and discharging duct Q is opened; the piston is then forced down, and the air is discharged. The slide is again shifted; which closes up the discharging aperture and opens the former, and the process is continued until the receiver is exhausted. At each end of the slide is a small ridge of brass that prevents its being pushed or pulled too far, so that the hole in the slide will exactly correspond with the holes in the plates.

If the pump has two barrels, by making the slide a little longer, and with two holes, it will serve both by only once moving; and, from what has been said, it is evident that, merely by changing the motion of the slide, the machine will condense as well as exhaust. When the receiver is ex-

 hausted,

hausted, the air is readmitted by a stop-cock under the plate of the receiver.

If you think the above improvement worth inserting in your Philosophical Magazine, by so doing you will greatly oblige

Your very humble servant,

Worcester,
Sept. 12, 1806.

T. SYLVESTER.

IX. *A Memoir on the best Method of measuring Time at Sea, which obtained the double Prize adjudged by the Royal Academy of Sciences; containing the Description of the Longitude Watch presented to His Majesty* the 5th of August* 1766. *By M.* LE ROY, *Clock-maker to the King*†. *Translated from the French by Mr.* T. S. EVANS, *F.L.S., of the Royal Military Academy, Woolwich.*

A GREAT deal has lately been said on the subject of chronometers, more especially with regard to what is contained in the description given by M. le Roy of his time-keeper; and the work to which that description is subjoined being now in the hands of very few persons, the translator thought this paper in English might be a valuable addition to the very little useful matter which we possess on that branch of mechanics. It has been asserted that the greater part of the improvements in chronometers, lately laid before the Board of Longitude, are mentioned in this account of Le Roy's; the public will therefore now have it in their power to judge for themselves. T. S. E.

Labor omnia vincit
Improbus. *Virg. Georg.* lib. i.

INTRODUCTION.

BY proposing *to determine the best method of measuring time at sea*, the Academy appears to require, first, such a measure of time as may give to mariners the knowledge of the longitude which has been so much desired, that for many ages

* Louis XV. king of France.

† This Memoir is subjoined to the *Voyage fait par ordre du Roi en* 1768, *pour éprouver les montres marines inventées par M. Le Roy, par M. Cassini, fils.* Paris 1770. 4to.

ages it has been the principal object of their researches, as well as of astronomers and philosophers of the greatest celebrity. In the second place, by the expression "*to determine,*" the academy appears to require also palpable proofs that the method proposed is the best possible.

To comply with these requests I shall divide this memoir into four parts.

In the first I shall go through the different methods hitherto attempted, or which may be hereafter tried, for measuring time at sea; I shall make known the insufficiency of most of these methods; and I shall show that, notwithstanding the irregularities of our portable watches, we are thoroughly persuaded that the best means of obtaining the desired measure of time consists in a kind of perfected watch.

In the second part I will endeavour to point out all these irregularities, and to discover the different causes, physical or mechanical, whence they are derived, in order to be more in a state to correct them afterwards.

The third shall contain the description of a *chronometer*, or kind of large watch, deposited with this memoir. I shall show that by its construction it is exempt from the defects remarked in common watches, and I shall enter into the detail of the expedients which have been used in the workmanship to prevent them.

Lastly, I shall terminate this memoir by a recapitulation or suite of observations, forming so many parallels to the methods which I have employed, with those that may be made use of with the same view. I hope to prove, by experiments and palpable reasonings, that mine must obtain the preference. If I succeed, I shall be well paid for twenty years consumed in these researches; since, besides the honour of being crowned by the Academy of Sciences, this discovery concerns the good of humanity, and even the preservation of a number of lives. If, on the contrary, my labour is without success, there will remain at least the satisfaction of having spared no pains or expense in endeavouring to fulfil the honourable task imposed upon me as a man, a patriot, and an artist.

PART

Part I.

Examination of different methods which may be tried to measure time at sea.

Moving bodies being evidently the only measures of duration or of time*, that nothing may be omitted in so important a subject, let us first cast our eyes on those whose motions may appear capable of giving an exact measure of time.

The first which present themselves are the stars. The perfection to which telescopes have been carried, the successful labours of many celebrated astronomers in the theory of Jupiter's satellites, and the tables which they have given of their revolutions †, give us reason to believe that they will presently become of very great help for measuring time at sea: the same must be said of the theory of the moon.

But when we shall have given to these tables and these telescopes all the perfection that can be desired, we shall find that they are yet insufficient. We cannot always see the moon, still less the satellites of Jupiter: supposing, even, that we could observe them as often as circumstances require, these observations are in some degree useless, without an instrument that would preserve the hour with exactness after we have determined it by the sun ‡.

The observations of the heavenly bodies cannot, therefore, entirely fulfil our wishes: let us therefore look among the bodies which are more at hand, if there be not some one which by motions arising from different causes would be proper to give us the required measure of time.

Those which offer themselves first to our examination are fluids, and solids reduced into insensibly small parts, forming *clepsydras*, or *sand-glasses*; bodies falling, or making oscillations by their gravity combined with their inertia; the vibrations of magnetic bodies; and those which solids make by the help of an elastic force, &c. By reflecting,

* Le Monnier, *Institutions Astronomiques*, p. 157.

† See the Essay on the Theory of Jupiter's Satellites, by M. Bailly; and M. Jeaurat's Tables.

‡ This remark is M. Daniel Bernoulli's, p. 21, of his *Recherches Méchan. et Astron. sur la meilleure Maniere de trouver l'Heure en Mer*, &c.

we

we shall presently find, that of all bodies in motion, there are only the latter which can, with any exactness, measure time at sea.

It appears, first, that all bodies, whether fluid or solid, moving by the effect of their gravity, are by that alone inadmissible in the present case. Besides that, their motion will be always more or less accelerated or retarded by the shocks that they will receive from the ship; we know also that their gravity is variable under different parallels: it is therefore not probable that we can ever correct this source of inequalities.

I know of no other person, except Sully*, who in his marine or *lever* clock has pretended to have obviated them on this principle, that by adding, proportionally, weight to the balance and to the lever, the rate of the clock would not be changed; but the academy, in approving the efforts of this artist, declares in its report that *it does not adopt all his reasonings*. Nothing can be more deceitful in effect than that on which he founded this pretended property of his clock.

To convince ourselves, let us remark, that the vibrations of his regulator, like those of a pendulum, are produced by the force of inertia combined with that of gravity; that the first cause operates principally on the balance, whose gravity has no influence on the time of vibration; that the second resides in the lever, whose inertia has very little effect, because it descends almost vertically; and that, lastly, the time employed in each vibration depends on the proportion which exists between the balance and the lever, that is to say, the same as in the pendulum, the ratio of inertia to that of gravity.

The experiment which Sully † made before the academy proves nothing. When by adding matter to his balance he augments its gravity, he makes also the force of inertia to increase in the same proportion; but under the pole, its

* See the machines approved by the Academy of Sciences, and the author's *Abridged Description*.

† *Abridged Description*, p. 7. In this experiment Sully attempts to prove that the unequal gravity of bodies in different latitudes produces no change in the going of his machine.—T. S. E.

gravity

gravity augmenting without its quantity of matter changing, its inertia would not experience any increase.

In heavy bodies in motion, to compensate the effect of their different gravity in various climates, (an effect which may go so far as to retard a second pendulum two minutes in twenty-four hours, when removed from the parallel of Paris to the equator,) it would be necessary to find an expedient by means of which their inertia is proportioned always to the increase or alteration of their gravity; but the force of inertia of bodies, being a first and unalterable cause, this does not appear possible.

In vain would they pretend to estimate the differences we have just spoken of, in keeping a register of the parallels under which they would navigate, and of the time which they would remain there. Besides all the difficulties of this method, and the very complicated calculations which it would require, it cannot be exact without a perfect homogeneity in the different parts of the earth, which homogeneity appears to be contradicted by the observations of the different lengths of the second pendulum made in different climates: it supposes, moreover, that the sailor can know, several times each day, at what height he is; which is a very strained supposition.

A long detail on this subject would be useless. We know sufficiently the defects of clepsydras and sand-glasses: we are not ignorant of the inconveniences of the *circular*, *triangular* pendulums, &c. proposed by M. Huygens; or of those coupled together by wheels acting in each other, like those tried by the late M. Dutertre *. Experience has sufficiently shown the defects of these methods and of many others, which for this reason I shall pass by in silence.

I come now to bodies which make vibrations by their inertia, combined with that universal force which directs the needle of a compass.

The celebrated Dr. Hook † thought that we might apply it advantageously to a clock in the quality of a regulator. But Graham, having observed at different times, and during

* See Thiout, *Traité de l'Horlogerie*, pl. 39. fig. 5.—T. S. E.

† Philosophical Transactions.

the

the same interval of time, the number of vibrations of the needle of a compass made and suspended on a pivot with the greatest care, has found that it was not always constant.

I have observed also, by means of an instrument, which I call a *magnimeter*, which by a long index marks on a limb the variations of magnetism, first, that this force in a body changes according as it is well or ill placed in the direction of the magnetic meridian, according as it is more or less elevated in the atmosphere, and according to the different degrees of heat and cold. I have besides observed that thunder produces sensible variations in these forces, and that in the aurora borealis there happen also considerable changes, as they have observed in Sweden. I only mention these experiments, the detail of which would draw me too much aside from my subject: I hope some day to give an account of them to the Academy.

There now remain only the bodies in vibration by the help of the elastic force. Every thing induces us to presume that these are the most proper to procure the required measure of time. The regularity of certain watches which are executed daily, but, above all, the trials that have been made with the *time-keepers* of the celebrated Mr. Harrison, the recompense that he merits for them, confirm, in some degree, what was before only a presumption, and appear to demonstrate that the true, and perhaps the only means of measuring time exactly at sea, consists, as we have before observed, in the perfected watch. But as watches in general are very distant from the precision requisite in a marine watch, we should first search out their different irregularities, and the causes, whether physical or mechanical, whence they are derived; according to the example of a wise physician, who, before he has recourse to a remedy, endeavours to understand the disorder well, and what may occasion it. This is what I shall do in the following part: in order to render the different objects which are treated of more evident, I shall make so many separate articles of them.

As mechanics is here continually intermixed with natural philosophy, whatever is only supported by reasoning, however solid it appears, will always be very uncertain. I have but too frequently found it so. Therefore I shall advance nothing of which I am not assured by facts, as the Commissioners may verify.

Part II.

Examination of the causes which make watches vary.

Article I.

Of the spring in general, and of the alterations which may happen in the force of the spiral spring.

The first question which presents itself to be cleared up in treating of the theory of watches, and which, however, appears to be made for the first time, is this: *Is the spring in itself, abstracting from the effects of heat, a constant power, on which we may establish a principle of perfect regularity, or is it not?*

These axioms of the philosophers, that *there is no perfect spring in nature*; that *it does not admit of any precision*, &c., would appear at first to announce in the spring a power not very proper to give the required accuracy in a marine clock. But, on the other hand, many philosophers and artists think that the spring is a constant power, when it is but little contracted.

To have more exact notions on this subject, I have constructed an instrument, fig. 1. Plate I.; I call it the *elaterometer*. This is in some degree only a long spring *rr*, stretched by a weight *p*, which, according as the force of this spring increases or diminishes, ascends or descends, the distance that it moves being rendered a hundred times more sensible by means of a long index *ll*, whose weight on the spring is insensible; this weight being counterbalanced by an opposite branch *bl*, which makes it almost in equilibrium.

By means of this instrument we find that the spring loses a considerable part of its force in the first month of its tension; that then the loss is much less; that at last it becomes almost insensible, unless the spring receives a considerable degree of heat; for then the index falls several degrees; and when the thermometer returns to the degree where

where it was before, this index does not ascend to the same point, but rests below.

These experiments appear to show that the vibrations of the spiral spring can measure time but imperfectly: but here follow several considerations that must convince us of the contrary. 1st, In the vibrations of this spring, its contraction and opening are only momentaneous: 2dly, By supposing that in its contraction, for example, it had been bent a little, it would return presently to its proper opening. But even when it experiences some loss on one side, this cannot be done without its gaining it on the other, as daily experience proves. There would result, therefore, from this a compensation. All the inconvenience that would follow is, that the watch will not be so perfectly adjusted in its escapement. Lastly, the experience of watches with dead escapements * confirms again what I have advanced. The greater part, after having gone for several years, are still found to be regulated when they have been cleaned, if there has been no considerable wear in the parts of their escapement.

These observations show, nevertheless, that we cannot take too much pains for the spiral spring to be fastened in a natural and unconstrained situation † (as recommended by Daniel Bernoulli). This is what does not take place in most watches, and it is (as we shall see in the end) that which I have particularly endeavoured to execute.

We must conclude also from what precedes, that nothing would be more disadvantageous in watches than two spiral springs in contraction, as John Bernoulli has proposed ‡: for then the effects observed by our *elaterometer* would absolutely take place.

Besides, a very simple reasoning suffices to convince us that there is not in nature any spring which is not in the case of the constrained equilibrium (*l'equilibre forcé*) recommended by M. Bernoulli ‡.

* *Echappements à repos.* See Berthoud, *Traité des Hor. Marines,* p. 316. § 969; and *Essai sur l'Hor.*, tom. ii. 1642.—T. S. E.

† *Recherches Méchaniques et Astronomiques,* p. 45.

‡ *Recherches Physiques sur la Propagation de la Lumière:* we may consult also on this subject D'Alembert's *Opuscules Mathématiques,* tom. v. p. 503.

In the piece which obtained the prize of the Royal Academy of Sciences for

It is not the mass of the spring which produces its strength; without changing this mass, we may augment or diminish considerably its elastic power by giving it a higher or lower temper, or by other operations. To know, therefore, whether a spring, when unwound, is in the case of a constrained equilibrium, we must not, as M. Bernoulli did, consider the whole of the mass, which by itself is incapable of any motion, but examine what passes in the interior of its pores, where the agent works, whatever it may be, that produces the elastic force; the same as in a foot-ball, the covering is not the first cause of the spring, but the compressed air in its interior. Now whatever may be the cause of the spring, it is easy to see that all the particles which compose it are themselves so many springs already wound, being continually in action to develop themselves. We know, in like manner, that all these particles are in a centre of constrained equilibrium, since the active principle which they contain, and which tends to dilate them, is necessarily counterbalanced by the force which compresses the parts of the metal one against the other; by that same force, without which all in nature would be disunited.

The constrained equilibrium is made manifest in effect very sensibly, when by a small excess of cold many springs break of themselves, and when we see steel become faulty when it is tempered.

for 1736, M. Bernoulli showed several things analogous to the spiral springs that are applied to watches. He recommends to attach two of them to the centre of the balance, whose spires turn in a contrary direction, to have what he calls *a centre of constrained equilibrium*: he pretends to remedy by this means the fluttering, and to render the vibrations more equal; instead of which, with one spring, he says an *idle centre of equilibrium* can only be had, which has not sufficient action on the balance to prevent fluttering, and to maintain the vibrations equal. What he understands by a *constrained centre of equilibrium* is the two springs, which, being wound, become antagonists to each other; that is to say, the first spring which is placed will draw the balance all on one side, and absolutely out of its escapement; but the second spiral, drawing in a contrary direction, will bring the balance so that the pallets are parallel to the balance wheel, and place it, as usual, in its escapement.

What he understands by the *idle centre of equilibrium*, is a balance with its common spring fixed. This spring is neither wound on one side nor the other, consequently it remains in inaction, which is what he means by *an idle centre of equilibrium*.—T. S. E.

Besides,

Besides, when we bend an elastic plate much, it begins to break in its convex part: does not this show that the pores of this part widen, whilst those of the cavity close? We therefore cannot bend a spring without part of the fluid which causes the elasticity being lost on one side of its strength by a small compression, whilst that of the opposite side gains by it. This evidently procures the effect of M. Bernoulli's two springs. The *idle equilibrium*, which this geometrician conceived, is therefore a chimerical being.

This error, I may remark by the way, comes from the same source which occasioned that of the *vis viva* (*forces vives* *). Those who have admitted them have not sufficiently discriminated the *apparent* from the *real* obstacles which a body in motion has to surmount. In the spring, for example, the first are always as the square of the velocity of the body that surmounts them, but the second are only as the simple velocity.

We may conclude that the vibrating force of a spring is a constant force, the effect of heat excepted, provided it is in a free and unconstrained state; that, every spring being in the case of the *constrained equilibrium*, what M. John Bernoulli says of this equilibrium is absolutely as applicable to one only as to two opposite springs, such as he requires. We may consequently infer from these principles, that the vibrations of the spiral spring are exactly isochronous; that being therefore applied to the watch by a good dead escapement, abstracting from friction, it would compensate the inequalities of the mover and of the wheel-work: this is what I shall examine in the following article.

Article II.

Second source of inequalities in watches: the non-isochronism of the vibrations of their regulator, arising as well from the spiral spring itself, as from the nature of the escapement.

Nothing is more important for the theory of watches in general, and to direct the artist who executes them, than to

* See Saverien, *Dict. de Mathématiques*, art. *Forces*; and vol. iii. p. 35, of John Bernoulli's Works. Geneva, 1742.—T. S. E.

know what is to be depended on with regard to the degree of isochronism of the vibrations of the spiral spring connected with the balance*: nevertheless nothing, perhaps, in philosophy is more obscure.

The following experiment, from which it has been attempted to deduce this isochronism, cannot, in my opinion, form a complete proof:

"In sonorous bodies, that are struck or played upon with more or less force, the tone remains, they say, always the same; nevertheless they would heighten or lower sensibly if there happens the least change in the duration of their vibrations; the different extent of these vibrations has therefore no influence on the time in which they are made. Now," continue they, "a balance joined to a spiral spring is analogous to the wire of a harpsichord; when either of them vibrates, it is always a mass moved freely by an elastic force:" therefore they conclude, "the balance, assisted by the spring, makes its reciprocations more or less wide, in times that are perfectly equal."

This reasoning proves, moreover, that all the vibrations of a springing body are nearly isochronous, the ear not being sufficiently delicate to perceive the small differences in the tones. Besides, *M. de Mondonville has found that the tone of a chord rises more or less according to the degree of force which presses it, and that this goes as far as a semitone, when it is done very softly, although the gradation observed in swelling and softening the sound commonly renders this difference insensible to the ear* †.

Something more precise is therefore necessary to know exactly whether (allowing for friction, for the resistance of the air, &c., circumstances to which we shall attend further on) the vibrations of the spiral spring connected with the balance

* John Hautefeuille, an ingenious mechanic, born at Orleans in 1674, appears to have been the first who applied a small steel spring to regulate the vibrations of the balance. It was laid before the members of the Academy of Sciences in 1694; and when Huygens applied for his patent for this discovery, it was opposed, because Hautefeuille had made use of it before him. See his Life in Dr. Hutton's Dictionary.—T. S. E.

† M. Ferrein's Dissertation on the Formation of the Voice, in the Memoires of the Academy of Sciences for 1741.

balance are isochronous, or whether they differ in time according as they are more or less extended.

We know, by the theory of forces, that the different excursions of a moveable body are isochronous, when those which push them are in the ratio of the distance of the term to which they make it bend. The true method of clearing up the present question appears therefore to be, to examine, by experiment, whether the force of spiral springs augments according to the proportion of the spaces described in their different contractions or their different openings.

To know what we are to think on this capital point, I took the main spring of a common watch and attached its interior extremity to an arbor, sustained by very fine pivots, which carried a large pulley: I then fixed the exterior end of this spring to a fixed point, so that it might rest in its natural state. This done, I fixed a wire to the pulley and wound it round; then I fixed to the other end of this wire a small hook, on which I placed successively different weights, these weights bending the spring in opening and shutting it, more than if it had caused a balance to vibrate: I observed the ratio in which the hook descended, and I found it always as the weight with which it was charged*. If, for example, half an ounce made it descend a certain quantity, an ounce made it descend a double quantity, and so on.

Indeed it was not the same when the arbor had made several turns; the spaces described then, no longer augmented in proportion to the weights: this difference, very sensible on the side where the spring shut, became almost nothing on the side where it opened: this is why I attribute it, in a great measure, to the change of the lever by which it acted.

However it may be, as the ratio of the weights takes place in our experiment for arcs much greater than those which the balances of watches describe, it appears that we should be in the right to conclude that its vibrations are exactly isochronous; that, consequently, the inequalities of the motive force, those which arise from losses of freedom in the

* Dr. Hook discovered this many years before, and made it the subject of an anagram, which Dr. Wallis found to be *Ut tensio sic vis.*—T. S. E.

 wheel-

wheel-work, &c., become nearly compensated in watches having a dead escapement: but this is what does not take place. In all the experiments that I have made on the duration of the vibrations of their regulators, making oscillations either by the action of the wheel-work, or freely and independently of this action, I have almost always found, as well as the most celebrated artists and men of science *, that the long vibrations were always slower than the short: I have even remarked that in a double arc the difference was most frequently $\frac{1}{120}$. This effect arises, I believe, from the mass of the spring, or perhaps from the obstacles which it experiences internally † to display its strength.

It is only lately that I have at last found, as I shall explain more particularly, what is very important, and which besides must serve as a base to the theory of watches, and a guide to workmen who construct them; viz. *that there is in every spring of sufficient extent a certain length where all the vibrations, long or short, are isochronous*; *that this length being found, if you shorten the spring, the long vibrations are quicker than the short ones*; *if, on the contrary, you lengthen it, the small arcs are described in less time than the large ones* ‡. It is on this important property of the spring, hitherto unknown, that the regularity of my marine watch particularly depends, as we shall see in the end. From what precedes we are sensible that the accuracy of watches depends, in a great measure, on the length given to the re-

* This is shown in the writings hitherto given on clock-work. See M. Sully's *Dissertation on his Marine Watch*, (4to. 1726, Paris.) *Les Etrennes chronometriques* of M. Le Roy, p. 69, &c. The attempts made by several artists to correct this retardation prove it also; witness the *compensation curb* of M. Gourdain, adapted to the spiral spring. *The Report made to the Board of Longitude who examined Mr. Harrison's time-keeper*, shows that the English artists are of the same opinion. See the *Gazette du Commerce* for the year 1765, Tuesday, October 8; and the Report, signed *Ludlam*, sent to the Academy. *The principles*, says this Report, *on which Mr. Harrison forms the third change is, that the long vibrations of a balance, whose motion is caused by the same spring, are made in less time. This principle is contrary to all the opinions received among men of science, workmen, &c.*

† See vol. iii. p. 97, of John Bernoulli's Works.—T. S. E.

‡ The way in which he made this important discovery is related in the beginning of Article VIII. Part III.—T. S. E.

gulating or spiral spring. If with the same kind of dead escapement certain watches go badly, whilst others are very regular, we here see the cause of it; and moreover the new observation may be of great help in the disposition of pendulums, whether small or second, where the pendulum is suspended by a spring: in effect, we know by what precedes that it must have such a length in the spring of suspension, that all the vibrations of these pendulums may be isochronous.

Though we should suppose, even in the regulating spring of a watch, the length requisite to render all the vibrations of the balance isochronous, if it was applied there by the common methods, this isochronism would be presently destroyed by the friction of the pivots of the balance, which, according to the remark made by Daniel Bernoulli *, would become always less considerable in great arcs than in small; for, by theory, the obstacles of friction, the tenacity of oil, &c., as those of gravity, of springs, of cohesion, &c. are proportional to the times during which they are surmounted. Now, the vibrations independent of these frictions being supposed isochronous, these frictions must become much less considerable with regard to the force which surmounts them, when, for example, these arcs are doubled; since this force, being as the arcs, is then doubled, and the time not sensibly different.

M. Sully has made experiments on this subject as decisive as they are curious, which may be consulted †.

The inverse of what precedes takes place for the friction occasioned by the balance wheel on the parts of the escapement, viz. the cylinder in those of Graham, and on the plates or planes in those of Debaufre, Sully, Le Roy, Gourdain, &c. These frictions, instead of rendering the long vibrations quicker than the short ones, on the contrary augment the duration of the former: the following is the cause: —The balance being supposed to have the necessary free-

* *Recherches Méchaniques et Astronomiques*, &c. p. 41.

† *Suite de la Description d'un Horloge*, &c. p. 168: *Dissert. sur une Montre Marine*, &c.

dom, it is impossible to augment the arc without augmenting the force which supports the vibrations, and consequently the pressure of the balance wheel on the cylinders or planes, &c.; and as, by theory, a quadruple force is necessary (abstraction being made from a great number of causes, both physical and mechanical, which concur here to destroy a part of the motion of the balance); as it is necessary, I say, to have a quadruple force to impress on the balance as well as on the balance wheel a double velocity, it follows, that the pressure on the cylinders, the planes, &c. (always in proportion to the motive force) augments here in a much greater ratio than the force of the regulator to overcome them, which is only as the velocity. We know, also, that when the wheel-work is impeded, whether by friction, the coagulation of the oil, &c., there must arise considerable variations in watches with a dead escapement, for then the force communicated to the balance is necessarily much less. But the friction on the cylinders or planes is not diminished by this, because the pressure of the wheel on these cylinders or planes is a dead force, and the resistances and friction of the wheel-work have no sensible effect, except when the moveable parts are in motion.

We can hardly determine any thing respecting those causes which affect the isochronism of the vibrations in watches with a dead escapement, and which, without any thing regular, make them advance or retard. All that we can say is, that they augment or diminish according to the quantity of friction on the cylindric portions, according to the form of the balance, the size of its pivots, the quality of the oil, the length of the spiral spring, the number of vibrations in a given time, the length of the arc, the points of that arc where the wheel ceases to act on the balance, the number of teeth in the balance wheel, its mass, the quantity of motive force, &c.

There are, without doubt, a number and a magnitude of vibrations where the effect of these different causes is least sensible; but what precedes has already proved to us, that the best way, without any comparison, will be always (as

I shall

I shall hereafter demonstrate more positively) to give the vibrations of the balance the greatest freedom possible. This is what I have practised in my machines.

Article III.

Third cause of variations in watches—the manner in which the balance is sustained, and the different situations in which they are placed.

It is clear that the weight of the balance occasions on the pivots that sustain it, a friction that is both variable and prejudicial; but much less considerable, when, the watch being laid flat, it is carried on the extremity of one of its pivots, than when, hanging, the weight of the balance is borne on the circumference of the two pivots: by what precedes, there follow causes of variations, greater or less, according to the size of the pivots, their polish, the polish of the holes, their depth, the oil which is applied, the weight and size of the balance, the number of its vibrations, their magnitude, the length of the spiral spring, its form, &c. Experience shows, in effect, that most watches, especially those which have dead escapements, lose when hung up. To correct this irregularity, we render the balance more weighty in that part of its circumference which is underneath when the watch is hung up: but by this expedient we palliate the evil rather than destroy it, and we render the watch more subject to vary by shocks and different motions, the effect of which to be done away requires (as we shall presently see) that the balance be throughout of equal weight.

Article IV.

Fourth inconvenience of watches,—they lose in heat, and gain in cold.

This effect arises in watches with a dead escapement, 1st, From the different causes which render the vibrations of the regulator greater by heat: 2dly, Because the dimensions of the balance, and its spring, are augmented by it: 3dly, From its diminishing the elasticity of the latter. From these united effects there results a variation, greater or less, according to the nature of the escapement, the length and force of the spiral, the greater or less freedom of the balançe, &c. In general, watches with dead escapements advance about five minutes in twenty-four hours, when, from the heat of the fob, which is nearly equal to that felt under the tropic, it passes to that degree of cold which produces ice.

If we wish to know how much influence the balance and the spiral spring have separately on these errors, the calculation is easily made.

Experience shows that a steel bar of three feet increases 1-60th of an inch nearly, or $\frac{1}{2160}$th part, when from freezing cold it passes to a heat which raises Reaumur's thermometer* to 30°, about equal to the heat of the fob of a middle-aged man.

Now the weight of a balance being known, the resistance which it gives to the spiral is in the direct ratio of the square of the distance of its *circumference of percussion*, if we may so express it, from the centre of its motion; and by theory, the number of vibrations is in the inverse proportion of this distance: therefore a watch taken from the fob to a place where it freezes, when it is arrived at the cold of the place, each of its vibrations, by the contraction of the balance alone, is accelerated the $\frac{1}{2160}$th; that is to say, the watch by this cause advances about $1\frac{1}{3}''$ per hour; the remainder of the gain being produced by the increase of elasticity in the spiral spring, and other causes.

* The degrees of Reaumur's thermometer may be converted to Fahrenheit's by the following equation: $\frac{\text{Reaum.} \times 9}{4} + 32 = \text{Fahren.}$ Therefore 30° of Reaumur's is $= 99\frac{1}{2}$ of Fahrenheit's.

Smeaton, in the Phil. Trans. for 1758, has given the expansion of one foot of blistered steel $= \frac{23}{20000}$dths of a foot for 180° of Fahrenheit, which for three feet at 100° amounts to $\frac{23}{12000}$dths of a foot. Now the English foot is to the French foot as 4000 to 4263; therefore the foregoing expansion in French measure is $\frac{32683}{16000000}$dths of a foot for 30° of Reaumur, which is nearly $4\frac{1}{2}$ times greater than Le Roy states it. If we had taken hard steel it would have been greater still, in the proportion of 138 to 147.—T. S. E.

Article

Article V.

Fifth cause of error in watches, the little power of the regulators with regard to their motive force.

This inconvenience arises, according to what has been said above, from the resistance of the air, the friction of the suspension, &c. causing a considerable loss of motion in the regulator in each vibration; and since the balance of a watch ought to go of itself, (*partir au doigt**) as watch-makers say, (that is, it should be put in motion by the motive force, when this motion has ceased from any cause whatever,) this balance can only be very slight. Make the balance of a watch vibrate separately from the wheel-work, and you will see that if at first the vibration is 180° it will lose all its motion in 90″ in a horizontal situation, and in 60″ in a vertical one; instead of which a pendulum preserves the oscillatory motion given to it for twelve or fifteen hours without any foreign help: consequently the impression of the motive force, and the variations which arise from wear and from friction, are in watches, with regard to the effect that they produce on the pendulum, in the proportion of 15 hours, or 900′ to $1\frac{1}{2}$′.

Part III.

Description of the new marine watch, and of the means by which we have avoided the different causes of irregularities related above.

Article I.

Of wheel-work.

If the defects remarked in the construction of watches are the sources whence all their irregularities are derived, in order to render a work of this kind capable of the greatest accuracy possible, it is necessary, consequently, to collect together the opposite properties. Thus, after having given to this work the greatest simplicity of which it is susceptible, it is necessary,

1st, To reduce the friction to the least possible value,

* They are obliged to take this precaution, that the watch may not stop by the different motions which it may receive, and by the losses which it experiences in its motive force from the difficulty with which the wheel-work acts, and the effect produced when the hands are put to the hour, &c.

and

and to render the regulator as free and as powerful as possible.

2dly, To give to its vibrations the most perfect isochronism.

3dly, To apply an escapement, by means of which this isochronism cannot be affected.

4thly, To compensate the effects of heat and cold with accuracy and simplicity.

5thly, To dispose the regulator in such a way, that all the parts, being in an unconstrained state, they may remain the same after having been subjected to the greatest differences in temperature.

6thly, To render the machine invariable in the different positions and shocks which it may receive.

This is what I think I have executed in the following construction. For greater clearness, after having said a few words on wheel-work, I shall treat each of these articles separately, as in the preceding part.

M. Bernoulli, in the researches which I have already cited several times, wishes marine watches to be as large as good clocks are commonly made, that the pieces may be worked with greater exactness, and that their defects, if there are any, may be more easily perceived: this is nearly what I have practised in the new marine watch. It goes 38 hours. Plate I. fig. 6 and 7, shows the plan and profile of the movement on a diameter of three inches: it is composed of a frame *cccc* (fig. 6 and 7.) containing four flat wheels, toothed; the first, placed below the barrel *b b*, containing the main spring, has fifty teeth, and turns, by means of a pinion of ten leaves, that of the centre *m*, which is called the *minute wheel*, because it makes one turn in an hour; the minute hand is adjusted on its axis. The minute wheel, by a pinion of eight leaves, turns the third; and this, by a similar pinion, turns the fourth, called the *seconds wheel*, because it makes sixty turns in an hour, and carries the seconds hand on its axis. Lastly, the seconds wheel, by a pinion *p*, of seven, (fig. 6 and 7 of Plate I, and fig. 4. Plate III.) turns the balance wheel, or rather a ratch or kind of star *r*, (fig. 6 and 7, Plate I, and fig. 1, 2, 3, and

4, of

4, of Plate III.) having six radii placed without the frame: it is by means of this wheel that the escapement works.

With regard to the hour wheel, or that which carries the hour hands, H, (Plate IV.) it has 48 teeth, and is conducted by a lanthorn pinion of four, which being adjusted on the axis of the centre wheel *m*, carries the minute hand *e* (fig. 7, Plate I.) on its extremity formed into a square.

By this disposition, the hour circle, and those of minutes and seconds, have each one its centre, as we may see in Plate IV. I have preferred this, although the hour hand necessarily turns to the left, because it suppresses one wheel and some slight friction*; for we can never render a watch destined for the sea sufficiently simple, the accidents which may happen to an instrument being always in the ratio of the number of pieces that compose it.

Moreover, in this wheel-work, the simplicity of which is evident, all the wheels are horizontal, and the escapement wheel moves on the extremity of its pivot; whence arises great freedom in the moving part.

Article II.

Continuation of the description of the new watch: means by which the friction has been reduced to the least value, by rendering the regulator as free and as powerful as it can be.

This regulator or balance *vvvv* (fig. 7, Plate I, and fig. 1 and 6, Plate III.) is of steel. It weighs about five ounces; it is four inches diameter, and is mounted on an arbor AA (fig. 7, Plate I. and fig. 6, Plate III.) of about five inches. A frame of copper *xxxx*, &c. (fig. 6, Plate III.), to which is adapted the movement, holds this balance horizontally, suspended by the upper extremity of its arbor, by means of a very fine harpsichord wire F, which is attached to it, whose length is about three inches, and forms the same vertical right line as the axis of the arbor.

That this balance may turn very freely on its axis, each of its pivots is retained, with the proper play, between four

* Mr. Earnshaw has made this alteration in some of the clocks at the Royal Observatory at Greenwich.—T. S. E.

rollers,

rollers, turning freely in two small frames *c c, c c,* (fig. 6, Plate III.) the one for the lower pivot adapted to the lower part of the large frame; the other to the upper, for the pivot or trunnion *t,* (fig. 7, Plate I.) at some distance from which is attached the wire of suspension. All this is arranged with the necessary precautions, so that the wire and the axis of the balance may form always the same vertical line.

This balance thus suspended makes vibrations of about 20″ duration each, by means of the elasticity of the suspending wire. Two spiral springs *ss, ss,* (fig. 6, Plate III.) similar to those which serve as a mover in common watches, adjusted at the bottom of the balance arbor, by means of their ferrules, as the spiral in common watches, and in a centre of equilibrium absolutely *idle* (as M. Daniel Bernoulli recommends in the researches above cited), act so that these vibrations are each made in about half a second.

By this construction I avoid those defects of watches remarked Articles III and V. of the preceding part; for, the balance being freely sustained by the suspension wire, the friction which it would occasion by its weight, the very rapid wear which would result from it, &c., are absolutely suppressed, and by means of the rollers, whose properties are well known, those which are produced by the effort of the regulating spring, by shocks, and the lateral motions of the balance, by the effect of the escapement, &c., are reduced to the least quantity; whence it happens, that instead of only preserving its oscillatory motion for about a minute, as the balance of the watch in the experiment related, (Article V. Part II.) the regulator keeps going here more than half an hour; the two springs contribute also to this; their efforts on the pivots being opposite, are reciprocally destroyed.

Article III.

New method by which the most perfect isochronism is given to the vibrations of the balance.

It appears so simple, when we are occupied with the theory of watches, to try, first, whether the different lengths of springs produce no changes in the proportion which exists

between

between the time of their vibrations of different extents, and consequently, whether in these lengths there may not be one where the long and short are isochronous; so many reasons, drawn from the principles of philosophy and mechanics, appear to lead us to this conclusion, that we shall find it difficult to conceive how we have hitherto been ignorant of this important fact; much less can we conceive that it was not till after twenty years researches that we arrived at this discovery. Happily, men of science are not ignorant that the simplest things, almost always the most useful, are frequently so much the more difficult to discover, as, according to the remark of an illustrious Secretary to the Academy, *we are less inclined to seek for them.*

However it may be, it is constantly the case, as I have already said, Article II. Part II., that *in every spring of sufficient extent there is a certain length where all the vibrations, whether long or short, are isochronous.* I have experienced this in a great number of springs.

To procure, therefore, in the vibrations the most perfect isochronism, I adjust the spiral springs to the balance, and I set the marine watch to go (which, as we have seen, has no fusee,) twelve hours in the long arcs and twelve hours in the short arcs; that is to say, twelve hours with the moving spring highly wound up, and twelve hours with it almost unwound. If, in this last case, the going of the watch is more accelerated than in the first, it proves that these springs are too long, and I shorten them. On the contrary, if it is slower, I lengthen them; and thus I proceed until I have found the point where the watch goes very equably both in the high and low strain of the spring: I then diminish or increase the weight of the balance until the watch is regulated. This operation at first appears long; but practice renders it so easy, that at first sight I know actually, very nearly, the length of spring where all the vibrations are of equal duration. The two spiral springs are here of some help, because we can only act on the one, and the quantities which we lengthen or shorten it, produce less effect. For example, in my marine watch about one line of diminution in the lower spring makes it gain in the high strain of its main spring a second and a quarter in six hours more than in the low strain of this spring, where the arcs of vibration are reduced to about a quarter of what they are when the watch has just been wound up.

I shall add to what precedes, that I am certain, from a number of trials, and it is easy to verify, that, the long and short arcs of vibration once rendered isochronous by this method, all the intermediate arcs are rendered so also, with the greatest exactness: this is what I do not believe can easily be produced by compensation curbs, cycloidal cheeks, and other methods, by which they have hitherto attempted to render the vibrations of the spiral spring isochronous; and when, by dint of penetration and care, an artist has perfected such curbs, &c., can others* expect to succeed equally as well? The Academy, without doubt, want a machine whose success does not depend on such rare execution. It was probably some case of this kind which made a learned man say, that *the novelties produced by artists* rarely have their success confirmed by time, it being frequently owing to the particular attention which they pay to the execution of the pieces which they announce as their invention; instead of which, scientific men set a higher value on things more theoretic and less dependant on practice.

Article IV.

Where we again establish the necessity of giving to the vibrations of the regulator the greatest possible freedom.

I believe I have already proved (Article II. Part II.) that to give to a clock the greatest degree of accuracy which it is susceptible of, it is necessary that the vibrations of its regulator should have the most perfect isochronism and the greatest freedom possible.

And now that I am going to treat of the escapement, (whose disposition must always be relative to nature, and to the properties of the regulator which is used,) to leave no

* See Graham's Letter to Sully, in the *Description Abregée*, p. 75. Bordeaux, 1726. This work of Sully's is extremely rare even in France. Berthoud was many years before he could procure a copy of it. See note C, page xv, of his Introduction to the *Traité des Horloges Marines*.—T. S. E.

obscurity

obscurity on this capital subject, I think it will be best to examine it a little more in detail.

I acknowledge that, in watches, where the very confined space does not permit us to apply all the resources of the art, it would be very difficult to determine, whether some slight differences in the times of the long and short vibrations do not sometimes, as well as slight frictions, produce compensations, whence results a greater degree of accuracy, or rather less inequalities; but in the present case, where, being master of space, we may use methods less uncertain, it may be demonstrated, and I shall prove, that the isochronism and perfect freedom of the regulator are the only means to obtain a greater degree of truth.

In effect, friction is a thing subject to a thousand varieties incompatible with much exactness. Let the regulator in its vibrations experience some friction; it necessary follows, that the quantity of this friction will vary according as the contact of the air alters the polish of the rubbing surfaces, according as they alter each other, and the softest body leaves its parts on the hardest, according as the oil which we apply to soften the friction becomes more or less fluid, &c.

Let us suppose, also, that the vibrations of the regulator, abandoned to itself, are not isochronous, but that by some mechanical artifice we happen to render them all of equal duration; (by friction, for example, or, as this operates in some clocks, by the curves of the anchor escapement, or by that with a double lever, &c.;) I maintain, that such an isochronism, being subject to a thousand uncertainties, can never give the necessary precision in a time-keeper at sea. In effect, besides the varieties of friction, M. Le Roy* has demonstrated, in treating of the pendulum, that a diminution of the arc of vibration arising from that of the motive force, from the clogging of the wheels, or from that of the regulator, requires in each of these cases, in order to be compensated, the curves of the anchor to be altogether different; and likewise longer or shorter pallets in the escapement with a double lever. Now what he has

* See my Memoir on Clockwork, &c. published in 1750.

said

said with regard to the pendulum is evidently applicable here, since we have seen, Article II., that supposing the vibrations of the free balance isochronous in its application to the watch, the long arcs, which arise from less friction upon the pivots, would make it advance; but if they proceed from an overplus of motive force, they retard it, on the contrary; and that the difficulty of the action of the wheel-work would produce again a different effect, &c.

What will it be if we introduce the changes which arise in the magnitude of the vibrations from shocks and divers motions? By a little attention we shall find that an escapement can never render the vibrations isochronous in these different cases, unless it be a true Proteus, whose form, continually varying, adapts itself to these different circumstances.

I shall say on this occasion, that notwithstanding the experiments made with the time-keepers of Mr. Harrison, which are so strongly in favour of this work, the methods which are used to render the vibrations of the regulator isochronous appear to me very imperfect, and that I am here of the same opinion as the person who has made the report to the Board of Longitude. *Supposing*, says he, *the opinion of Mr. Harrison to be true*, (he speaks of the short vibrations which Mr. Harrison pretends are slower than the long,) *I am by no means certain*, continues he, *that the methods he employs are proper to produce the effect which he expects from them.* In truth, this article of the report appears to me absolutely unintelligible. *Mr. Harrison uses*, says the report, *two methods to render the motion of the vibrations equal: the first is, to put a pin, against which the balance may press, which augments its force; but it is found to be diminished, as Mr. Harrison pretends, when the vibrations are greater. The second method is, to give to his pallets such a form, that the wheels may press them less when the vibrations augment.* Although the terms of this Report are not very intelligible, and appear even incomprehensible, since it mentions *wheels* which press less when the vibrations augment, instead of which, by the description, *there is a very delicate spring which acts on the balance by one wheel only*; we may nevertheless suspect that the isochro-

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nism of the vibrations proceeds, in a great measure, from the curve of the escapement; that thus, by what precedes, this isochronism does not appear founded on fixed and invariable principles.

A still more powerful motive to determine us in favour of the isochronism and freedom of the vibrations, as far as it is possible to obtain them, is, that the same obstacles, of whatever nature they may be, arising from the air, or from some slight friction, which oppose themselves to the motion of the regulator, will have so much the less influence on the time of its vibrations as they are more free: this is what it is so important to clear up, and which I shall demonstrate by the following propositions.

Definition.

It is necessary to distinguish two times in the vibrations of a body: that which it employs to overcome the accelerating force, and that where this force restores to it the motion which it has lost. I call that where the accelerating force is surmounted, *retarded semi-vibration*, and that where the body returns to its point of rest, *accelerated semi-vibration.*

Proposition.

In every body that would make isochronous vibrations when disengaged from foreign obstacles and aided by an accelerated force, the resistance of the air, friction, &c. shortens the time of the retarded semi-vibration.

Suppose that at the instant when it begins to be retarded, a body A has, for example, the requisite velocity to describe a space of thirty, but that it only describes one of twenty, because the friction which it experiences consumes a part of its motion; it is required to show, that A will describe the space twenty, with its initial velocity thirty, in less time than, if having experienced no resistance foreign to the accelerating force, it had described this same space twenty by means of an initial velocity of twenty. The following is the way I prove it:

It is only in the last point of the space twenty, that A moves with the same velocity which it would have had in

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this point, if it had only consumed the velocity twenty instead of that of thirty; in the penultima it has the necessary velocity to overcome the resistance of the accelerating force; in the last it has besides, that of friction, or of the air, which it experiences there; in the antepenultima it has the requisite velocity to surmount the resistance of that same force in the two last, plus that of the air, friction, &c. Applying the same reasoning to all the other points of the space twenty, we shall find that in the present case the velocity there is greater than if, disengaged from every obstacle foreign to the accelerating force, A should describe the space twenty only, by means of the initial velocity twenty: the same must be concluded for every other space. Therefore the foreign resistances experienced by a body in vibration shorten the time of the retarded semi-vibrations.

Corollary I.

The inverse of the preceding evidently takes place in the accelerated semi-vibrations.

Corollary II.

The obstacles which may be opposed to the motion of a body in its retarded semi-vibration, being so many causes which make it stop the sooner; on the contrary, in the accelerated semi-vibrations, the foreign obstacles destroying a part of the acceleration, and hindering this semi-vibration from being made so readily, (since every body which oscillates necessarily feels some foreign resistances, whether from the air, from the friction of the parts which sustain it, or from the particles themselves of the spring which holds it;) it follows, that in every body which vibrates, the retarded semi-vibration is always quicker than the accelerated semi-vibration which succeeds it; and that the more considerable the resistances are which we have just spoken of, the greater is the difference between the time of the acceleration and the retardation.

Observation I.

In the vibrations that a body makes by the help of an elastic force, or of gravity, if we admit that it is the effect of a fluid, a second cause again renders the accelerated semi-vibrations slower than the retarded; it is in the latter that the active principle, whatever it may be, has always its full effect; whilst in the others it acts only with the excess of velocity which it has on the body when it returns to its point of rest.

Observation II.

If in the vibrations of a body the difference of time employed for each of the parts which we have distinguished in it is not sensible, the small foreign resistance which may take place will not sensibly alter the time of the whole vibrations; for the retardation which happens in the accelerated semi-vibrations will then be compensated by the gain which they will produce in the retarded semi-vibrations, and *vice versâ*.

Observation III.

But when several causes render the accelerated semi-vibrations sensibly slower than the retarded, then the whole vibrations are considerably retarded by the new resistances which take place; for, 1st, in the accelerated semi-vibrations, the body having less velocity than in the retarded of the same magnitude, the force which it has to overcome the new obstacles which are opposed to it is so much the less: 2dly, We have seen that the resistances produce always in a body in motion obstacles proportional to the time that it remains exposed to them; these obstacles are therefore more considerable in the accelerated semi-vibrations than in the retarded; consequently, the retardation which they produce in the former is greater than the advancement which they occasion in the latter, and this retardation follows the ratio of the square of the difference of times employed in each of the accelerated and retarded semi-vibrations.

Corollary III.

Hence we see, 1st, That foreign resistances necessarily tend to destroy some little of the isochronism of the vibrations of a body, and to render them slower at the same time that they diminish their extent: this is what the experiment

riment proved to us Article II. Part II. 2dly, That the more we reduce the friction of the regulator the nearer we approach to the compensation, of which we have spoken in the second observation. 3dly, That by the preceding we are very distant from this compensation in common watches with a dead escapement.

[To be continued.]

X. *Extract of a Letter from M.* PROUST *to M.* VAUQUELIN, *upon Porcelain, and the nutritive Use of the Lichen islandicus* *.

Madrid, Dec. 22, 1805.

WE have been to visit the porcelain manufactory of M. Sureda, who makes the finest biscuit china I have ever seen. He does not make use of kaolin, however, but of a siliceous-magnesian stone called sea-froth or lithomarga, found at the gates of Madrid †. We shall send you some specimens which must astonish you. M. Sureda covers his china with feldspars of Galicia, which are very elegant. You may regard the above kind of stone as one of the best for making chemical furnaces. When it comes out of the quarry it is shaped like soap wedges. The lightness of these furnaces is extraordinary; and they never melt, however strong the fire may be raised. Besides magnesia, silex, and some atoms of argil and lime, this stone contains a little potash, which contributes not a little to the beauty and fineness of the china.

I have now to mention a fact perhaps as interesting as the foregoing. Don Mariano la Gasca, a student of Cavanilles, and a young botanist of great promise, has sent me a quantity of *lichen islandicus*, which he discovered in the mountains of Leon, where it grows in great abundance.

I expected to have only found a more or less tinctorial substance; but I found that it was an excellent plant for eating when it was cooked, very tender, and that it

* From *Annales de Chimie*, tom. lvii. p. 196.

† For a memoir on this substance, see Philosophical Magazine, vol. iii. p. 165.

ought

ought to be rescued from obscurity, as a resource furnished by nature in every climate, and which has not been hitherto known. I hope the botanists of Paris will cultivate this plant. Upon tasting it, you will find it an excellent pulse. I think I have seen it formerly at Vincennes, and in the Bois de Boulogne.

One pound of dry lichen yields three pounds of boiled herb very well tasted, which may be eaten with oil, beer, or in various other ways. We have already eaten it six times in our family, and all my friends were highly pleased with it. Its tissue is purely membranous; it contains neither wood nor thready substance; this renders it very pleasant to the teeth. It is extremely probable that in the numerous kinds of mosses there may be several others equally nutritive, and perhaps still better. Although very elastic after boiling, there is nothing of an animal nature in the lichen, as its products are like those of sugar; and this surprised me most of all. One pound of this lichen furnished about eight pounds of soup, which became jellied like meat broth. It is a little bitter, but not more so than endive water. I seasoned it with sweet and bitter almonds, citron bark, and sugar; and I produced a very pleasant mess. Its mucilage is gelatinous, very different from gum: it is the same, I think, as that of fruit. I am now about to occupy myself with other researches on the subject, to ascertain if this plant would furnish any thing useful in dyeing. In the mean time nature does not furnish any matter more nourishing than this vegetable.

XI. *Thirty-second Communication from Dr.* Thornton, *relative to Pneumatic Medicine.*

To Mr. Tilloch.

October 21, 1806.
DEAR SIR, No. 1, Hinde-street, Manchester-square.

The following is an extremely interesting case, as the disease cured is considered very dangerous; and it was accomplished chiefly by means of the vital air alone.

Case of White Swelling cured by Vital Air.

Sarah Copeling, æt. 24, servant to Mr. Harrison, No. 14, Little Tower-street, had an extremely large scrophulous tumour on the right side of the neck; and this disposition showed itself also in the knee of the same side, which was greatly swelled and enlarged, rendering her extremely lame. The tumour in the neck was of six years duration, and the white swelling in the right knee had existed three months when she commenced the inhalation of the vital air. A saturine lotion was the only application used to the knee: this plan was pursued during two months; when her constitution became so much invigorated, that the swellings in her neck disappeared to the sight, being so much reduced as no longer to be observed, or thought worthy of attention; and the white swelling entirely gave way: and this benefit has now continued six months. No medicine of any kind was taken; therefore the constitutional change was effected by the sole power of the vital air.

Observations on this Case.

1. When Dr. Beddoes inhaled the vital air by way of experiment, he remarks, "that his complexion from being sallow became ruddy, and that he was diminished greatly in size, being naturally disposed to obesity; and his appetite was rendered remarkably keen."

2. This experiment, which was repeated for the space of six weeks, proved the power of vital air in promoting the action of the absorbents.

3. He also remarks, "that he felt in consequence so much warmer than usual, that he was obliged to lay aside a blanket."

4. Now as torpor is the character of scrophula, is not the vital air indicated from this experiment by Dr. Beddoes, in scrophula?

3. The quantity inhaled daily was two quarts, mixed with about fourteen of atmospheric.

I remain, dear sir,

Your obliged devoted servant,

ROBERT JOHN THORNTON, M.D.

XII. *Ob-*

XII. *Observations of M.* Bonefos, *Assistant Physician of the Infirmary at Perpignan, upon Fumigations with the Oxygenated Muriatic Acid*.*

Towards the commencement of last year, a person accused of a capital crime was conducted to the prison of Perpignan and shut up in a cell, the capacity of which was about 60 or 65 cubic metres. This unfortunate prisoner was afflicted with a severe dysentery. When I was called in, his cell exhaled a most infectious smell; the palliass upon which he lay, and the rags which covered him, were impregnated with fecal matter. The jailor opened the door with great repugnance, but would not enter. I immediately made a strong fumigation, according to Guyton Morveau's process. Scarcely was the vapour of the oxymuriatic acid gas liberated when the fetid smell was annihilated, although the fecal matters still existed in this confined place. I approached the sick man and conversed with him, experiencing no disagreeable sensation: the jailor followed my example; he entered, and every necessary attention was paid to the prisoner. A fumigatory apparatus continued to furnish gaseous emanations during the time requisite to cleanse the cell. A clergyman came to visit the prisoner some time afterwards, and spent three quarters of an hour with him, without being in the least incommoded. The fumigation was repeated the same day. All the prisoners, the jailor, the turnkeys, and the gens d'armes, learnt, with surprise, a fact so speedily and so easily obtained. The jailor asked me what was necessary to renew these fumigations. I furnished him with a sufficient quantity of the mixture of muriate of soda and oxide of manganese, prepared in proper proportions, and an analogous dose of sulphuric acid. He has since established fumigatory vases in the various parts of the prison where there were bad smells.

A few days before, I had cleared from infection a great part of the house of M. Durand, a very respectable merchant

* From *Annales de Chimie*, tom. lvii. p. 184.

 of

of this city. A considerable quantity of cochineal, grown mouldy and in a state of fermentation, emitted a most disagreeable smell; the oxymuriatic acid gas destroyed all these putrid emanations, allowed us to approach the cochineal without fear, and we were able to save a part of this precious commodity.

XIII. *Memoir upon Animal Fat, and some Medicinal Preparations which are administered through that Medium. By M.* Vogel*.

Fat has been a long time the object of chemical inquiries. Some have occupied themselves with establishing its characters, and others in ascertaining the propriety of its application in the healing art. The late M. Vogel, professor of chemistry at Gottingen, is one of the first whose attention was occupied in discovering the nature of this substance. On directing his observations to distillation, he perceived that human fat yielded a liquid product which had all the properties of an acid.

The author of this memoir was desirous of ascertaining the difference which existed in the fat of animals whose exercises are violent, such as the wolf, the hare, &c., as well as that of some carnivorous birds; but the difficulty of procuring a sufficient quantity at one time compelled him to defer his labours to a future period.

His first experiments had for their object to examine hog's lard, *per se*, and combined with other substances. Although this labour is still incomplete, it may lead to some very useful observations on the preparation of medicines.

Effects of Light on Animal Fat.—It is well known that fresh lard well purified is without any smell, and of a mildish insipid taste. Exposed for two months to the solar rays, without the contact of the air, it acquires a very rancid penetrating smell, a bitter taste, which burns the throat,

* From *Annales de Chimie*, tome lviii. p. 154. Extracted by M. Bouillon Lagrange from an Essay read in the Pharmaceutical Society of Paris by the late M. Vogel, Chemical Instructor in the School of Pharmacy at Paris.

and

and changes from a white to a yellow colour, without, however, acquiring any acidity. When the contact with the air is added to the solar rays, the same phænomena take place, and it always becomes acid.

Caloric.—Fat melts at 32·34 of Reaumur (90·21 Fahr.). At this temperature it remains in fusion without undergoing decomposition; but when the temperature is pushed beyond 80° (174° Fahr.) it begins to be decomposed.

The author did not think necessary to describe the distillation of hog's lard: this operation has been already performed, and its products examined, by Messrs. Von Crell, Guyton and Thenard; he only observes that fat, when well washed, does not yield ammonia upon distillation, while that which has not been washed yields very sensible traces of it. The water in which muscular substances have been washed, the cellular tissue of which is not exactly separated, takes away from it a notable quantity of gelatinous animal matter which accompanies the membranes; it is this animal gelatinous matter which produces the ammonia in distillation.

Sulphur.—Fat mixed with half its weight of sulphur sublimed and washed, forms what is vulgarly called sulphur pomatum.

This compound was examined four days after its preparation, as well as a similar mixture a little older, and no traces whatever of sulphuric acid were discovered. By a slow fusion in B. M. we separated, by decantation, a quantity of fat; and on passing the rest through fine linen, we obtained the greatest part of the fat employed: it had a gray colour, a bitter, sharp, and very strong taste; it congealed much more quickly than common fat when cooled, and it blackened silver vessels.

Thus there is sulphur dissolved in this compound: it will even be found in solution, every time it is employed in friction. The elevation of the temperature facilitates this solution.

We know how rapidly sulphur penetrates through places far remote from the spot where it was made use of in the shape of pomatum: this is not so surprising when we reflect that the sulphur is in solution. I am ignorant, says

 M. Vogel,

M. Vogel, if sulphur, divided by any other vehicle, (thick mucilage or gelatine, for instance,) acts in an analogous manner. I suspect, however, that when it is employed in friction, divided by means of one of these bodies, its results would be different from those of sulphur dissolved in fat.

If we raise the sulphurated fat to the boiling point, and if it is hastily decanted and cooled, a part of the sulphur is precipitated ; but if it has been allowed to cool slowly, the sulphur then crystallizes in beautiful needles.

When we distil in the open fire sulphurated fat in a luted glass retort, and receive the products over mercury, we obtain a great quantity of gas, which, being collected and examined, appears to be a mixture of plenty of sulphurated hydrogen gas, carbonated hydrogen gas, and a little carbonic acid gas. We never found any sulphurous acid gas, as several chemists have asserted.

From the moment that the elastic fluids cease to pass, white thick vapours are perceived, condensing with difficulty, and there is sublimed at the neck of the retort a yellow matter, which was merely fat mixed with a little sulphur : the liquor of the receiver looked milky ; it yielded, upon cooling, small white crystals ; this was merely sulphur in minute division. The retort contained a brilliant prismatic charcoal in abundance.

Sulphurated hydrogen gas, passed through melted fat, effected no change on it, and was not dissolved in it.

Phosphorus.—I melted, says the author, half an ounce of fat in B. M. ; I added, after the fusion, two grains of good phosphorus, very transparent ; I kept the whole for about a quarter of an hour at the same temperature : I took care not to agitate the liquid too much, in order to avoid the action of the air, which would have acidified the phosphorus.

When the fat was cold, I recovered a part of the phosphorus which was not dissolved. This fat had a slight smell of garlic, and a disagreeable taste ; it reddened turnsole ; it formed a very abundant black precipitate with the nitrate of silver, and a less abundant precipitate of the same colour with the neutral nitrate of mercury at the minimum.

As

As the heat of the B. M. was not sufficient to dissolve the phosphorus employed, I made other mixtures of fat and phosphorus in different proportions, which I brought to the boiling point: this method favoured its solubility. After many trials, I ascertained that one ounce of fat, at a slight ebullition, can dissolve five grains of phosphorus, a part of which is precipitated upon cooling.

This phosphorated fat was washed several times in boiling water: the washings were acid, blackened the nitrate of silver, and formed a flaky precipitate with lime water: this water had taken from it its acid property, but not that of blackening the nitrate of silver: thus, one part of the phosphorus remained in a true solution without acidifying.

These two kinds of phosphorated fat, that which had been prepared in B. M., and that prepared by ebullition, either washed or not, emitted no light in the dark at a temperature of 10·15 (50·27 Fahr.), nor even by rubbing with the hand; but if the temperature was raised to 60° (140° Fahr.), the luminous effects were a little visible. The phosphorated fat, the undissolved phosphorus of which had been carefully separated, did not shine in the ordinary temperature.

I distilled twelve grains of phosphorus with two ounces of fat: the matter soon assumed a charry appearance, much more speedily than common fat submitted to the same operation; there was liberated at the beginning, phosphorated hydrogen gas, which took fire in the receiver; and we afterwards obtained under a bell-glass, in the mercurial apparatus, phosphorated hydrogen gas and carbonated hydrogen gas. The receiver contained fat which had been blended with phosphorus and phosphorated hydrogen gas. After cooling, it took fire with the contact of the air, and burned the fat rapidly.

Whatever is the temperature, therefore, employed to dissolve phosphorus in fat, there is formed every time a greater or less quantity of phosphorous acid:—this inclines me to think that the same thing happens in many other phosphorated compounds.

M. Bouillon Lagrange last year, in his lectures upon fixed and

and volatile oils, has presented analogous results; he has shown, that the solution of phosphorus in one or other of these oils can never be considered as a regular medical application; that there is immediately formed a small quantity of acid, and that this quantity increases through time.

All the experiments hitherto mentioned were made with the contact of the air, the result of which always was an acidification of the phosphorus.

I did not omit to repeat several experiments without the contact of air, such as M. Boullay had announced in one of his reports to the Pharmaceutical Society.

In a small flask, almost entirely filled with melted fat, I put a morsel of phosphorus. I hastily corked it, and heated it for five minutes in B. M.: a part of the phosphorus was dissolved; and I remarked, with M. Boullay, that fat was not acid, but that it blackened the nitrate of silver. A few minutes afterwards, upon decanting or agitating liquid fat in the open air, it acquires acidity.

This speedy change, therefore, gives us little hope of finding a sure or constant medical application in the solution of phosphorus, in spite of the processes continually recommended for this purpose. The physician, therefore, can never be certain of the quantity of phosphorus, because the contact with the air is unavoidable.

Being desirous of knowing the action of fat upon phosphorated hydrogen gas, I passed a piece of fat under a bell-glass filled with mercury; I liquefied it with lighted charcoal, which I carried round about the bell-glass; at the same time I passed phosphorated hydrogen gas into it: there was very little apparent absorption. For the greater certainty, I varied the experiment in the following manner:

Into a cylinder nearly ten inches long and eight lines in diameter I poured melted fat until it was full: having carried it to the mercury tub, I made phosphorated hydrogen gas pass into it, so as to drive off a part of the fat; I corked the cylinder below the mercury with a linen stopper; I plunged it for some minutes in hot water to keep the fat in fusion; I shook the mixture continually until it cooled; I uncorked it below the mercury, it rose five or six lines

above

above its level; the gas which remained in the cylinder did not inflame in the air, but took fire immediately on the approach of a lighted candle.

The gas was thus entirely decomposed, and the whole quantity of phosphorus was decomposed. I attribute the cause of this absorption not only to the loss of phosphorus, which, without doubt, had diminished the volume of the gas, but also to its temperature; since it was recently liberated, and had passed through the hot fat, which would consequently dilate its volume a little.

The fat which remained had all the characters of phosphorated fat; it soon became acid in the air.

The Acids.—As the sulphuric and muriatic acids are not very interesting, the latter having even no action at all upon fat, I directed my attention chiefly to the phænomena presented by the nitric acid.

This acid has become a valuable agent in the hands of the chemists. To the action of this acid upon organized bodies we are indebted for a great number of discoveries.

It is well known how much facts have multiplied since we have been able to explain the changes which take place upon animal and vegetable compounds.

Berthollet has in some measure paved the way by his important labours upon animal substances; and the experiments of Messrs. Fourcroy and Vauquelin have left us nothing more to desire on that subject. These gentlemen have considerably enlarged the sphere of our knowledge in this department, so difficult of comprehension, and so useful in the science of medicine.

M. Fourcroy was the first who ascertained the action of the nitric acid upon fat. M. Alyon and several other chemists have since presented interesting results on the subject.

I treated fat in the manner prescribed by Messrs. Fourcroy and Alyon for making the oxygenated pomatum. The latter observes, that it does not stand in need of washing, as it is not acid: I repeated the process prescribed by him with one ounce of acid at 32° to the pound of fat; I employed afterwards nitric acid of an inferior strength, from 30° down to 24°; the oxygenated fat was always acid.

I made

I made this experiment in a retort in the pneumatic apparatus, and I obtained azotic gas as the produce: this gas was not disengaged pure, as M. Alyon announced; it was mixed with nitrous gas and carbonic acid gas, as justly observed by M. Van Mons.

Fat thus oxygenated, of a hardness equal to suet, melts at the temperature of 36° or 38° of Reaumur, (96° to 100° of Fahr.)

I boiled it with water, which acquired a citron yellow colour from it, had a bitter and sharp taste, reddened turnsole paper, and constantly precipitated the acetate of lead and nitrate of mercury. This water distilled in a retort, almost to dryness, yields a colourless white liquor which contains a quantity of acetic acid; it does not then precipitate the above metallic solutions.

The washings of fat, evaporated to the consistence of a thick liquid, deposit, upon cooling, a tenacious brown substance, which attracts humidity from the air. The liquor being decanted and exposed to evaporation in a stove, an infinity of very brilliant white needles crystallize in it. I took these crystals at first for oxalic acid; but lime-water was not in the least affected by them; besides, they had no other of the properties of the oxalic acid: we shall see their nature a little further on.

However often we may wash oxygenated fat, its yellow colour and its acidity never leave it. After the twelfth boiling it is still yellow, and the water coming from it reddens turnsole.

Alcohol acts differently: on boiling it with oxygenated pomatum, it dissolves a very great quantity of it; upon cooling, plenty of flakes are separated from it, which, being collected and dried, yield an oxygenated fat which is singularly bleached. The fat remaining is also whiter; the alcohol acquires a yellow colour, and becomes acid: it retains enough of matter in solution, to be abundantly precipitated by water.

I evaporated this alcohol; plenty of yellow acid fat remained; water effected its solution in part.

Boiling alcohol, frequently employed to wash oxygenated fat,

fat, does not completely take off its acidity; it rather dissolves it for the most part, and this last liquor is still acid.

Since the acid adheres so intimately to the fat, I tried to separate it from the latter by salifiable bases, and I made use of lime-water, which I boiled with oxygenated fat; the fat lost its alkalinity, and acquired a colour of a citron yellow. This neutral liquor, which I regarded more as a combination of lime with an acid, than fat, was abundantly precipitated by the acetate of lead.

Evaporated to the consistence of a syrup, it is discoloured by the nitric and muriatic acids, which form in it a whitish precipitate; at the same time, when we pour the acid, a very rancid odour is manifested.

Barytes water acts upon oxygenated fat in a more efficacious manner. The orange yellow colour which the water acquires from it is equally destroyed by the acids. I poured into it a quantity of sulphuric acid sufficient to carry off the barytes; I boiled the whole, and I filtered the liquor while boiling.

The filtered liquor, which contained no barytes, was in a great measure evaporated in a sand-bath; small fine needles were crystallized mixed with silky tufts, not precipitable by lime water, insoluble in alcohol, and which were not sublimed in close vessels.

When fat is boiled with concentrated nitric acid, and the ebullition is continued, adding water from time to time, a white crystalline powder is formed upon cooling.

This substance is rough to the touch, insoluble in alcohol, much more soluble in boiling water than in cold water. By its combination with the bases, and by several other characters, I was convinced that it was mucous acid*.

Fat thus oxygenated at the maximum is soft, of a brown colour, sensibly soluble in water, and very soluble in alcohol. Its washing was saturated by potash; from this resulted a leafy salt, attracting humidity from the air, and

* Beef suet, although it decomposes less strongly the nitric acid, also yields mucous acid.

which

which liberated acetic acid upon treating it by the sulphuric acid *.

The precipitate formed by the acetate of lead in the washing of oxygenated fat, is nothing else but the fat itself combined with oxide of lead, and which carries with it a little mucous acid; the former floats above, when the precipitate is decomposed by the sulphuric acid.

Oxygenated fat being very soluble in alcohol, a great part of it may be precipitated by water. By the energetic action of the concentrated nitric acid upon fat, there is a notable quantity of nitrate of ammonia formed, of which we may be convinced by mixing potash or quicklime with the washings.

Oxygenated Muriatic Acid.—The action of this acid upon fat not having been yet described, I think it may be useful to enter into some details.

I passed a great quantity of oxymuriatic acid gas into fat kept in fusion in B. M., the gas, before arriving at it, passing through a vessel containing water. The fat absorbed a very great quantity of it. I continued to pass it until the bubbles no longer arose.

After cooling, the fat was considerably augmented in weight; its whiteness became dirty, and its consistence was entirely changed; it was soft, resembled an oily thick liquid, which might be easily poured from one flask to the other, even at the temperature of 10° (50° Fahr.): in the air, white vapours of acid are disengaged at the commencement.

Having left it nearly two months in the air, it resumed a little more solidity, but never that of common fat, much less that of oxygenated fat: its taste was rancid, not sensibly acid, leaving behind it a slightly bitter taste, which burns the throat. The simple muriatic acid is so combined with fat, that by washings in boiling water I only took off a very small quantity of it. The nitric acid disengages the above acid from it in abundance, with effervescence and

* Rancid fat, and very old suet, also furnished me with acetic acid upon treating them in this manner.

white

white vapours. What is singular is, that the nitric acid is no longer decomposed in it, notwithstanding the quantity employed; and the fat acquires neither colour nor solidity. I shall now proceed to speak of the effects of the metals on fat.

[To be continued.]

XIV. *Report made to the Class of Physical and Mathematical Sciences of the French Institute on the 6th of January* 1806, *by* M. Pinel, *upon the advantageous Results obtained by* M. Desgenettes, *from the Use of Fumigations of Oxymuriatic Acid.*

M. Guyton and myself were directed to report to the society on the use of fumigations of oxymuriatic acid gas employed by M. Desgenettes according to the ordinary processes, and upon the results he obtained from it. These fumigations seem not only to have had an influence upon the salubrity of the air, but also in the cure of diseases.

The author of these observations remarked, at first, that the military prisons of the capital regularly furnished a number of cases of adynamic fevers to the hospitals, which frequently spread to the patients in the nearest beds, and also to the nurses; he adds, that for nearly a year past no such infections have taken place. M. Desgenettes also observed that gangrenes, being common among the wounded, were also greatly checked: the specific smell of the gangrene was not annihilated, but, according to him, it was modified by the fumigations.

Another general observation made by M. Desgenettes was, that for several years the scurvy had been very frequent; that three persons, in particular, had been affected with it in a very violent manner; and, in short, one of them had been shut up from the rest on account of the insupportable infection spread by torrents of sanious saliva. Nevertheless, by means of fumigations, this specific smell was neutralized. It seemed to be concentrated, as it were, round the patient. The nurses were then permitted to sleep near these scorbutic patients

patients without any bad effects, and they attended them regularly.

M. Desgenettes does not confine himself to these general remarks; he renders the favourable effects of fumigations, according to M. Morveau's method, much more manifest, by giving monthly, for these last nine months, the respective numbers of patients admitted into the military hospital, with the addition of those patients who had been there from the first; this formed a sum total, which may be easily compared with the number of those who died during the nine months. Upon turning over the registers it appears, that out of 3617 patients, 223 died; that is to say, a sixteenth, or 0·06. This report of mortality is one of the most advantageous that could be expected in an hospital; and it is so much the more conclusive in favour of mineral fumigations, according to M. Guyton's process, that the military hospital of the capital often contains serious diseases, and is, for the most part, filled with refractory conscripts, and veterans not in garrison, who never come into the hospitals until they have suffered a great deal elsewhere.

Such are the facts related by M. Desgenettes; and they confirm more and more the advantages of mineral fumigations, already established by other numerous observations collected in the work of M. Guyton. It may be inferred that these fumigations not only act as preservatives against adynamic fevers and scurvy, but also concur in the cure of the same maladies, by destroying the baneful influence exercised upon sick persons by deleterious miasma. The use of this salutary process in hospitals ought to excite a lively interest. I shall here add some facts within my own knowledge, and which have shown, in the Hospital de la Salpetriere *, that some variations in the mode of mineral fumigations are necessary, according to local circumstances.

The distribution which I made of the lunatics, to enable me to follow out, with proper attention, my object of research, was to confine in particular apartments such of them as were attacked with other incidental maladies, as agues or

* The hospital for lunatics.

chronic

chronic diseases, intermittent or continued fevers, rheumatisms, dropsies, &c., which require a particular treatment. Some of these patients were more or less affected; and we may easily imagine the cries and confusion produced by the first trials of fumigation a year ago, when the lunatics were enveloped in clouds of vapours: but experience has shown that such fumigations as were then used retard the cure of lunatic patients. At that time I employed nitric fumigations, following the usual method of throwing successively small quantities of nitrate of potash into a little sulphuric acid, put in a glass. At other times I caused to be carried into different parts of the hall a mixture of muriate of soda and oxide of manganese in the usual proportions, pouring, by degrees, into it some drops of sulphuric acid, that slight vapours only might be formed.

Such patients as exhale a fetid smell are confined at the end of the hall, and it is there where the ordinary fumigations are used. But I think I have ascertained the source of the evil by remarking, that, in general, the lunatics attacked with adynamic fevers, scurvy, or even scorbutic gangrene, who were brought to the infirmaries, came always from certain infected places, which I resolved to discover.

The first place I examined was a small room containing fourteen beds, and in which were confined such lunatics as are of a very advanced age, or in a complete state of madness or idiotism. The most of them are constantly bed-rid, and in such a state of stupor and imbecility that they can scarcely indicate the object of their necessities. The infection must arise from the insalubrity of the air in a place inhabited by such unfortunate persons, heated in winter by a stove, and very small besides. Whatever precautions were at any time used, this place was always very unwholesome; and it was there where adynamic fevers and scurvy were continually cherished previous to the repeated use of fumigations of oxymuriatic acid gas, which were repeated once or twice a month in summer, and oftener in winter, producing little or no inconvenience on account of the state of stupor and insensibility of lunatic patients. I then mixed fifteen decigrammes of muriate of soda with three decigrammes of oxide of manganese, which I distributed in two crucibles placed at a certain distance from each other, and into each of which we successively poured six decigrammes of sulphuric acid.

Another place of the hospital, where the infected state of the air required reiterated mineral fumigations, was where the cells were situated for the confinement of certain dangerous or delirious melancholic patients. Some of these are confined in strait waistcoats, others are left to themselves, as experience shows that a superfluous restraint only prolongs the duration of lunacy. Their rooms are commonly very small, their dimensions not being more than two metres in length and breadth, and three metres in height: this only forms a capacity of twelve cubic metres for each lunatic. The air not circulating in these close apartments, rendered humid by frequent washings, we may easily conceive how fetid emanations are accumulated in them, as well as from the description of patients who inhabit them, who from negligence or a propensity collect around them every kind of nastiness: it was in these unwholesome apartments that the fumigations of the oxymuriatic acid gas were most frequently practised, by filling them successively with these mineral vapours, and shutting the door and window. The infectious smell is always destroyed by this method in a sure manner; and I did not perceive any inconvenience to arise to the lunatics who were lodged in them, having previously removed the worst among them into a neighbouring cell until the vapours were dispelled.

In making use of these fumigations, there is a variety which ought to be remarked. Some of these apartments are ill paved, and the urine then remains more or less between the crevices of the pavement; this causes a very disagreeable smell of ammoniacal gas. In this case I made use of fumigations with plain muriatic gas, by simply pouring sulphuric acid upon muriate of soda: this produces a rapid combination of muriatic acid gas with the ammoniacal gas, and forms, without doubt, a new chemical compound which does not permit the old smell to continue. We must, however, take care to remove the patient for two or three hours,

hours, on account of the action of the muriatic vapours upon the nostrils and the lungs.

Several things may concur to diminish the number of adynamic fevers and scurvy in an hospital; but every thing leads us to believe, that one of the most powerful means is the use of mineral fumigations; and that they act particularly by destroying the deleterious miasmata which communicate infection.

XV. *On the Acetic Acid and its Ether. Extracted from a Letter of M.* Gehlen *to M.* Guyton*.

Berlin, 31st Dec. 1805.

You are acquainted with the assertion of Scheele, that the acetic acid, without the intervention of a mineral acid, is incapable of forming ether. M. Schultze, of Kiel, has proved, by experiments, that this assertion is well founded. M. Lichtemberg and myself have repeated these experiments, and found them very exact; but it is necessary that the acetic acid should be very pure, so that the acetates of silver and barytes may have no re-action upon it. A *minimum* of sulphurous acid is sufficient to form ether. On distilling to dryness a mixture of equal parts of acetic acid and absolute alcohol, (*i.e.* prepared according to Richter, with the melted muriate of lime,) no trace remains in the retort, and no gas is formed. On mixing the acid and the alcohol they are not sensibly heated.

I found, also, that the acidity of the acetic acid is not always in direct proportion to its specific gravity. An acid, (obtained from one part of acetate of soda, deprived of the water of crystallization by means of one part and a half of smoking concentrated sulphuric acid), crystallizing at a low temperature, which had a specific gravity of 1·055, saturated much more alkali than another acid, (distilled from the acetate of copper crystallized by 0·75 of concentrated sulphuric acid), having a specific gravity of 1·075.

* From *Annales de Chimie*, tom. lvii. p. 91.

XVI. *Observations upon the two Preparations of Acetic Ether. By M.* Henry, *Professor of Pharmacy in the Pharmaceutical School of Paris*.*

Having been directed by the society to ascertain the difference which exists between acetic ethers prepared immediately, or by the intermedium of the sulphuric acid, I have now to communicate the experiments made with that view.

M. Gehlen, in a letter addressed to M. Guyton, lately published in the Annales de Chimie †, insists that, according to the assertion of Scheele, it is proved that the acetic acid, without the intervention of a mineral acid, is not capable of forming ether.

The author does not tell us what acetic acid he employed; he merely asserts the fact, recommends very pure acetic acid to be used, and adds, that "a *minimum* of sulphurous acid is sufficient to form ether."

I know that in a great part of Germany they extract the acetic acid from acetate of soda by the intervention of sulphuric acid: in place of that pointed out by Pelletier, acetate of copper is made use of. It was the latter I employed, it did not contain an atom of mineral acid.

But, without entering upon the discussion of a point long ago decided by constant facts, I return to the present object of my inquiries.

We are indebted to Pelletier for the process of obtaining acetic ether, which consists in making a mixture of equal parts of rectified alcohol and acetic acid, in re-cohobating three times the produce of the distillation upon the residue, and rectifying the ether over potash.

I followed this process, with this difference, that I re-cohobated the produce of the distillation six times.

From a mixture of 500 grammes of rectified alcohol, at 36 degrees, and an equal quantity of acetic acid at 11 degrees, I obtained 495 grammes of ether at 24 degrees, im-

* From *Annales de Chimie,* tome lviii. p. 199.

† See the preceding article.

miscible

miscible in water, of an agreeable smell, reddening strongly the blue vegetable colours. No particular gas was liberated during the operation, merely atmospheric air displaced by the ethereal gas. I rectified this ether over pure alcohol (concentrated by potash): it did not redden any longer the blue vegetable colours; it marked 25 degrees in the areometer, and weighed 420 grammes.

As to the process by the intermedium of the sulphuric acid, indicated by our fellow member Durosier, it consists in introducing 500 grammes of the acetate of copper, in powder, into a tubulated retort, to which the apparatus of Woulff is adapted. A mixture is afterwards made of equal parts (500 grammes of each) of rectified alcohol and sulphuric acid, which is introduced, when it is cooled, by the tubulure of the retort; it is gradually heated, and 640 grammes of acetic ether are obtained, mixed with sulphurous acid in a small quantity, marking in the areometer 25 degrees and a half, reddening strongly the blue vegetable colours, forming a precipitate with water of barytes and lime-water. During the operation a small quantity of an elastic fluid is liberated, which I ascertained was sulphurous acid gas.

I rectified this ether with 50 grammes of alcohol (purified by potash); and in order to ascertain if any sulphuric ether existed in this ether, I divided the produce of the rectification into portions of 50 grammes each.

The first produce marked 31 degrees in the areometer; the second, 28 degrees; the third, 27½ degrees; the fourth, 26½ degrees. These different products marked 28 degrees, and weighed 535 grammes.

In order to ascertain if it was easy, by dividing the products into portions, to ascertain the presence of sulphuric ether in acetic ether, I made a mixture of 50 grammes of the former at 56 degrees, the thermometer at 0, (32° Fahr.), and a mixture of 200 grammes of the second, at 25 degrees. The two ethers, thus mixed, yielded, after two days, 30 degrees.

I distilled about 70 grammes of ether; it marked 39 degrees, and had a very decided smell of sulphuric ether;

 whence

whence I concluded, that the method I employed was the only one for separating these two ethers.

I afterwards submitted the acetic ethers to the following examination:

1. Both of them have an agreeable smell.

2. Their specific weight does not differ more than four or five degrees.

3. They boil at almost equal temperatures; the former at 50° of Reaumur, (122° of Fahr.), the latter at 46° (114·8 Fahr.).

4. Exposed to the air they slowly evaporate.

5. Both of them are equally soluble in eight parts and a half of water.

6. The sulphuric acid has but very little action upon these ethers: it colours them slightly: one part of ether and one part of acid mixed very exactly, disengage some heat, about 30° (86° Fahr.).

7. Nitric acid, at 46 degrees, is strongly decomposed by these ethers: there is a considerable liberation of nitrous gas: the residue is oxalic acid.

It results from these different facts, that these two ethers are nearly the same; that they only differ in some slender shades, which do not change their intimate nature.

Thus the process proposed by M. Parmentier is preferable to that of M. Pelletier, in so far as it is less expensive, shorter, and the product more abundant. But, in giving the preference to this last process, I am far from subscribing to the assertion of M. Gehlen, who admits the necessity of a mineral acid for the formation of acetic ether.

XVII. *Proceedings of Learned Societies.*

SOCIETY OF ARTS AND SCIENCES AT UTRECHT.

This society has offered a gold medal, or thirty ducats in money, for the best answer to the following question:

1. "As the two hemispheres of our earth, the north and south, are in some measure different from each other in shape

shape and size, what astronomical appearances should this give rise to? And cannot the existing difference between the summer and winter observations upon the obliquity of the ecliptic be accounted for from these?"

The above society has also offered a double medal, or sixty ducats in money, for the best answer to the following question:

2. "As the latest observations and inquiries upon electricity, the electrical eel, (*Gymnotus electricus*,) and the galvanic powers of other similar fishes, seem to demonstrate a very strong coincidence in the nature of the various electrical powers of these animals, and yet a most remarkable difference in their effects; it becomes requisite to reconcile these apparent contradictions from actual experiments."

The answers to the above questions must be transmitted to Utrecht on or before the 1st October 1808.

THE FREE SOCIETY FOR PHYSICAL AND MEDICAL KNOWLEDGE AT LUTTICK,

On the 6th of June 1806, announced a prize of 200 francs for the best answer to the following question:

"What influence have the passions in bringing on diseases?"

The answers must be sent in on or before the 1st of April 1807.

LITERARY SOCIETY AT MONTAUBAN.

This society has offered a prize of a gold medal for the best answer to the following question:

"What connection subsists between electricity, magnetism, and galvanism? and what part does caloric act in all the operations of the above principles?"

The answer must be transmitted on or before the 20th of March 1807.

ROYAL ACADEMY OF BERLIN.

On the 7th of August, 1806, this academy had its grand annual meeting, when the following prize questions were announced for the ensuing year:

"Has electricity, or have any other pure chemical powers

any

any influence on the greater or lesser strength of magnetism? and if this influence is proved by actual experiments, what are the modifications which the magnetic power thereby undergoes?"

"What were the frontier lines of the Roman possessions in old Germany? and what were the different periods when these possessions were acquired or renounced?"

AGRICULTURAL SOCIETY OF AMSTERDAM.

At the sitting of this society on the 6th of May 1806, the following prize question was announced:

"As the price of wax and honey has risen much within these late years, and as the propagation of bees is by no means increased, but rather diminished; what is the best method of increasing the propagation of bees in Holland, and of removing the obstacles that may exist to their increase?"

The prize is a gold medal of the value of 50 ducats, or a silver medal along with 50 ducats ready money. The answers may be written in Dutch, Latin, French, English, or German; and must be transmitted to the society on or before the 31st of January 1807.

XVIII. *Intelligence and Miscellaneous Articles.*

TRAVELLERS.

WE are happy to announce the arrival in London of lord viscount Valentia, in good health, on his return from India, by the way of Suez, with his secretary, Mr. Salt, after nearly five years absence from England in various parts of India. His lordship made several months stay in the Red Sea, and the adjacent sea coasts; and has made some valuable charts of those different places. His lordship had, by order of the government of India, the Panther (company's cruiser), captain Court, to assist him. Mr. Salt, his lordship's secretary, made an excursion into Abyssinia with major Aundle, of the honourable company's service, as far the capital, Gondar. Lord Valentia's state of health, at this period, would

would not permit him to accompany Mr. Salt. The public will be exceedingly gratified by much valuable information, collected by his lordship during this long, laborious, and laudable research. A young Abyssinian prince is in his lordship's suite, who is a near relation of Negade Ra Mahomet, one of the principal officers of state, so often mentioned by that celebrated traveller the Abyssinian Bruce, as being his friend. This young prince appears possessed of great natural endowments, and anxious to become acquainted with the manners and customs of Great Britain.

We are happy once more to contradict the report of the death of Mungo Park, who was said to have fallen a victim in the interior of Africa. Accounts were received of him about the middle of the present month, which stated his arrival at Tombuctoo, and that he is now on his return. It is to be feared that he will encounter great difficulties in his journey, as he has to tow his boat all the way back against the stream. The journal that is sent to Europe is dated from Sansapang.

DESTRUCTION OF VERMIN.

The following methods are practised in Germany for freeing granaries from mites or weevils:

1. Cover completely the walls and rafters, above and below, of the granaries which are infested by weevils, with quick-lime slaked in water in which trefoil, wormwood, and hyssop have been boiled. This composition ought to be applied as hot as possible.

2. A very sagacious farmer has succeeded in destroying weevils by a very easy process. In the month of June, when his granaries were all empty, he collected great quantities of the largest-sized ants in sacks, and then scattered them about the places infested with the weevils. The ants immediately fell upon and devoured every one of them; nor have any weevils since that time been seen on his premises.

3. Another method, not less efficacious, but which requires a great deal of care and attention in the application of it, is the following:—Place in your granaries a number of

of chafing-dishes filled with lighted pieces of wood. Every aperture must then be carefully closed, in order to prevent any fresh air from entering. The carbonic acid gas, produced from the burning wood, proves fatal to the insects. Rats and mice, also, are so strongly affected by it, that they are seen running out of their holes, and dying in all directions. The persons employed to manage this process must take great care of their own safety, by keeping a current of air around them until the burning wood is properly placed. Another danger may arise from the premises taking fire; but this also may be avoided by proper caution, particularly if they are paved with brick or stone.

EDUCATION OF THE BLIND.

Professor Haüy, of Paris, has introduced into Prussia his plan for educating the blind, and an institution for the encouragement of this laudable object is in great forwardness at Konigsberg.

VOYAGE TO ICELAND.

M. Leopold de Buch, member of the Academy of Sciences at Berlin, and the friend of baron Humboldt, has set out on a voyage to Iceland at his own expense, where he intends to pass the winter for the purpose of making physical and geognostic researches in that hitherto neglected country.

ANTISEPTIC PROPERTIES OF CHARCOAL.

The crews of the two Russian ships which lately sailed round the world were extremely healthy. During the whole three years of their voyage only two men died of the crew of the Neva, and the Naveshda did not lose a single man. It is already known that their fresh water was preserved in charred casks, but it is not so generally known that they used the same precaution for preserving their salted provisions. The beef they carried out with them tasted as pleasantly upon their return as it did three years before, when first salted.

MISCELLANEOUS.

The celebrated Von Mechel, from Basle, but who is at present in Berlin, is occupied in company with Messrs. Humboldt and De Buch, the travellers, Tralles the mathematician, and Bode

Bode the astronomer, in preparing a grand work for publication. It is to be a large copper-plate, which will exhibit a general picture of the highest mountains of the globe, under the title of "*Tableau general des plus hauts Montagnes du Globe.*" It will contain about 150 mountains, with an exact measurement of their several heights above the level of the sea. These heights are reckoned by the fathom of six feet. The drawing is by M. Von Mechel, and the explanatory text is from the pen of baron Humboldt. America is the most eminent for the height of its mountains of all the other quarters of the world.

Several literary works in the modern Greek language are continually issuing from the press in various parts of Germany. Among these is a History of Walachia and Moldavia; and a translation into modern Greek of Goldsmith's History of Greece, accompanied by a map of antient Greece.

Dr. Hager, of Paris, has been appointed by the vice-king of Italy, professor of the oriental languages in the university of Pavia, the first school of learning in Italy. His appointment was accompauied with a flattering letter from his majesty.

Dr. Bozzini, of Frankfort, has invented an instrument which must be very interesting to the medical profession. The object of this instrument, to which the name of *light-spreader* has been given, is to afford an inspection of the interior of wounds, or the various parts of the human body, such as the œsophagus, the vagina, the uterus, &c. The inventor is preparing for the press drawings and descriptions of this curious instrument, which has already attracted considerable notice on the continent.

M. Lebrun, first trumpeter in the king of Prussia's band at Berlin, having long reflected on the deleterious consequences arising from the oxides of copper being collected in the inside of brass trumpets, and thus inhaled into the lungs of the performer, has invented a method to avoid any bad effects from this circumstance. He coats the inside of his trumpets with a lac, which unites to smoothness tenuity; and does no injury whatever to the sound of the instrument. Nitric acid poured into these trumpets does not change the

colour;

colour; which proves that no place of the copper is uncovered. M. Humboldt has examined this improvement, and has declared himself perfectly satisfied as to its superiority in point of wholesomeness and fineness of tone.

DEATHS.

On the 23d of June, 1806, died at Broissy, near Versailles, in the 83d year of his age, Mathurin Jacques Brisson, member of the French national institute, and author of several useful elementary books in chemistry and natural philosophy.

On the 10th September, at Dresden, in the 75th year of his age, John Christopher Adelung, author of the celebrated dictionary of the German language.

LIST OF PATENTS FOR NEW INVENTIONS.

To Henry Pratt, of Birmingham, in the county of Warwick, steel toy-maker; for a new toast-stand, or an improvement on the article called cats or dogs, upon which things are placed before a fire. Dated October 2.

To Robert Salmon, of Woburn, in the county of Bedford, surveyor; for a mathematic-principled safe and easy truss for the relief and cure of ruptures. Dated October 2.

To William Cooke, of Chute House, in the county of Wilts, gent.; for certain improvements in the construction of waggons, and other carriages which have more than two wheels. Dated October 2.

To Ralph Wedgwood, of Charles-street, Hampstead-road, in the county of Middlesex, gent.; for an apparatus for producing duplicates of writings. Dated October 7.

To Ralph Sutton, of Macclesfield, in the county of Chester, brazier and tin-plate worker; for certain improvements in an apparatus for cooking either by steam or water. Dated October 7.

To William Sampson, of Liverpool, in the county of Lancaster, millwright; for an invention to be acted by the impulse of wind, in order to work mills, pumps, and other machinery, suitable to its application. Dated October 7.

To Archibald Jones and James Jones, of Mile End, in the county of Middlesex, printers; for a method of discharging colours from shawls, and other dyed silks, and silk and worsted

worsted of every description, or such part or parts thereof as may be required for the purpose of introducing by printing or staining various patterns on such discharges, or otherwise. Dated October 7.

To William Clegg Gover, of Rotherhithe, in the county of Surrey, carpenter; for an improved wheel or purchase, for the steering of ships, by means of which wheel or purchase a considerable degree of labour is saved, and a ship may be steered with more ease, and greater steadiness and certainty, and with more safety to the steersman. Dated October 15.

To Joseph Bramah, of Pimlico, in the county of Middlesex, engineer; for a machine whereby valuable improvements in the art of printing will be obtained. Dated October 15.

To John Fletcher, of Cecil-street, in the Strand, in the county of Middlesex, esq.; for a composition for agricultural purposes, which is not only of the greatest value as a manure, but is also extremely efficacious in the destruction of the fly in turnips; snails, slugs, ants, and the majority of those other insects which are detrimental to vegetables; which composition he usually denominates "Prepared Gypsum." Dated October 21.

To Elihu White, of Threadneedle-street, in the city of London, gent. in consequence of a communication made to him by a certain foreigner residing abroad; for a method of making a machine, for casting or founding types, letters, spaces, and quadrates, usually made use of in printing. Dated October 23.

To John Prosser, of Back Hill, Hatton Garden, in the county of Middlesex, smith; for various improvements upon smoke- or air-jacks, which may be applied to those now in use. Dated October 30.

To James Capam, of Leicester, in the county of Leicester, brazier; for a machine for discharging smoke from *smoking* chimneys. Dated October 30.

To Isaac Sanford, of the city of Gloucester, civil engineer, and Stephen Price, of Stroud, in the county of Gloucester, civil engineer; for a new improvement or method to raise a nap or pile on woollen, cotton, and all other cloth which may require a nap or pile, as a substitute for teasels. Dated October 30.

METEORO-

METEOROLOGICAL TABLE,
BY MR. CAREY, OF THE STRAND,
For October 1806.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Sept. 27	56°	64°	54°	30·20	36	Fair
28	55	63	58	·12	26	Fair
29	59	58	55	29·82	0	Rain
30	50	63	50	·96	36	Fair
Oct. 1	44	59	51	30·02	27	Fair
2	46	60	55	29·98	30	Fair
3	55	60	56	·61	6	Rain
4	56	64	55	·76	14	Showery
5	56	60	56	30·02	25	Fair
6	57	64	51	·18	42	Fair
7	46	59	55	·34	15	Cloudy
8	55	59	54	·28	47	Cloudy
9	55	62	54	·20	36	Cloudy
10	54	59	53	·10	25	Cloudy
11	55	60	54	·05	35	Fair
12	52	58	46	·02	29	Fair
13	45	59	53	29·85	25	Fair
14	55	62	54	·70	21	Showery
15	51	59	55	·76	20	Fair
16	53	55	50	·74	18	Cloudy
17	49	54	47	·74	24	Fair
18	42	54	43	·50	33	Fair
19	47	56	42	·56	27	Fair
20	42	52	50	·60	10	Showery
21	51	55	47	·10	0	Rain
22	50	51	38	28·75	0	Rain
23	37	45	35	29·87	35	Fair
24	36	49	36	30·12	36	Fair
25	40	50	50	·10	20	Cloudy
26	51	62	51	·01	25	Fair

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

XIX. *Memoir on the Saccharine Diabetes.* By Messrs. Dupuytren *and* Thenard *.

It has been long known that the human urine is so strangely altered in the disease called *diabetes*, that in place of being in a small quantity and acrid, like that of a person in health, it becomes, on the contrary, saccharine and very abundant. It is only about thirty years, however, since the first analyses were made of diabetic urine. Three causes retarded this investigation: on the one hand, the infrequency of the disease; on the other, the uncertainty of the chemical apparatus formerly employed; and, thirdly, the neglect which animal chemistry has suffered until within these very few years.

It was not sooner than 1778 that the existence of sugar in diabetic urine was demonstrated. This discovery, for which we are indebted to Cauley, was established in 1791 by Franck. It was, indeed, hinted at by Willis, at the beginning of the 17th century; and afterwards, in 1775, by Pool and Dobson. M. Cauley, however, having only directed his attention to the saccharine substance alone, left a great deal undone. It was necessary to ascertain the other principles of diabetic urine, and particularly those which enter into common urine; and this was accomplished in 1803 by Messrs. Nicholas and Guendeville of Caen. It results from their researches that the urine of diabetic patients does not contain a sensible quantity of *urea*, or of uric acid; that the most sensible reagents scarcely indicate traces of phosphate and sulphate; that it is impossible to discover a free acid in it; and, in short, that nothing is found except sugar in great quantity, and more or less marine salt.

In the present memoir we purpose not only to confirm the results we have cited, but in particular we shall communicate,

1st, The medical observations made by us upon the diabetic patient whose urine we analysed.

* From *Annales de Chimie*, tom. lix. p. 41.

2d, The very peculiar nature of the saccharine substance we found in that urine.

3d, The different transformations which it underwent before being brought back to its natural appearance and consistency.

PART FIRST.

Medical Observations upon the Case in which we analysed the Diabetic Urine.

It resulted from these observations:

1st, That the saccharine diabetes may last several years, and even as long as the digestive faculties continue, and are able to administer to the excessive losses which take place through the urine.

2d, That this disease is not incurable in any of its stages, not even when an altered digestion seems no longer to furnish matters of secretion and the animal œconomy is exhausted.

3d, That the seat of this affection seems to be in the kidneys and not in the intestinal canal.

In fact, the appetite and thirst of diabetic patients are not perverted; they appear merely to be in proportion to the want of organical repair, as well as the digestive faculties; in the second place, the alimentary substances undergo the same operation in the stomach of a diabetic patient as in that of a healthy person; and what proves that the digestion is not altered, but merely increased, in diabetes is, the quantity of food taken, the rapidity with which it is digested, the great proportion of matter which is absorbed from that food, and the small quantity of fæces to which it is reduced: in short, we find no saccharine liquid, or any liquid which has undergone any alteration in its composition, all the way from the organs of digestion to those of the urinary secretion.

4th, That the cause of saccharine diabetes appears to be a heightened and perverted action of the kidneys.

That in consequence of this action the saccharine matter of urine is produced, and to this cause we may ascribe all the symptoms of this malady.

5th, That the excessive losses which take place in this disease seem to ascertain, in some circumstances, a very considerable absorption at the surface of the body in diabetes.

6th, That the new relations determined by saccharine diabetes between the aliments and the secretions in general, and between each of their species in particular, are analogous to those which are determined by an excessive evacuation, of whatever kind it may be.

7th, That the treatment prescribed by Rollo, and afterwards employed with so much success by our countrymen Messrs. Nicholas and Guendeville, and which consists in a regimen purely animal, has the same degree of efficacy in diabetes as Jesuits' bark has in intermittent fevers.

8th, Finally, that the saccharine diabetes produces no other change in the state of our organs than a development of the digestive and urinary apparatus; both of which are highly active during this disease, the one in preparing and the other in discharging the alimentary substances.

Part Second.

Analysis of the Urine evacuated by a Diabetic Patient, from the 15th Day of his Admission into the Hotel-Dieu until he was carried into the Infirmary of the School of Medicine of Paris.

The above urine came from the patient in an uncommonly large quantity, and exhaled a smell by no means disagreeable.

It was limpid, yellowish, specifically heavier than water, and scarcely reddened the tincture of turnsole; slightly saccharine, it had at the same time something of the taste of sea salt.

Left to itself at a temperature of 15° (59° Fahr.), it became turbid in five or six days; bubbles of carbonic acid were liberated from it if it was ever so little shaken; the urinous smell it had at first was dispelled; and it contracted a smell analogous to that of newly made wine: it yielded alcohol upon being distilled, and became strongly acidified on exposure

to

to the air; it therefore presented, in a weak degree, all the characters of a spirituous fermentation.

When distilled in a retort, or evaporated in a capsule, the phænomena were the same; it became turbid, thickened by little and little, and was reduced to a syrup sometimes equivalent to a seventeenth, sometimes a twentieth, but never less than a thirtieth of its own weight. We extracted in this manner from the urine we treated, nearly thirty pounds of this syrup, which upon cooling always became one mass, composed of a multitude of small grains without consistency. These soft granulated crystals being hardly sweet, it was natural to think that the substance of which they were formed was not homogeneous, and contained only a very small quantity of the saccharine principle. In order to ascertain it, the following experiments were made:

We took 100 parts of this substance and distilled it in a retort, the neck of which was inserted into a receiver which was continually kept at a low temperature. We obtained plenty of water, a little oil, and no ammonia; a great quantity of gases a little fetid, and abundance of charcoal easily incinerated, yielding, upon complete incineration, two parts and a half of sea salt and one half part of phosphate of lime.

From this result may be drawn the three following consequences:—1st, That this substance contains no animal matter, since when calcined it yields no volatile alkali: 2d, That it contains very little saline matter, because when reduced to ashes it only presents a residue equal to some one hundredth parts of its weight: 3d, That it is formed of vegetable principles alone, because it gives all the products of vegetables upon being distilled.

Presuming that the sugar was one of these principles, and forming no kind of conjecture upon the nature of those with which it was supposed to be mixed, we resolved to employ fermentation to destroy the former, and to keep the latter principles unaltered, in such a manner, that by filtration and evaporation we ought to have obtained them very pure. We mixed in a large flask 100 grammes of the substance to be analysed, 25 grammes of ferment, and 500

 grammes

grammes of water; we adapted to the neck of this flask a tube fixed under a flask full of water; the temperature being then raised to 18 degrees, the experiment was left to itself. Some hours after the contact took place between these substances a motion was evinced in some parts of the liquor, which soon became general: plenty of solid flakes, which gave birth to a great number of gaseous bubbles, were raised to a considerable height; these bubbles rapidly passed into the vessels full of water, but the flakes fell back to the bottom of the flask, and, giving birth to new bubbles, they again ascended to be once more precipitated. This phænomenon, which took place for three days, announced a very active fermentation, and, consequently, the presence of a great quantity of saccharine matter: in fact, more than thirteen pints of pure carbonic acid gas were liberated; the liquor was very alcoholic, and contained nearly 48 parts of alcohol at 40 degrees; evaporated to dryness, only 23 parts of extract were obtained, formed of three parts of sea salt and twenty parts of a viscous brown matter.

We know that 100 grammes of sugar produce 12 grammes of a similar residue, 56 of alcohol, and 36 of carbonic acid. Thus the substance drawn from diabetic urine yielded by fermentation the same products, and almost in as great quantity, as pure and finely crystallized sugar; and if we add, that it acts like sugar with the nitric acid, alcohol, and the other reagents, we must regard these two substances as being in some measure precisely the same.

We ought, however, to recollect that it is scarcely sweet, at least much less so than sugar. It may be thence concluded, 1st, That, as has been long suspected, there are different species or varieties of sugar, the differences being so striking as now to render certain what before was only probable. But as the taste only is not a certain criterion of the saccharine principle, it becomes necessary to examine if, among the substances which have hitherto been confounded with sugar on account of their taste, there are not some of them which differ essentially from that substance.

For this reason we were led to examine manna. Our first care was to mix it with ferment and water at the temperature of 18° (64° Fahr.), and to remark attentively all the phænomena produced by this mixture. Fermentation soon began: it was brisk at first, but it soon decreased; and in two days it ended. The liquor had nevertheless a very strong vinous smell; but far from being alcoholic, it was, on the contrary, very saccharine, and deposited upon evaporation, in the form of crystals, almost all the matter employed, deprived of the fermenting faculty.

Although convinced by these results that manna contained only a very small quantity of sugar, we ought nevertheless to compare it in all its properties with this substance, in order to place the fact beyond a doubt, and thus to discover all the characters peculiar to the particular principle of which it appeared almost entirely formed: we therefore tried its action on spirit of wine, which did not attack the saccharine principle, and upon the nitric acid, which did not convert any portion of that principle into mucous acid.

The former of these reagents, at the temperature of 60° (140° Fahr.), dissolved so great a quantity of manna, that, upon cooling, the liquor was a mass composed of a crowd of crystalline heaps, every crystal in each heap springing from a common centre. The second reagent produced, after a long ebullition, so great a deposit of mucous acid, that it was almost half the weight of the manna employed.

Here, therefore, there are two characters which are strikingly different; sugar properly so called, and the particular principle of manna.

New researches will doubtless present many other differences more or less prominent; but those above related being sufficient to occasion these bodies to be regarded as very distinct from each other, we did not think it necessary to enter more deeply into the subject.

It follows from hence, that it would be always easy to recognize and separate manna, or rather the peculiar principle of manna, whatever may be the substances with which it is mixed. It is only necessary to treat with warm alcohol the matter which contains this peculiar principle of manna, and it will be almost entirely precipitated upon cooling. In truth, there are other vegetable substances which possess

this

this principle even in a remarkable degree; but these substances always hold some acid in their composition, from which they may be freed by combining it with an alkaline or earthy base, or a metallic oxide, according to the nature of the acid; and consequently this mode of separation may be generally employed.

It is in this manner that we may ascertain if the honey-like substance observed upon the leaves of certain trees, and particularly on those of the linden tree, is in reality a kind of manna; and if the saccharine principle which exists in asparagus is of the same description; it having been lately discovered by Messrs. Vauquelin and Robiquet, that in asparagus the saccharine substance is mixed with a principle entirely peculiar*.

Analysis of the Urine evacuated by the Patient from the Time he was brought to the Infirmary of the School of Medicine until he went out cured.

During the whole time the patient was in the Hotel-Dieu he could not be subjected to any particular regimen. He lived as he pleased; his disease therefore remained stationary, and his urine, at all times very abundant, was never changed in its nature. It was then resolved to carry him to the Infirmary of the School of Medicine, where, being almost continually watched by M. Dupuytren and his pupils, it became easier to make him do whatever he was desired.

After a few days all kinds of vegetables were refused, and animal food only was given him. The weights of what he ate, and of the liquids he drank to quench a most dreadful thirst, were ascertained.

For the first two or three days no change was remarked in his urine; but in five or six days it became less white, sharper, more acid, and less saccharine; when submitted to evaporation, in place of continuing limpid as before, it became turbid, and was covered with a thick pellicle of albuminous matter. When I perceived this change, particularly the presence of an animal matter in his urine, although the

* The paper here referred to will be given in a subsequent number.

state of the patient was absolutely unknown to me, and I knew nothing of the treatment which he had undergone, I presumed that the disease began to yield: having afterwards observed this animal matter to become daily more abundant, I regarded the cure of the patient as at no great distance: I communicated my opinion to M. Dupuytren, who seemed surprised at my prediction; this surprise, however, ceased when I informed him what had taken place in the patient's urine.

From this period the patient became better and better. His urine was daily more animalized and less saccharine. The animal albuminous substance gradually diminished also, and the *urea* and uric acid began to appear. His urine then became perfectly similar to that of a person in health. In other words, he was cured; but, having given himself up to various excesses, he soon fell ill again; the diabetes again made its appearance, accompanied with other diseases which had also formerly attended it in the same patient.

If we resume, however, all the consequences which may be drawn from the experiments we have related in the second and third parts of this memoir, we may assert,

1st, That the urine of the diabetic patient above examined was almost entirely composed of a matter only a little saccharine; that nevertheless this matter enjoys all the properties which characterize sugar; because it is transformed into alcohol and carbonic acid by fermentation; it yields plenty of oxalic acid and no mucous acid by the nitric acid; it is very little soluble in alcohol at 36°; it produces when calcined little oil, and plenty of water and carbonic acid; thus it is clearly demonstrated that there are different varieties of sugar.

2d, That manna is not a species of sugar; that it only contains a small quantity, which may be destroyed by fermentation; that it contains, on the contrary, plenty of a peculiar principle, the taste of which is very sweet, and the character of it is not to ferment with yeast; it gives plenty of mucous acid with the nitric acid, is more soluble in warm than in cold water; but, above all, it is soluble in alcohol to such a degree, that, upon cooling, the solution becomes a crystalline mass.

3d, That

3d, That upon giving nothing but animal food to diabetic patients, their urine speedily changes; that at first we find an albuminous matter in it; that this albuminous matter, which always increases for some days, is an unequivocal sign of a speedy cure; that afterwards this albumen gradually disappears; that the kidneys then begin to secrete the substance called *urea*, uric acid, and also the acetous acid: the urine soon becomes like that of an individual in health.

Notwithstanding all this, however, the patient, in order to prevent a relapse, ought to observe the animal regimen for a long time, and avoid every thing which may tend to cause the reappearance of diabetes.

XX. *Memoir upon Animal Fat and some Medicinal Preparations which are administered through that Medium. By M.* VOGEL.

[Concluded from p. 81.]

Fat and the Metals.

It has been ascertained that fat acts more or less upon a great number of the metals. Copper, for instance, gives a green colour to fat, when the air also is allowed to act upon it. The solution of the oxide of copper in fat is a fact proved by daily experience.

As mercury, however, is the metal which excites most interest in the pharmaceutic art in its combinations with fat, I shall restrict myself particularly to discussing the effects of these combinations.

Several apothecaries have endeavoured to improve the quality of mercurial ointments, and particularly of the double ointment. M. Vean-Delaunay proposed rancid oil, and M. Fourcroy has shown that oxygenated fat is most proper for killing mercury.

Of late, several chemists have imagined that the mercury of the double mercurial ointment is not oxidated, but that it exists in the metallic state.

I know

I know of no experiment which supports this idea. The following were my own experiments on the subject:

I triturated equal quantities of mercury and fat in a mortar, the weight of which I had previously ascertained exactly; when the mercury was entirely absorbed, I weighed the mortar with the ointment, and I found no increase: this made me suspect, that if the mercury was oxidated, it was not by the oxygen of the atmosphere, but by means of the fat.

Being desirous of ascertaining the state of the mercury, I introduced some newly prepared ointment into a glass cylinder hermetically sealed at one of its extremities; I plunged it horizontally*, for three hours, into boiling water. After it cooled there were two very distinct layers formed; the upper one was perfectly white like fat, and I separated the under layer by cutting the cylinder with a file. Then, upon slightly mixing the mercury with boiling water, there was collected three drachms and eight grains of mercury quite liquid. The remainder of the mercury, which obstinately retained a little fat, was treated with a ley of caustic potash. The soap which resulted from it being collected, was redissolved in rectified alcohol; and by this method I obtained the whole of the mercury employed.

I also separated fat from mercury by boiling the double ointment with water; the fat floated above, and it was slightly coloured in consequence of a little of the mercury which strongly adhered to it. I obtained at the bottom of the vessel metallic mercury mixed with a little fat. The least agitation brought the globules together.

I also treated the ointment directly by the muriatic acid. Wishing to ascertain if the muriatic acid could take the oxygen from oxidated mercury, and if it passed to the state of oxymuriatic acid, I made the experiment in close vessels with the chemical pneumatic apparatus, but I had no liberation of oxymuriatic acid gas; besides, it is difficult to believe that the oxide of mercury at the *minimum*, containing

* We suspect an error here in the French, and that we ought to read *vertically.*—EDIT.

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so little oxygen, yields any part of it to the muriatic acid, and makes it pass to the state of oxymuriatic acid.

I made the comparison with different ointments, which had been prepared three months, eight months, and several years; in the latter compounds I found a little oxidated mercury, but the greatest part was always in the metallic state.

I also triturated mercury with Venice turpentine; and it was absorbed with great facility. To ascertain if the mercury obsorbed the oxygen from the air, or if it got any oxygen from the turpentine, I dissolved the mass in alcohol at 40°; all the turpentine was dissolved, and the mercury was precipitated in small globules; the alcoholic solution was afterwards evaporated: I then obtained the turpentine with all its properties.

It results, therefore, that in the ointments above mentioned the mercury is not in the state of an oxide, as generally supposed, but that it is in the metallic state and in very minute division, particularly in newly prepared ointment. I am of opinion that mercury is in a similar state in several mercurial compounds more or less in use, as in the *vigo-mercurial* plaster, ethiops saccharatus, ethiops alkalisatus, or mercurius gummosus Plenkii, &c. of the shops.

It may be objected that the colour of the ointment and of the above preparations speaks very much for the oxidation of the mercury; but let glittering antimony, bismuth, or any other metal susceptible of pulverization be reduced to a fine powder, and it will be seen that these substances are of a blackish gray when in minute division.

It remains for me to speak of the action of fat upon the metallic salts. I shall confine myself more particularly to the nitrate of mercury, from which a compound results very much used in pharmacy.

I prepared the citrine mercurial ointment according to the process described in Baumé's Elements of Pharmacy, by dissolving three ounces of mercury in four ounces of nitric acid for two pounds of fat.

As the surface of this ointment always becomes white after some time, the cause of which is only explained by asserting

asserting that it arises from the absorption of the oxygen of the atmosphere, I was anxious to ascertain if this was correct.

After having poured the ointment, while yet liquid, into squares of paper, I placed one part of it under a bell-glass filled with air over mercury: at the end of 24 hours no absorption had taken place; yet the surface was uncommonly white.

I placed another part of it under the receiver of an air-pump, and I suddenly made a vacuum, which I kept up for some hours, giving from time to time a stroke of the piston, which produced at first some bubbles of air. The ointment extracted from the vacuum was perfectly yellow, and remained always so, without the least change.

I think, therefore, that this white crust is owing to the disengagement of the gases, whether azotic or nitrous, which takes place from every part of the interior towards the surface; and they augment the volume of the ointment. The latter cools by degrees, and does not allow the gases to escape entirely; one part of the gas remains, and forms an infinity of small white bubbles at the exterior part.

In support of what I have here advanced it may be added, that when the ointment is left to cool in the vessel in which it had been melted, and particularly when it is heated a little more, the quantity of caloric given is sufficient to drive off all the gas, and the ointment remains constantly yellow, without experiencing any further alterations.

In order to examine this compound, and to appreciate the chemical changes which might have taken place, I boiled an ointment which had been two years prepared, for half an hour with water. It became very clotted; the water was interposed in such a manner that it was difficult to separate the whole quantity from it. The water had acquired a yellowish colour from it, and a slightly bitter taste; it was scarcely acid at all, and did not contain an atom of mercury.

In order to establish a comparison, I made use of fresh ointment, made within 24 hours; I exhausted it by warm water, which had almost the same characters as the wash-

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ings of old ointment, and scarcely contained any traces of mercury, indicated by a hydrosulphuret.

According to these data it would be natural to think that the acid nitrate of mercury had undergone a change, and one would think that it had passed to the state of a yellow nitrate or nitrous turbith, which is little soluble in water.

I kept ointment a long time in fusion, but I could not separate the nitrous turbith from it; the latter, therefore, is not merely disseminated in the ointment, but must be intimately united with and dissolved in it. I convinced myself of the possibility of the solution of turbith in oxygenated fat, by heating these two matters together. I decanted the liquid clear; it perfectly imitated the citrine ointment, and contained a very great quantity of mercury.

As to the use of this production, the effects of which, some physicians allege, are analogous to pomatum simply oxygenated by the nitric acid, I do not allow myself to give any opinion on the subject; it is probable, however, that a substance which holds mercury in true combination will produce different effects from one which does not contain any at all.

In place of the acid nitrate employed above, I took neutral nitrate at the *minimum*.

When reduced to fine powder, I projected it upon heated fat: bubbles were immediately produced, and the white powder of the nitrate was soon converted into yellow powder.

The fat acquired a solid consistency; it contained mercury in solution.

The neutral nitrate is therefore decomposed by hogs' lard. It is not that the mercury here yields its oxygen because it is already in it at the *minimum*; but it is the nitric acid which abandons in part the oxidated mercury and acts upon the fat where it is decomposed; from which results the yellow nitrate of mercury, which contains little nitric acid *.

I examined several other metallic salts with fat, such as the

* Nitrous turbith contains, according to the Portuguese chemists Braamcomp and Oliva, 12 per cent. of nitric acid. (See their analytical treatment of the mercurial substances by the phosphorous acid in the *Annales de Chimie*, No. 161.)

the nitrates of silver and lead, the muriates of platina and hyperoxygenated mercury, and I found that there was very little decomposition, and that they produced upon fat similar effects with the nitrates of mercury.

It results from the facts announced in this memoir:

1st, That light without the contact of the air makes fat become yellow, gives it a sharp rancid smell and taste without acidifying.

2d, That fat yields no ammonia upon distillation, and that it contains no azote; we may regard it therefore as a purely vegetable substance.

3d, That in sulphurated pomatum there is a part of the sulphur in solution, and that, either dissolved or mixed, it does not pass to the acid state.

4th, That phosphorus is dissolved in it, but it speedily passes to the state of phosphorous acid; and this acidification increases by the contact of the air.

5th, That fat oxygenated by long contact with the air constantly becomes acid. Its washing precipitates some metallic solutions; at the end of the distillation of this washing, acetic acid passes into the recipient.

6th, That the nitric acid forms with fat a bitter yellow matter, acetic acid, and an acid susceptible of crystallizing, which could not be entirely separated from it by simple washing. This solid acid is mucous or saccholactic acid, which is also obtained with suet by the nitric acid.

7th, That the oxymuriatic acid is decomposed with fat, but the latter remains whitish, and becomes very soft. The bitter yellow matter is not formed, nor can it even be produced by afterwards treating it with nitric acid.

8th, That mercury is in the metallic state, and in very minute division in mercurial ointments recently prepared.

9th, Lastly, that in the citrine ointment mercury is in the state of nitrate oxidated at the *minimum*. The white crust which is formed is owing to a simple disengagement of the gases, which could not entirely escape, and which fill the surface with small bubbles. The nitrate of neutral mercury at the *minimum* is decomposed in fat.

XXI. *Me-*

XXI. *Memoir upon the Acetic Acid.* *By M.* TROMSDORFF.*

THIS memoir has for its object to ascertain if, as M. Proust has asserted, azote forms any part of the acetic acid.

The author, after having considered what is at present known on the composition of ammonia and the vegetable acids, was much surprised to find in M. Proust's memoir that this chemist had found ammonia and prussic acid in the composition of acetates. The author of the present memoir, notwithstanding the exactitude and sagacity of the chemist of Madrid, was anxious to ascertain the existence of azote in the concentrated acid from his own experiments, and he was directed to the research by the love of truth and the importance of the fact.

Before giving the processes employed by M. Tromsdorff to attain the object he proposed to himself, we shall give a summary of the objections which presented themselves to M. Proust's theory.

If, says M. Tromsdorff, by the distillation of acetates ammonia is formed, it is evident that azote is contained therein: But where could this principle come from? Could it be from the alkaline base? But that is not the case; for, according to M. Proust, the ammonia has also been extracted from the acetate of lead. In this case it can only be furnished by the acetic acid; or it must be said that azote is only hydrogen modified.

The author afterwards examines if among the vegetable acids azote is met with as frequently as in the animal acids; because, if that was the case, the classification of it must fall to be changed. In order to repeat the experiments of M. Proust, it appeared essential to the author to employ only the purest substances.

Thus, in order to obtain the acetic acid in the greatest degree of purity possible, he decomposed the acetate of potash by the sulphuric acid; he afterwards saturated this acid by carbonate of soda finely purified, and evaporated in a silver

* Extracted from the *Journal de Berlin.*

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bason the saline solution which resulted from it. The salt obtained, which he closed up in a flask shut with a ground stopper, was of a very fine white colour.

He used the same precautions in the preparations of the acetates of potash and lead.

He took eight ounces of each of these salts, which he introduced separately into strong glass retorts; the retorts were placed upon an open fire in a furnace; he adapted to each a bell-glass, from which a glass tube issued opening into a bell-glass proper for receiving the gases.

The products were always (as all the world knows) an ethereal acidulated fluid mixed with oil. The alkali and the carbon remained in the retort, and in the decomposition of the acetate of lead nothing of this salt remained except oxidated lead.

Thus, by the predisposing affinity of the alkalis for carbonic acid, the former determined the decomposition of the acetic acid, in order to afford room for the formation of the carbonic acid. The oxide of lead, on the contrary, not having so much affinity for carbonic acid, abandoned the acetic acid in its greatest purity.

The author asks if the metallic base abandons oxygen to burn charcoal, or if the power of attraction is less powerful between an oxide and an acid than between an acid and an alkali.

He has made a great number of experiments in order to answer this question.

The gases examined exhaled no ammoniacal smell; their smell was only empyreumatic and penetrating. The liquids had the same smell, and all the chemical reagents did not ascertain any ammonia there.

The residues which, according to M. Proust, contain some prussiate, were only pure alkaline carbonate or pure oxide of lead.

M. Proust, on examining the residue of the acetate of potash, says that it was a residue formed partly of prussiate and partly of carbonate of potash. M. Tromsdorff expected to find these two salts; but upon breaking the retort he only found

found a homogeneous charcoal, which neither yielded him ammonia nor prussic acid, and which had not the smell of any of these substances. Yet we know well how easy it is to recognise the smell of the latter acid wherever it exists in a state of liberty.

M. Proust adds, that the residue of the acetate of potash was so saturated with prussic acid, that it had a bitterness as if the alkali had been directly combined with this acid; whence M. Tromsdorff concludes that the above author had made use of common vinegar.

In order to ascertain the products resulting from the decomposition of the acetic acid, M. Tromsdorff passed the vapours through a tube made red hot; this operation gave him nothing but carbonic acid gas, carbonated hydrogen gas, and a small quantity of empyreumatic liquid, without ammonia and without prussic acid; these substances no longer exist in the residues.

M. Tromsdorff afterwards attentively examined the ethereal acidulous fluids mixed with oil. He distilled them over carbonate of potash, and obtained an ether which appeared to be true acetic ether in every respect.

The results of this memoir are:

1st, That the presence of azote in acetic acid is by no means demonstrated.

2d, That the pure acetates, when distilled, yield neither ammonia nor prussic acid.

3d, That the pure acetic acid is changed a little in its nature by passing through red hot tubes.

4th, That in an iron tube it is completely decomposed into carbonic acid gas and carbonated hydrogen gas.

5th, That it is demonstrated that the constituent parts of acetic acid are, oxygen, carbon, and hydrogen.

6th, That the ethereal fluid is, by its properties in general, similar to the other ethers; the author regards it as a medium between alcohol and ether. And as the acetic acid partly changes it into ether, and the latter, when treated by the nitric acid, is transformed into oxalic acid, the transition of the acetic acid into oxalic acid is thus demonstrated,

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indirectly it is true; and lastly, that M. Proust probably did not make use of pure acetates in his labours, or that the ethereal pungent empyreumatic smell made him conjecture that there was ammonia present.

XXII. *Account of a Discovery of Native Minium. In a Letter from* James Smithson, *Esq. F. R. S., to the Right Honourable Sir* Joseph Banks, *K. B. P. R. S.**

My dear Sir,

I beg leave to acquaint you with a discovery which I have lately made, as it adds a new, and perhaps it may be thought an interesting species, to the ores of lead. I have found *minium* native in the earth.

It is disseminated in small quantity in the substance of a compact carbonate of zinc.

Its appearance in general is that of a matter in a pulverulent state, but in places it shows to a lens a flaky and crystalline texture.

Its colour is like that of a factitious minium, a vivid red with a cast of yellow.

Gently heated at the blowpipe it assumes a darker colour, but on cooling it returns to its original red. At a stronger heat it melts to litharge. On the charcoal it reduces to lead.

In dilute white acid of nitre it becomes of a coffee colour. On the addition of a little sugar, this brown calx dissolves, and produces a colourless solution.

By putting it into marine acid with a little leaf gold, the gold is soon entirely dissolved.

When it is inclosed in a small bottle with marine acid, and a little bit of paper tinged by turnsole is fixed to the cork, the paper in a short time entirely loses its blue colour, and becomes white. A strip of common blue paper, whose colouring matter is indigo, placed in the same situation, undergoes the same change.

The very small quantity which I possess of this ore, and the

* From the *Transactions of the Royal Society* for 1806.

the manner in which it is scattered amongst another substance, and blended with it, have not allowed of more qualities being determined, but I apprehend these to be sufficient to establish its nature.

This native minium seems to be produced by the decay of a galena, which I suspect to be itself a secondary production from the metallization of white carbonate of lead by hepatic gas. This is particularly evident in a specimen of this ore, which I mean to send to Mr. Greville as soon as I can find an opportunity. In one part of it there is a cluster of large crystals. Having broken one of these, it proved to be converted into minium to a considerable thickness, while its centre is still galena.

I am, &c.

Cassel in Hesse,
March 2, 1806.

JAMES SMITHSON.

XXIII. *Analytical Essay on Asparagus. By M.* ROBIQUET *junior, Apothecary at Vale de Grace.*

[Concluded from p. 38.]

AFTER what has been stated, we cannot conclude, with M. Antoine, that the acid obtained from asparagus by the process pointed out is malic acid. The difference is very remarkable. The latter forms with barytes, strontian, and lime, salts a little soluble; while those of the former are very insoluble. We may be convinced of this by taking equal proportions of these two acids, supposed to be of the same degree of concentration, and afterwards diluted with a great quantity of water. If we pour into these two liquors, drop by drop, a solution of lime for example, a precipitate shows itself with the acid obtained from asparagus; but the malic acid does not give any trace of one, provided always that the liquors be sufficiently diluted. The same thing takes place with barytes or strontian. A character still more marked is that of decomposing the sulphate of iron, the acetates of iron and of copper, which the malic acid does not. Besides, as I have formerly recalled to the reader's recollection, the malate of lead is soluble in vinegar; but

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the precipitate formed by the other acid in the acetate of lead refuses altogether to dissolve itself therein, whatever quantity be employed. The malic acid, besides, does not decompose earthy acetates.

The malate of magnesia is so very deliquescent that it has been proposed by M. Chenevix as the means of separating alumen from magnesia, by treating those two earths with malic acid and dissolving them in alcohol; and since the acid of asparagus, when saturated with an alkali, precipitates abundantly the muriate of magnesia, if, as I have said, the solutions are concentrated, we may infer that this salt is not soluble in spirit of wine.

It is equally impossible to confound this acid with vinegar, since it decomposes almost all the earthy and metallic acetates.

Its property of not crystallizing prevented me from comparing it with other vegetable acids, and I could not, with any better appearance of probability, regard it as a mineral acid, since when submitted to the action of heat it became charred. I was therefore naturally led to believe it a new acid: but having communicated my experiments to M. Vauquelin, he advised me to examine carefully its combinations with earthy bases, and from this examination resulted, as we shall see, the discovery of its nature.

I precipitated it by lime water, and after filtering and washing it well, I calcined it in a silver crucible. On the first action of the heat this salt blackened, and gave out at the same time a slight ammoniacal odour. I continued this process until there no longer remained a trace of carbonaceous matter: the residue was insipid, and insoluble in water. I boiled it with distilled vinegar. I filtered and saturated it with ammonia; an abundant white flaky precipitate was formed, presenting the characters of phosphate of lime. To assure myself that the acid of asparagus was phosphoric acid, I treated with the blowpipe some grains of its combination with lead. It presented at first the same phænomena as the calcareous salt; it passed immediately to yellow, entered into fusion, and the button, on cooling, assumed the form of an irregular polyhedron, a property exclusively belonging

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to the phosphate of lead. It is thus demonstrated, that if this acid had presented, in its principle, differences from phosphoric acid, it must have happened from its holding in solution an animal matter, and which I believe to be the same with that which this matter contains in the green feculence: at least, I dissolved a portion of the latter in pure phosphoric acid, I evaporated it so as to burn the animal substance, and there was developed an odour very similar to that which the phosphoric acid of asparagus yields.

In was in that portion of the extract that was insoluble in alcohol that I ought to have found, according to M. Antoine, this animal matter, which he has recognised for gelatine. Distilled water, as I have shown, entirely redissolved this extractive substance; and although it had been washed repeatedly with alcohol and dissolved in water, it reddened very sensibly turnsole paper.

I believed it possible that the precipitate formed by infusion of galls was occasioned by an animal substance held in solution by means of this free acid; consequently I poured into the liquor ammonia till it was saturated. There was a very abundant flaky precipitate, which I separated by the filter and washed very carefully. The solution, united with the washings, did not give any more precipitate with gall-nut; and the acetate of lead, as well as the oxygenated muriatic acid, did not produce any change.

I burnt a part of the precipitate formed by ammonia to have some notion of its nature; but in place of presenting the characters of an animal or vegeto-animal matter, as I suspected it to be, it burnt without swelling, and left for residue a grayish powder, which I ascertained to be a calcareous salt. This experiment showed me, that if the liquor had ceased to precipitate by gall-nut, &c. this would have depended merely on the degree of concentration. I therefore evaporated it; and after having reduced it to three-fourths of its volume, I still obtained precipitates by the acetate of lead, the oxygenated muriatic acid, and gall-nut.

I continued the evaporation till I obtained a dry extract: it was of a dark brown colour, had an agreeable taste, a little saltish, and slightly attracted the humidity of the atmosphere.

mosphere. I calcined a part of it; it swelled considerably, and the smell that was disengaged rather announced a vegetable substance than a product of an animal nature. The incinerated charcoal left a little lime. Thus certain reagents denoted the presence of an animal matter; others appeared to show the opposite. To obtain a more conclusive result, I redissolved this extract. I treated it with tincture of galls, and after washing and drying the precipitate, I submitted it to distillation to see if it yielded ammonia. Some was disengaged, but very small in quantity; so that, although we cannot deny that this principle, which is soluble in water, has many characters exclusively belonging to animal matters; it is, however, fair to say, that it has some properties which appear very different, and which could not agree with the admission of animal gelatine.

It only remains to me to speak of a salt which is found in the decoction and in the juice of asparagus. To obtain it, we ought to evaporate both to the consistence of thick syrups. At the end of some days a salt is deposited, which when washed and redissolved in water, presents the following properties: it crystallizes in little rhomboids more or less regular, transparent and very white, almost insipid; very little soluble in the cold, but much more so in heat; and depositing itself upon cooling: it swells when burning, giving out a sharp but agreeable smell, and a little ammoniacal. Its charcoal, which was very considerable in volume, is easily incinerated, and leaves very little residue: when pounded with caustic alkali it gives sensible marks of ammonia. Its solution is not precipitable either by the alkalis or by alkaline earths. The oxalate of ammonia produces a slight turbidness, the muriate of barytes and the nitrate of silver do not occasion any change, neither is it decomposed by acetate of lead. Thus all that I have been able hitherto to discover of its nature is, that it has a double base, lime and ammonia; but I must confess I am entirely ignorant what acid is its constituent. Its not decomposing the acetate of lead and the muriate of barytes, prevents us from confounding it with tartarous acid, oxalic acid, &c. Vinegar is what appears to come nearest to it; but it is in vain

vain that I have tried to produce a similar salt with the acetous acid. I am therefore obliged to wait till I shall have obtained a greater quantity of the salt which this juice contains, before I can pronounce definitively on the nature of this acid.

I must further observe, that the presence of ammonia does not authorise us to think that we cannot obtain this triple salt till after having allowed a commencement of alteration in the juice of asparagus; for I have had it on evaporating decoctions and juices recently prepared, but always in small quantities.

To recapitulate what I have had the honour of laying before you, I shall detail the principal substances which have been furnished me by the juice of asparagus. The green feculent substance is itself composed of three others; the first insoluble in alcohol, and which approaches nearer to the nature of animal matter than any that is contained in the vegetable. The two others are soluble; but one of them is deposited on cooling. This is what I call vegetable wax. The latter is only obtained by evaporation, and appears to hold a middle nature between volatile oils and resins.

We find in the filtered juice, 1st, Albumen, which coagulates on the first ebullition: 2d, Phosphate of potash, of which we separate the acid in precipitating it by the acetate of lead: 3d, The same acid combined with lime, and held in solution by a portion of free acetous acid: 4th, Foliated earth (*de la terre foliée*) and phosphate of potash in very great abundance: 5th, The vegeto-animal substance which is found in the aqueous solution: 6th, An extractive matter which we obtain after having precipitated by gall-nuts that portion of the extract which is insoluble in alcohol: 7th, A triple salt of lime and ammonia, of which the acid is yet unknown to me: 8th, and lastly, A colouring principle susceptible of becoming red by acids and yellow by alkalis*.

The results which I have obtained are different, as you observe, in several points from those announced by M. Antoine; but I beg you to remark that we have not followed

* It may be presumed that it is the acetous acid which carries the animal substance of the green feculum with it down to the urinary passages.

the same mode of analysis, and that certain substances which I have separated without difficulty, could not present themselves to him but after a number of experiments. But I am far from pretending to have made a complete analysis, and I shall esteem myself very fortunate if I have been able to present you with some facts worthy of your attention.

XXIV. *On the inverted Action of the alburnous Vessels of Trees. By* Thomas Andrew Knight, *Esq. F.R.S. In a Letter to the Right Honourable Sir* Joseph Banks, *K.B. P.R.S.**

MY DEAR SIR,

I have endeavoured to prove, in several memoirs† which you have done me the honour to lay before the Royal Society, that the fluid by which the various parts (that are annually added to trees, and herbaceous plants whose organization is similar to that of trees,) are generated, has previously circulated through their leaves ‡ either in the same or preceding season, and subsequently descended through their bark; and after having repeated every experiment that occurred to me, from which I suspected an unfavourable result, I am not in possession of a single fact which is not perfectly consistent with the theory I have advanced.

There is, however, one circumstance stated by Hales and Du Hamel which appears strongly to militate against my hypothesis; and as that circumstance probably induced Hales to deny altogether the existence of circulation in plants, and Du Hamel to speak less decisively in favour of it than he possibly might otherwise have done, I am anxious to recon-

* From *Philosophical Transactions* for 1806.

† In the Phil. Trans. for 1801, 1803, 1804, and 1805.

‡ During the circulation of the sap through the leaves, a transparent fluid is emitted, in the night, from pores situated on their edges; and, on evaporating this liquid, obtained from very luxuriant plants of the vine, I found a very large residuum to remain, which was similar in external appearance to carbonate of lime. It must, however, have been evidently a very different substance, from the very large portion which the water held in solution. I do not know that this substance has been analysed, or noticed by any naturalist.

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cile the statements of these great naturalists (which I acknowledge to be perfectly correct) with the statements and opinions I have on former occasions communicated to you.

Both Hales and Du Hamel have proved, that when two circular incisions through the bark, round the stem of a tree, are made at a small distance from each other, and when the bark between these incisions is wholly taken away, that portion of the stem which is below the incisions through the bark continues to live, and in some degree to increase in size, though much more slowly than the parts above the incisions. They have also observed that a small elevated ridge (*bourvelet*) is formed round the lower lip of the wound in the bark, which makes some slight advances to meet the bark and wood projected, in much larger quantity, from the opposite or upper lip of the wound.

I have endeavoured, in a former memoir*, to explain the cause why some portion of growth takes place below incisions through the bark, by supposing that a small part of the true sap, descending from the leaves, escapes downwards through the porous substance of the alburnum. Several facts stated by Hales seem favourable to this supposition; and the existence of a power in the alburnum to carry the sap in different directions, is proved in the growth of inverted cuttings of different species of trees †. But I have derived so many advantages, both as a gardener and farmer, (particularly in the management of fruit and forest trees,) from the experiments which have been the subject of my former memoirs, that I am confident much public benefit might be derived from an intimate acquaintance with the use and office of the various organs of plants; and thence feel anxious to adduce facts to prove that the conclusions I have drawn are not inconsistent with the facts stated by my great predecessors.

It has been acknowledged, I believe, by every naturalist who has written on the subject, (and the fact is, indeed, too obvious to be controverted,) that the matter which enters into the composition of the radicles of germinating seeds

* Phil. Trans. for 1803. † Ibid. for 1804.

existed

existed previously in their cotyledons; and as the radicles increase only in length by parts successively added to their apices, or points most distant from their cotyledons, it follows of necessity that the first motion of the true sap, at this period, is downwards. And as no alburnous tubes exist in the radicles of germinating seeds during the earlier periods of their growth, the sap in its descent must either pass through the bark or the medulla. But the medulla does not apparently contain any vessels calculated to carry the descending sap, whilst the cortical vessels are during this period much distended and full of moisture; and as the medulla certainly does not carry any fluid in stems or branches of more than one year old, it can scarcely be suspected that it, at any period, conveys the whole current of the descending sap.

As the leaves grow, and enter on their office, cortical vessels, in every respect apparently similar to those which descended from the cotyledons, are found to descend from the bases of the leaves; and there appears no reason, with which I am acquainted, to suspect that both do not carry a similar fluid, and that the course of this fluid is, in the first instance, always towards the roots.

The ascending sap, on the contrary, rises wholly through the alburnum and central vessels; for the destruction of a portion of the bark, in a circle round the tree, does not immediately, in the slightest degree, check the growth of its leaves and branches; but the alburnous vessels appear, from the experiments I have related in a former paper *, and from those I shall now proceed to relate, to be also capable of an inverted action, when that becomes necessary to preserve the existence of the plant.

As soon as the leaves of the oak were nearly full grown in the last spring, I selected in several instances two poles of the same age, and springing from the same roots in a coppice, which had been felled about six years preceding; and making two circular incisions at the distance of three inches from each other through the bark of one of the poles

* Phil. Trans. for 1801.

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on each stool, I destroyed the bark between the incisions, and thus cut off the communication between the leaves and the lower parts of the stem and roots, through the bark. Much growth, as usual, took place above the space from which the bark had been taken off, and very little below it.

Examining the state of the experiment in the succeeding winter, I found it had not succeeded according to my hopes; for a portion of the alburnum, in almost every instance, was lifeless, and almost dry, to a considerable distance below the space from which the bark had been removed. In one instance the whole of it was, however, perfectly alive; and in this I found the specific gravity of the wood above the decorticated space to be 1114, and below it 1111; and the wood of the unmutilated pole at the same distance from the ground to be 1112, each being weighed as soon as it was detached from the root.

Had the true sap in this instance wholly stagnated above the decorticated space, the specific gravity of the wood there ought to have been, according to the result of former experiments *, comparatively much greater: but I do not wish to draw any conclusion from a single experiment; and indeed I see very considerable difficulty in obtaining any very satisfactory or decisive facts from any experiments on plants in this case, in which the same roots and stems collect and convey the sap during the spring and summer, and retain within themselves that which is, during the autumn and winter, reserved to form new organs of assimilation in the succeeding spring. In the tuberous-rooted plants, the roots and stems which collect and convey the sap in one season, and those in which it is deposited and reserved for the succeeding season, are perfectly distinct organs; and from one of these, the potatoe, I obtained more interesting and decisive results.

My principal object was, to prove that a fluid descends from the leaves and stem to form the tuberous roots of this plant; and that this fluid will, in part, escape down the alburnous substance of the stem when the continuity of the

* Phil. Trans. for 1805.

cortical

cortical vessels is interrupted: but I had also another object in view.

Every gardener knows that early varieties of the potatoe never afford either blossoms or seeds; and I attributed this peculiarity to privation of nutriment, owing to the tubers being formed preternaturally early, and thence drawing off that portion of the true sap, which in the ordinary course of nature is employed in the formation and nutrition of blossoms and seeds.

I therefore planted, in the last spring, some cuttings of a very early variety of the potatoe, which had never been known to blossom, in garden pots, having heaped the mould as high as I could above the level of the pot, and planted the portion of the root nearly at the top of it. When the plants had grown a few inches high, they were secured to strong sticks, which had been fixed erect in the pots for that purpose, and the mould was then washed away from the base of their stems by a strong current of water. Each plant was now suspended in air, and had no communication with the soil in the pots except by its fibrous roots; and as these are perfectly distinct organs from the runners which generate and feed the tuberous roots, I could readily prevent the formation of them. Efforts were soon made by every plant to generate runners and tuberous roots; but these were destroyed as soon as they became perceptible. An increased luxuriance of growth now became visible in every plant, numerous blossoms were emitted, and every blossom afforded fruit.

Conceiving, however, that a small part only of the true sap would be expended in the production of blossoms and seeds, I was anxious to discover what use nature would make of that which remained; and I therefore took effectual means to prevent the formation of tubers on any part of the plants, except the extremities of the lateral branches, those being the points most distant from the earth in which the tubers are naturally deposited. After an ineffective struggle of a few weeks, the plants became perfectly obedient to my wishes, and formed their tubers precisely in the places I had assigned them. Many of the joints of the plants during the experiment

experiment became enlarged and turgid; and I am much inclined to believe, that if I had totally prevented the formation of regular tubers, these joints would have acquired an organization capable of retaining life, and of affording plants in the succeeding spring.

I had another variety of the potatoe, which grew with great luxuriance, and afforded many lateral branches; and just at that period, when I had ascertained the first commencing formation of the tubers beneath the soil, I nearly detached many of these lateral branches from the principal stems, letting them remain suspended by such a portion only of alburnous and cortical fibres and vessels as were sufficient to preserve life. In this position I conceived, that if their leaves and stems contained any unemployed true sap, it could not readily find its way to the tuberous roots, its passage being obstructed by the rupture of the vessels, and by gravitation; and I had soon the pleasure to see, that, instead of returning down the principal stem into the ground, it remained and formed small tubers at the base of the leaves of the depending branches.

The preceding facts are, I think, sufficient to prove that the fluid, from which the tuberous root of the potatoe, when growing beneath the soil, derives its component matter, exists previously either in the stems or leaves; and that it subsequently descends into the earth; and as the cortical vessels, during every period of the growth of the tuber, are filled with the true sap of the plant, and as these vessels extend into the runners, which carry nutriment to the tuber, and in other instances evidently convey the true sap downwards, there appears little reason to doubt that through these vessels the tuber is naturally fed.

To ascertain, therefore, whether the tubers would continue to be fed when the passage of the true sap down the cortical vessels was interrupted, I removed a portion of bark of the width of five lines, and extending round the stems of several plants of the potatoe, close to the surface of the ground, soon after that period when the tubers were first formed. The plants continued some time in health, and during that period the tubers continued to grow, deriving their nutriment,

ment, as I conclude, from the leaves by an inverted action of the alburnous vessels. The tubers, however, by no means attained their natural size, partly owing to the declining health of the plant, and partly to the stagnation of a portion of the true sap above the decorticated space.

The fluid contained in the leaf has not, however, been proved, in any of the preceding experiments, to pass downwards through the decorticated space, and to be subsequently discharged into the bark below it; but I have proved with amputated branches of different species of trees that the water which their leaves absorb, when immersed in that fluid, will be carried downwards by the alburnum, and conveyed into a portion of bark below the decorticated space; and that the insulated bark will be preserved alive and moist during several days*; and if the moisture absorbed by a leaf can be thus transferred, it appears extremely probable that the true sap will pass through the same channel. This power in the alburnum to carry fluids in different directions probably answers very important purposes in hot climates, where the dews are abundant and the soil very dry; for the moisture the dews afford may thus be conveyed to the extremities of the roots: and Hales has proved that the leaves absorb most when placed in humid air; and that the sap descends, either through the bark or alburnum, during the night.

If the inverted action of the alburnous vessels in the decorticated space be admitted, it is not difficult to explain the cause why some degree of growth takes place below such decorticated spaces on the stems of trees; and why a small portion of bark and wood is generated on the lower lip of the wound. A considerable portion of the descending true sap certainly stagnates above the wound, and of that which escapes into the bark below it, the greater part is probably carried towards, and into, the roots; where it preserves life, and occasions some degree of growth to take place. But a small portion of that fluid will be carried upwards by capillary attraction, between the bark and the alburnum, exclu-

* This experiment does not succeed till the leaf has attained its full growth and maturity, and the alburnum of the annual shoot its perfect organization.

sive

sive of the immediate action of the latter substance; and the whole of this will stagnate on the lower lip of the wound, where I conceive it generates the small portion of wood and bark, which Hales and Du Hamel have described.

I should scarcely have thought an account of the preceding experiments worth sending to you, but that many of the conclusions I have drawn in former memoirs appear, at first view, almost incompatible with the facts stated by Hales and Du Hamel, and that I had one fact to communicate relative to the effects produced by the stagnation of the descending sap of resinous trees, which appeared to lead to important consequences. I have in my possession a piece of a fir tree from which a portion of bark, extending round its whole stem, had been taken off several years before the tree was felled; and of this portion of wood one part grew above, and the other below, the decorticated space. Conceiving that, according to the theory I am endeavouring to support, the wood above the decorticated space ought to be much heavier than that below it, owing to the stagnation of the descending sap, I ascertained the specific gravity of both kinds, taking a wedge of each as nearly of the same form as I could obtain, and I found the difference greatly more than I had anticipated, the specific gravity of the wood above the decorticated space being 0·590, and of that below only 0·491; and having steeped pieces of each, which weighed a hundred grains, during twelve hours in water, I found the latter had absorbed 69 grains, and the former only 51.

The increased solidity of the wood above the decorticated space, in this instance, must, I conceive, have arisen from the stagnation of the true sap in its descent from the leaves; and therefore in felling firs, or other resinous trees, considerable advantages may be expected from stripping off a portion of their bark all round their trunks, close to the surface of the ground, about the end of May or beginning of June, in the summer preceding the autumn in which they are to be felled. For much of the resinous matter contained in the roots of these is probably carried up by the ascending sap in the spring, and the return of a large portion of this matter

matter to the roots would probably be prevented *: the timber I have, however, very little doubt would be much improved by standing a second year, and being then felled in the autumn; but some loss would be sustained owing to the slow growth of the trees in the second summer. The alburnum of other trees might probably be rendered more solid and durable by the same process; but the descending sap of these, being of a more fluid consistence than that of the resinous tribe, would escape through the decorticated space into the roots in much larger quantity.

It may be suspected that the increased solidity of the wood in the fir-tree I have described was confined to the part adjacent to the decorticated space; but it has been long known to gardeners, that taking off a portion of bark round the branch of a fruit-tree occasions the production of much blossom on every part of that branch in the succeeding season. The blossom in this case probably owes its existence to a stagnation of the true sap extending to the extremities of the branch above the decorticated space; and it may therefore be expected that the alburnous matter of the trunk and branches of a resinous tree will be rendered more solid by a similar operation.

I send you two specimens of the fir wood I have described, the one having been taken off above, and the other below the decorticated space. The bark of the latter kind scarcely exceeded one-tenth of a line in thickness; the cause of which I propose to endeavour to explain in a future communication relative to the reproduction of bark.

I am, &c.

T. A. Knight.

* The roots of trees, though of much less diameter than their trunks and branches, probably contain much more alburnum and bark, because they are wholly without heart wood, and extend to a much greater length than the branches; and thence it may be suspected that when fir-trees are felled, their roots contain at least as much resinous matter, in a fluid moveable state, as their trunks and branches, though not so much as is contained, in a concrete state, in the heart wood of those.

XXV. *A*

XXV. *A Memoir on the best Method of measuring Time at Sea, which obtained the double Prize adjudged by the Royal Academy of Sciences; containing the Description of the Longitude Watch presented to His Majesty the 5th of August* 1766. *By M.* Le Roy, *Clock-maker to the King. Translated from the French by Mr.* T. S. Evans, *F.L.S., of the Royal Military Academy, Woolwich.*

[Continued from p. 68.]

Article V.

Description of the escapement of the new watch, which preserves isochronism in the regulator, and freedom in its vibrations.

All that precedes has proved to us, that the best, the most certain, and even the only method of bringing a marine clock to the requisite degree of truth, is, as has been said, to render the vibrations of its regulator as free and as isochronous as possible*: this is what I have followed. It has been seen in what manner I have arrived at it,—by the suspension of the regulator. But to preserve to it this freedom, so precious in its application to the wheel-work, it is necessary to employ an escapement totally different from those which have hitherto been made.

Of what use would it have been, in effect, to have annihilated friction in the suspension, if by the nature of the escapement the regulator had met with twenty times as much? This is what would have happened if I had had recourse to the cylinder, or to other dead escapements, which, in the end, amount to the same as this first; the friction being always very much increased. I dare affirm that it was, in a great measure, on account of the escapement that most of the attempts to discover the longitude by clock-work have miscarried. We may consult on this subject the remarks made by M. Sully on marine clocks.

The following is an experiment which will show how considerable the friction is even in the best escapements. I took a cylinder watch, by Mr. Graham, and turned the ferrule

* I cannot here agree in opinion with Mr. Daniel Bernoulli, who recommends (*ibidem* pag. 43) to augment purposely the resistance of the air to the motion of the balance, and to add to it three or four small wings.

ferrule of the spiral in such a manner, that the points of the wheel, instead of corresponding to the lips of the cylinder, fell in the middle of the cylindric portion where the dead part takes place: I then moved the balance, whose axis was vertical, from its point of rest by an arc of about 80°; it only remained in vibration four seconds and a half; instead of which, when it was free on its pivots, it vibrated a minute and a half.

To avoid this inconvenience of the best known escapements, I have used in the new watch an escapement nearly similar in its principle to that with *a detent*, which is the invention of M. le Roy, and described in the History of the Academy for the year 1748 *. The balance wheel *r* (Pl. I. fig. 7, and Pl. III.), whose teeth are very wide apart, and very slight, and consequently whose strength is very small, its power consisting in the length of the lever on which it acts; the balance wheel, I say, by means of the pallet *p*, adapted to the circumference of the balance (Plate III. fig. 1, 2, and 3.) restores to it, every second vibration, the motion which it loses; and its action is suspended in these vibrations by an obstacle foreign to this regulator, that is to say, by a sort of detent D, *e*, H, C, F, (fig. 2, 3, and 5, Pl. III.)†. The following is the way it is done:

The balance wheel stopped by the detent at D, as in fig. 2. Plate III, and the balance turning on its axis from *i* to A, after having surmounted the spiral springs, and consumed its force, these springs bring it back, and make it turn from A to *i*. In this return, by means of a pin situated on its upper plane at *i*, the balance pushes the arm of the lever F, H; and consequently draws out the arm D H of the detent from the circumference of the wheel and makes the arm *e* H enter, on which the following radius K *r* of the wheel rests: this is what I call the preparation, and is represented in fig. 3. In the following vibration the balance wheel restores the motion to the balance by means of the pallet *p*, in the following manner:—A pin situated as the preceding, but on the lower plane of the balance, pushing the arm of the lever

* This is the first work that I published.

† This detent forms a kind of escapement which may be varied at pleasure, using indifferently that of Graham, Aman, Sully, &c.

c H,

e H, draws out the arm e H of the anchor from the circumference of the wheel, and makes D H enter; and when the pallet p has arrived at F, then, the wheel being free, the radius F r (fig. 1.) restores to the balance its lost motion, and pushes the pallet p until it is stopped by the arm D of the detent, &c., as in fig. 2.

By this construction, with the exception of the very small arc employed for the disengagement of the detent, and the pulsion of the balance wheel, the vibrations of the balance are absolutely free, and disengaged from all friction on the part of this wheel, stayed, as I have said, on an obstacle absolutely foreign to the regulator; and as the disengagement of the detent, and the restoration of motion by the wheel, are executed about the middle of the arcs of vibration, where the regulator has arrived at its greatest velocity, the very slight obstacle which this detent causes, &c. becomes yet much smaller; the obstacles of friction, of cohesion, like those of gravity, are proportional, as has been said, to the time during which the body surmounts them.

I shall add to this description, that to hinder the detent from getting out of its place by great shocks, I have placed in the circumference of the balance, near each pin which removes the detent, a portion of a circle i A, A i (fig. 1, 2, and 3, Plate III.), which the correspondent arm of this detent, nevertheless, can never touch, except in cases of the most violent shocks.

To find out the degree of freedom preserved to the regulator by my escapement, I made an experiment similar to that which I made with Graham's escapement. I took away from my balance the pallet p, by which the balance wheel restores the motion, leaving only the detent. Having then removed this regulator from the point of rest, about 90°, it vibrated seven minutes nearly; at the end of this time it would even yet describe in its vibrations a sensible arc, although not sufficiently great to disengage the detent. I conclude from this, that the influence of friction is almost nothing in my regulator; for the detent, although very slight, has nevertheless a small mass, and in the preceding experiment the regulator could not move it in each vibration without employing a considerable part of the lost force;

force; whence we may legitimately infer, that what is destroyed by friction is almost nothing: now the obstacle arising from my detent must be reputed of no value, consisting in a mass always the same, whilst the friction varies continually.

I made the same trial with a seconds pendulum having an anchor escapement. The whole motion of the pendulum ceased in about thirty minutes, whence I believe I can conclude, considering what has been said of the resistance of the detent, and of the motion which remains to the balance after seven minutes, that this balance in the new watch has almost as much regulating power as this pendulum.

I have said that my pallet was situated near the circumference of the balance. I placed it thus, in order that it might be drawn by a point in its circumference of percussion, which point is where the wheel in its action makes no effort on the pivots, and where the balance only receives the circular motion*.

Moreover, although the escapement of the new watch, and that which M. le Roy presented to the Academy in 1748, are founded on the same principle, they differ nevertheless essentially. In the first, the effect of the detent operates by means of a small spring, which brings it back into the teeth of the wheel: in this there is nothing of that kind, as we have seen. Various trials have proved to me the inconveniences of the spring escapements. These springs are either strong or weak: in the first case, it is to be feared that the detent would be disengaged by shocks; in the second, you give to the regulator a considerable obstacle to overcome in each vibration, which obstacle being the same for the smallest as it is for the largest arcs, must be disadvantageous. Besides, if this detent, that is moved by so feeble a spring, meets with ever so slight a difficulty; or if this spring loses its strength, it does not enter sufficiently quick

* It is extraordinary that M. le Roy nowhere mentions his having applied jewels either to the pivot-holes or to the balance of his watch. Sully (p. 248, Regle Artif. du Temps) says that in 1704 Sir Isaac Newton showed him a watch, that was put into his hands to try by Messrs. Facio and de Baufre, the pallets of whose balance were formed of a diamond; and expressly mentions that the art of piercing rubies was invented by this M. Facio, of Geneva, about the year 1700. The utility of their application must therefore have been fully known to watch-makers at the time Le Roy wrote this paper, and it is extraordinary that he did not make use of this additional advantage.

into

into the teeth of the balance wheel, and then several teeth escape. Lastly, after many attempts of this kind, I was only completely satisfied with that of the new marine watch.

Article VI.

Of the compensation for the effects of heat and cold:—Of the necessity of preserving to the spiral spring an invariable length:—Means by which, without changing this length, we regulate the new watch to almost the smallest quantity:—Description of the new compensation, &c.

The first thing that I thought necessary to clear up before I attempted to compensate the effects of the different degrees of heat and cold in my machine, was, the proportion that its gain or loss followed by these different degrees. I feared much that this progression was not proportional to that of the degrees marked by the thermometer; that, for example, the watch having lost three seconds for six degrees of ascent in the thermometer, it would not lose six for twelve degrees of this same instrument, but either a greater or less quantity. Various reiterated experiments happily proved to me that my fears were unfounded; that when the regulator was free, as it is in my machine, the progression of gain or loss absolutely follows that of the thermometer; that is to say, that the watch losing 3 seconds when the thermometer from 0 passes to 6, it will lose 6 at 12, 9 at 18, and so on. If in our researches nature often contradicts our views, we may say that she is sometimes more favourable than we had reason to hope: of this, the precision to which clock-work is arrived furnishes undoubted proofs.

All this shows the indispensable necessity of having a perfectly free regulator, without which, the effect of heat on the machine depending more or less on the fluidity of the oil, which is very variable in different degrees of heat and cold, and the alterations of this fluidity, produced by time and by the wear of the parts, &c., the progression remarked above no longer takes place, and has not even any thing certain: this is a *just* objection made by M. Bass r* against Mr. Harrison, among a number of others which are not so.

* See the *Gazette du Commerce*.

After being well assured of the fact I have just described, it appeared necessary to examine a second, not less important to clear up, and to know whether metals would follow the same progression as fluids in their extension or contraction by heat and cold; which required very nice experiments. To make them with some success, I nailed in a cabinet against a thick stone wall, at four feet distance from each other, in a vertical line, two potences of copper, the upper one of which carried an index of thin hard steel about four feet in length, which descended almost vertically: I then took three rods, one of copper, one of iron, and one of steel, of nearly equal size, and four feet in length. I had made to each of these rods, as well as to a tube of glass of exactly the same length, to serve as a standard, a sheath, made sufficiently thick of cloth: these rods and this tube were adjusted firmly, without being able to turn, by their lower extremity, and by a pivot adapted to their upper extremity, they caused the index to move whose path was marked on a limb.

All being equal in the arrangement of the three rods and the tube, I began my experiments, and I presently saw that, to have any thing exact on this subject, it was necessary that the rods should remain a long time exposed to the degree of heat and cold in which we would make these experiments; particularly when from a considerable degree of heat, as 20 or 30 degrees * for example, I wished to remove my rods to a degree of cold approaching that of ice. The reason is known: by the experiments of Boerhaave, and those of Newton, bodies attract heat in proportion to their specific gravities. Now when you would remove a body whose degree of cold corresponds to 0 of the thermometer, for example, to the term 30 degrees of this same instrument, by placing it in an air that is of this degree, it is clear that by its attraction it will presently have acquired the quantity of caloric that will give it the degree of heat 30. But it will not be the same if you then remove this body to an air where the thermometer is 0†, to make it acquire this degree of cold;

* Of Reaumur's thermometer, equal to from 77 to 99½ of Fahrenheit's.

† 32 of Fahrenheit.

for

for then, its attraction being much stronger than that which the ambient air opposes to the quitting of the particles of caloric, it cools with so much the more difficulty. I have found, indeed, that after having heated our rods and replaced them again in the temperature 0, whence they had been taken, it required sometimes almost twelve hours to reduce them to the length they were before; that is to say, for the overplus of caloric totally to abandon the interior. After having found this effect, and paid the greatest attention in my experiments, I happened at last to find in their results the exactness which I had vainly sought for before; and I found that the glass and metals in their contractions and expansions followed precisely (as well as the augmentations and losses of elasticity of springs) the proportion of the degrees described by the spirit-of-wine thermometer.

These methods of proving the various contractions and dilatations of metals appear to me very exact: for, 1st, the cabinet where the instrument was placed being defended from the external air, no considerable change could happen to the wall (which was hung with tapestry, and to which our potences were fixed) between one experiment and the other: 2dly, when it does happen (and in effect it is sometimes seen very evidently that the wall of which I have spoken is dilated by heat as much as the steel very nearly), then, I say, this effect is announced to us by the spirit-of-wine thermometer on the one part, and by the tube of glass on the other, which was very little dilatable; and which tube may, besides, be kept in the same temperature.

The sheaths with which our rods were covered enabled them to be removed from one place to another; that is to say, from a stove, or from a cool place in the cabinet of trial, and to adjust them on the instrument before they had undergone any alteration in their dimensions and in their degree of cold or heat, which would not appear practicable otherwise. Being well assured of these facts, I turned my attention to compensating the effects of heat and cold in my machine.

The first idea that occurred to me, as to many others, was, to apply to the regulating spring a metallic thermometer, which would shorten or lengthen it by different temperatures, as has been practised in the seconds pendulum. I found presently the insufficiency of this method. Having in my machine two regulating springs of about 18 inches each, to produce the desired effect, the shortening or lengthening would be proportional to this length. By the computation which I made, four lines passed over in the compensation pyrometer would barely have sufficed; now three feet of copper combined with three feet of steel hardly gives a sixth of a line of difference in their lengthening for 30 degrees of ascent in the liquor of the thermometer. We see, therefore, that I had not the power of obtaining an exact compensation, unless by multiplying the effect by very large levers, and a great number of metallic bars; but all this brings in a play of the parts, and a want of solidity in the pieces of the regulator, absolutely incompatible with the desired exactness.

A second consideration determined me against making use of these expedients in my machine, and in general all those that alter the length of the regulating spring; which is, as I have explained, that the isochronism of the vibrations absolutely depends on a certain length of the spiral springs: now every method which renders this length variable, not even excepting that of Mr. Harrison, although very ingenious, is on this account inadmissible.

Here, I am sensible, an objection may be made to me which I shall make myself: as a spring loses its elastic force by heat, it may be suspected that the place in its length where all its vibrations are isochronous cannot be the same in different degrees of heat and cold. The following are the experiments which dispelled my doubts on this subject:—After having found by experiment, in a temperate place, the length of a spring where all its vibrations, long and short, were isochronous, I removed the machine to a cold five degrees below freezing*; the watch experienced an advance proportional to the cold, (for the sake of greater simplicity I had not applied any thermometer to it:) then I made it go six hours, the great spring being almost down; and during

* About 20¾ of Fahrenheit.

six

six other hours the machine being wound to the top, which produced a difference of one-half in the extent of the arcs. I found, then, that the regulating spring had preserved all the isochronism of its vibrations, the machine having advanced precisely the same quantity in the first six hours as in the six last. Not contented with this trial, I removed the machine into a stove where the thermometer constantly stood at about 35 or 40 degrees*: it then retarded proportionally to this degree of heat. I repeated the above trial, which again gave me the same result; whence it follows, that the different degrees of heat do not sensibly change the laws of isochronism in springs.

I concluded from this experiment, that whatever expedient we may use to render the vibrations of the balance isochronous, the inconvenience remarked above in the compensations which are made by the shortening or lengthening of the spiral spring, does nevertheless take place. For let it be by means of a compensation curb formed at the parts of the escapement, or applied to the spiral spring, &c., that the isochronism is produced, or by other similar methods; it will be perceived that these curbs, &c. are always according to the relations which exist in the times of vibrations of different extent of this spring, supposed to be free; and these relations can never change without the conditions of these curbs varying at the same time. Thus all the reasons which have induced me to render the length of the spiral invariable, must also apply, in whatever construction it may be, where we aspire to give the greatest degree of accuracy to a watch.

It follows again, from what precedes, that nothing can be more opposite to the regularity of a marine watch than to regulate it, as in common watches, by shortening or lengthening the spiral; therefore, for my own part, I took good care not to make use of it in my watch. For this purpose I have placed two screws, GZ, GZ, (Plate III. fig. 6.) perfectly equal, at the bottom of the balance arbor; so that we may, by turning them with the hand, make them approach or recede equally from this arbor. These screws by their mass, which may be diminished at pleasure, according to the exi-

* From 110¾ to 122 of Fahrenheit.

gency

gency of the case, and which we may make to describe a large space, permit us to regulate the machine to the greatest nicety.

If the effects of heat and cold were less durable, the inconvenience I have just explained might be neglected; but as the machine is in a state of trial during more than six months together, it is clear that as the vibrations of different extent of the regulator have not then the requisite isochronism, the causes which may make the magnitude of these vibrations vary, would alter the regularity of the clock considerably. Convinced of the principle I have just established, to compensate the effects of different temperatures in my machine, I took a method altogether new.

I adapted to the balance several small bars of copper and steel, disposed in such a manner, that, by their lengthening or shortening in heat and cold, they make to approach, or recede from its centre, two considerable parts of its mass, each placed at the extremity of a lever, and diametrically opposite. By the computation which I had made, it sufficed that the total mass of the balance should approach or recede from the centre by about the thirteenth of a line, to compensate a variation in heat, which would produce one of 15 seconds per hour in the rate of the watch.

One inconvenience of the preceding method made me abandon this presently; the play of the levers, and the small solidity of the balance, produced errors greater than those which I wished to compensate: this made me have recourse to a third method, which completely succeeded.

It consists in applying to the balance two small thermometers *ttttt*, &c. (Plate III. fig. 6.), each made of a tube of bent glass, open at O, fig. 7. These thermometers, composed of mercury and spirit of wine, would each form an exact parallelogram, if the upper side which carries the ball, in which is contained the spirit of wine as well as in this side, were not a little inclined. Both these thermometers are firmly adjusted and placed on opposite sides of the arbor of the regulator, so that the axes of their tubes and that of the balance are in the same plane that cuts the balls through the middle.

It is easy to conceive how this construction produces the required

required compensation. The thermometers making part of the regulator, when the spirit of wine, by its dilatation, pushes a part of the mercury contained in the outer branch *tt* (fig. 7.) towards that *to*, which is near the axis of motion, a portion of the mercury, forming part of the mass of the regulator, passes then from its circumference towards its centre. At temperate, for example, the mercury occupies the parts *tkkt* of the tube; whilst, in extreme cold, when Reaumur's thermometer is at 15 degrees below freezing*, the branch *to* is empty, and that corresponding, *tt*, is full of mercury. Now as the mass of a balance resists in the ratio of the square of its distance from the centre, there arises evidently from this a compensation; the retardation arising from losses of elasticity in springs, and from the dilatation of the balance by great heat, being compensated by the loss of mass in the circumference of the regulator, and *vice versâ* in the passage to cold: this effect is so much the more certain, as there is no play to fear here; besides, the dilatation of spirit of wine by heat, and its condensation by cold, are constant effects, as we have found by the thermometers of this liquor, which at the end of thirty years had lost nothing of their exactness.

The following is the computation of these thermometers, to which I have given the form we see them of, in order that the balls might be turned towards the centre of the balance, and also to diminish the resistance that the air gives to the motion if it was near the circumference.

Calculation for the Thermometers.

Suppose the weight of the balance to be three ounces, or 1728 grains, experience proves that for 30 degrees of heat a marine watch where this effect is not compensated loses 15″ per hour, or $\frac{1}{240}$th part. It is therefore necessary, that by the diminution of its mass the balance should produce an equivalent acceleration: now 1728, the number of grains of which three ounces are composed, divided by 240, is equal to seven grains; and as the accelerations are only as the square roots of the diminutions of the mass, it is necessary to take away about 14 grains of the mercury, placed towards

* Or $1\frac{1}{4}$ below zero of Fahrenheit's.

the

the centre of percussion of the balance, to produce the desired compensation: this is what is effected by a ball of spirit of wine of about five lines and a half in diameter, which size may be varied according to the greater or less force of the liquor. Whence it is evident that we may diminish or increase the effect at pleasure to arrive at the precise term of the compensation: 1st, by augmenting or diminishing the size of the ball: 2dly, by placing the exterior branch farther from, or nearer to, the centre of the balance; 3dly, by using spirits of wine more or less rectified.

These thermometers, having a very small mass, produce their compensation at the same instant that the springs are dilated or contracted: this is what cannot happen in all kinds of thermometers composed of metallic bars, which to be solid must be made very strong with regard to the blade of the regulating spring.

I use two thermometers instead of one, which at first would appear to suffice; without this, in the different degrees of heat and cold, the regulator would not preserve its equality of weight in all points of its circumference; which is an important thing, as I have explained, to prevent the effect of shocks and various motions.

Before we terminate this article let us remove some difficulties that may be thrown in our way.

It may at first be feared that the two liquids would mix by violent shocks; it is easy to be assured of the contrary by taking a similar thermometer, turning it upside down, and shaking it.

Perhaps it may also be apprehended that the spirit of wine would be compressed, more or less, according to the weight of the atmosphere, because I leave the inner branch open, in order that all the parts of the mercury and spirit of wine may tend with more force towards the ball: I will not deny that this will happen, but in so small a quantity that it must absolutely be neglected. By Mr. Canton's experiments the whole weight of the atmosphere only compresses the spirit of wine the $\frac{66}{10000000}$th part; therefore the variations of these weights, which hardly amount to $\frac{1}{14}$th, would only form in 24 hours the variation of four millionth and a half of 5′ or 300″, 300″, which the compensation produces; that is to say, about the 600dth part of a second in 24 hours.

Lastly, it may be asked why, instead of making my thermometers of one liquor only, I have used mercury and spirit of wine. It is well known that spirit of wine, being more than eighteen times lighter than mercury, it would have made my thermometers too large for my regulator, which would have augmented the resistance of the air, &c.

With regard to mercury, exclusive of its having the inconveniences which I have just remarked in spirits of wine alone, its dilatation would not have been sufficient. Besides, it would have been imprudent to have used too great a mass that was fluid and moveable, whose inertia in any shock would have broken the tubes, &c.*

Article VII.

Of the methods used in the new watch to make all its parts remain the same, after having been subjected to the greatest differences in temperature.

The effects of heat and cold which I have just examined in the preceding article, are not, in my opinion, those which would be most opposed to the regularity of a marine watch; there is another, which I was unable to correct until after a great number of useless trials. The following is what made it known to me:

Being assured of the regularity of my machine in the same temperature, I was willing to try if it would preserve this same accuracy in great degrees of heat, such as those that are felt between the tropics: for this purpose, by means of a stove, I kept for near a day, in a small room, a degree of heat where the thermometer stood between 35 and 40 degrees: my compensation being a little too strong, instead of losing, as others would, my watch gained, as I had foreseen, some seconds: the advancement was easy to correct by the methods pointed out in the preceding article; but this I did not wait for: having then removed my machine into the mean temperature where it was before, it gained

* Berthoud says, one of the thermometers of one of Le Roy's watches broke during its trial in *La Flore*, by a blow which it received on the case.—T. S. E.

10″

10″ in 24 hours. I repeated the trial, which furnished a new advancement, less considerable than the preceding, and the same with a third, &c.: other watches, instead of gaining, on the contrary would lose by the preceding operation, which sometimes also does not produce any effect.

By reflecting on these subjects, I conceived that there happened here some effect similar to those observed in the elaterometer: it might very well be, that the springs, or the regulating springs (for I made this trial by different methods), experienced some change of figure by heat, which would augment or diminish their strength; whence I concluded that it would be necessary to take particular care that these springs move very freely, and that they do not receive the smallest constraint in their application to the balance.

With this view I made the pieces *dd* (Plate III. fig. 6.) capable of receiving all the requisite situations, that the spring might be attached without experiencing the least constraint. By their first motion they could move backwards or forwards in their groove to receive the springs; we might then raise or lower them at pleasure by turning them on the screws *dd* (Plate III.), by which they are affixed to the frame: lastly, the part where the spring is attached turns itself on a pivot, in order that this spring might be applied without changing its figure in any respect. When all these precautions are taken, we fix all the parts by their screws. By means of these pieces we diminish this effect greatly, if we do not totally destroy it; and we completely annihilate it, by successively heating and cooling the machine until it no longer exists. Without these precautions it is only by chance that we can produce a good marine watch.

Article VIII.

Means used to prevent the effect of shocks and different positions.

These shocks may be opposed to the regularity of our marine watch by two causes: 1st, because the regulator in receiving its own motion, that is to say, the motion that the motive force of the watch keeps up, may be augmented or diminished, &c.: 2dly, since by the repeated agitations

agitations of all the parts of the machine, especially those of the regulator, the situation, the form, and the texture of these parts may be changed; whence arise errors so much the more important, because, instead of being transitory, as others are, they would only cease when we had applied the remedy to the changes as they happen.

The methods which appeared to me proper to prevent these inconveniences are, 1st, to render all the parts of the watch as solid and as unalterable as they can be made: 2dly, to suspend the machine in such a way that it may receive little motion from shocks: 3dly, to diminish the effect of this motion as much as it can be done: lastly, to arrange things in such a manner, that this motion when received may not be preserved, but may cease, on the contrary, as soon as possible. To fulfil these different conditions, I took care not to give my spiral springs too great a magnitude or mass with regard to their strength. M. Daniel Bernoulli recommends in his *Mémoire* to make them very large; but, having applied some of them to my watch, I presently perceived that agitations a little considerable made them move and vibrate not only according to their length, but even in their width; whence I concluded that there was a mean term to be chosen in the size of these springs. After some experience, I fell upon that point of magnitude in the spring where all the vibrations are exactly isochronous* without being affected by considerable shocks.

Nothing could appear more opposite to that solidity, which is so necessary in the parts of the regulator, than the harpsichord wire by which it is suspended. Although its strength be such as to sustain a weight fourteen times greater than the balance without breaking, nevertheless it was much to be feared that it would not withstand violent shocks (which happened, in effect, before I had taken the precautions which I am going to mention), or at least that it would lengthen. To prevent this inconvenience, I attached the upper extremity of this wire to a spring x, x (fig. 7. Plate I.), sufficiently strong to keep the wire and the balance suspended, and elevate it to a fixed point above, against which this spring presses, and is stopped; by this means it is no longer the

* See Article II. of Part II.

wire which sustains the balance in great shocks. Then, the suspension spring, which could only raise a weight a little greater than the balance, gives way, and the extremity of the lower pivot of the balance touches and rubs during the moment of the shock on a plate, as is done in common watches when they are laid down.

An essential thing to determine was the magnitude of the vibrations. I acknowledge that the true place where we might find this with most success would be a sea-port. M. Bernoulli recommends that they should be very small; but I feared then that shocks would have had a considerable effect on them. I have therefore made the balance describe an arc of about 100 degrees in the long vibrations—that is to say, when the watch is just wound up—which is reduced to 90 after the watch has been going 24 hours. In other respects I have followed the principles of M. Bernoulli, *ibid.* who requires that a considerable motion of the balance should produce little change in the figure of the spring.

After having put the regulator, by the proper disposition of its parts, as much as possible out of the power of shocks, to fulfil our views entirely, it is necessary to find some method of rendering these motions as small as they can be; the least abrupt, and of as short duration as possible: this is what I have done by the suspension which I have given to the new watch.

The motions which it may receive can only be either horizontal, vertical, or in a direction compounded of these two. To compensate the first, which would particularly affect the spiral springs, I have suspended and rendered my watch moveable on two axes A, A (fig. 1. Plate IV.) adapted to a frame or strong parallelogram of copper AB, AB, which itself, as well as the watch, turns on two other axes B B, fixed to the box, which contains the whole machine*. By this means the watch forms a kind of pendulum, at the bottom of which are placed the spiral springs. When, therefore, they receive a shock, we see at first sight that it is the

* A watch suspended in this way is said to be hung upon *gimbals.* Berthoud (Eclaircissemens, p. 38.) attributes the contrivance to *Cardan:* but I have seen an old work on Mechanics, of much earlier date than any thing of Cardan's, where this suspension was proposed for a carriage to convey wounded soldiers from the field of battle, or from one place to another.—T.S.E.

points

points of suspension which feel it, and that the lower part of the pendulum, or the watch, remains almost fixed at that moment; this pendulum then redescends by an inclined plane to the vertical, whence it was removed by the shock, and that by a gentle motion, progressive and slow, which can neither affect nor derange that of the spiral springs.

These motions, although rendered more gentle and infinitely less prejudicial, nevertheless cannot be otherwise than contrary to the accuracy of the watch, if they were of long duration; that is to say, if the oscillations of this watch, forming a pendulum, continued like that of the common pendulums: this construction would then have had another more considerable disadvantage. These oscillations would increase by a continuation of shocks; whence it would happen that the points of suspension would be worn; that the watch would be found, in a great number of instances, in situations very remote from the vertical (which is the most advantageous situation, being the one in which it was regulated); and that the sum of the motions, now rendered less contrary to truth, would be considerably augmented. I have prevented this inconvenience, 1st, by gentle friction springs acting near the points of suspension on planes or large surfaces, which, with a sufficient resistance to diminish considerably the oscillatory motion, are very little subject to wear, and nevertheless permit the watch to move on these axes: 2dly, by a pad B, B, (fig. 2. Plate IV.) formed at the bottom of the case which contains the watch, and by cushions which are placed round the sides of the box, in such a way, that in great shocks the lower part of the watch meets obstacles that are supple and moveable, which extinguish the motion by their softness, and prevent it from continuing.

But a very essential point to render the motion of the balance unalterable by shocks is, as M. Bernoulli recommends, p. 39, of the Researches above cited, that it be of equal weight throughout. This is one of the considerations which has induced me to use two opposite spiral springs in my watch *, both opening and shutting together, in order that

* Sully used two spiral springs in his watch. See the *Description abrégée,* p. 175 and 177.—T. S. E.

each spring may have one-half less mass than if, being single, its height were double; and in order that, this mass being more equally distributed around the centre of motion, the regulator might not be exposed to gain or lose by lateral shocks. The advantages of this method have been confirmed to me by a number of experiments.

By means of these precautions we may turn the watch rapidly, or make it vibrate quickly on its suspension, without any sensible difference resulting in the arcs of vibrations.

There is but one foreign motion that can derange it; and that is the one which it may receive circularly on the axis of its regulator. But the machine being adapted to the vessel by means of four fastenings, fixed at the bottom of the box, it is impossible that it can receive any thing of the kind.

Moreover, I have thought it best (on the authority of what has been observed by M. l'abbé *Chappe*, and on what has been said by M. *Bouguer*, p. 214 of the *Manœuvre de Vaisseaux*, that *the inclination of a ship is much too great when it is from* 18 *to* 20 *degrees*,) to dispose my machine, not for inclinations which rarely take place, but for a mean term. It has therefore the liberty of describing on its suspension, and in its box, only 15 or 16 degrees; this may go as far as 18 or 20 by the giving way of the cushions and pad, if the weight of the watch press them any time.

[To be continued.]

XXV. *Chemical Examination of the native Cinnabars of Japan, Newmarktel, and Idria. By M.* Klaproth*.

I.

The cinnabar of Japan, which is brought to Europe in crystallized grains, is of a deep cochineal red, inclining to steel gray; and in other places it is of a scarlet red, inclining to brick colour.

There are fragments of flat hexaëdral prisms, outwardly very smooth, of a metallic lustre, inwardly very brilliant, and semi-metallic.

* From *Annales de Chimie*, tome lviii. p. 303.

The

The transverse fracture is scaly, the longitudinal one lamellated.

The fragments are irregularly angular and opake. Pyritous points are partly scattered over them, or rather they adhere to a quartzy matrix. The mineral is tender; its powder is of a scarlet colour; its specific gravity is 7·710.

A. 1000 grains were sublimed in a retort furnished with a globular receiver, in which a little water was put. The produce sublimed was exactly similar to artificial cinnabar. The water of the receiver, rendered turbid by some parts of sulphur, contained sulphuretted hydrogen gas and sulphurous acid in small quantity. The residue in the retort, weighing 38 grains, being digested with muriatic acid, the latter took up the iron coming from the pyrites and left the quartzy matrix.

B. *a.* 104 grains of mineral, which according to the above experiment contained 100 grains of cinnabar, were heated with 500 grains of muriatic acid, which disengaged from it sulphuretted hydrogen gas. 100 grains of nitric acid were successively added, which produced, with lively effervescence, the decomposition of the cinnabar, and the complete solution of the metallic parts.

b. The sulphur remaining, of a grayish yellow, had a viscous consistence; it weighed 11·8 grains. When burnt it left a blackish residue of 1·5 grains, which deducted from the preceding weight determines the quantity of sulphur at 10·3 grains.

c. The solution of cinnabar in nitro-muriatic acid was mixed with muriate of barytes. The precipitate, after having been made red hot, presented 30 grains of sulphate of barytes, which correspond with 4·2 grains of sulphur. Besides these 14·5 grains of sulphur, we may count a quarter of a grain of loss by the sulphuretted hydrogen gas; whence it results that 100 parts of pure cinnabar contain 14·75 of sulphur.

C. 1040 grains of cinnabar, which contain, according to the above experiment, 1000 grains of pure cinnabar, were distilled with half the weight of iron filings; the produce was 845 grains of mercury: thus cinnabar contains, not including the heterogeneous parts,

Mercury	- -	84·50
Sulphur	- -	14·75
		99·25

II.

Cinnabar of Newmarktel, in Carniola.

This ore is distinguished by its beauty from every other in Europe. The mineral is of a lively cochineal red. It is found in considerable masses, enveloped with a blackish gray chalk, traversed with veins of calcareous spar of a milky white. Its specific gravity is 8·160.

A. 100 grains were heated to ebullition with 500 grains of muriatic acid; 100 grains of nitric acid were afterwards successively added. After perfect solution there remained 10·20 grains of yellow sulphur, which burned without leaving any residue. Muriate of barytes produced in the solution 27 grains of sulphate of barytes, which correspond to 3·80 of sulphur. If the loss of the sulphur, which formed the sulphuretted hydrogen gas, amounts to a quarter of a grain, the quantity of sulphur in 100 parts of cinnabar ought to be 14·25 grains.

B. 500 grains of cinnabar mixed with half as much iron filings, and distilled, yielded 425 grains of mercury.

100 parts of the cinnabar analysed, therefore, contain:

Mercury	- -	85
Sulphur	- -	14·25
		99·25

III.

Hepatic Sulphuret of Mercury of Idria.

A. 1000 grains of cinnabar, distilled with half that quantity of iron filings, yielded 818 grains of pure mercury. The sulphuret of iron remaining was mixed with a black dust.

B. 100 grains were treated with the nitric and muriatic acids,

acids, and precisely the same phænomena were observed as in the preceding analysis of the cinnabar of Japan. Upon the combustion of the sulphur a black residue remained, consisting of three grains of charcoal, which left upon incineration one grain of reddish ashes. The quantity of sulphur obtained was 13·75.

C. *a.* 1000 grains of hepatic sulphuret were distilled in the chemico-pneumatic apparatus: 34 cubic inches of sulphuretted hydrogen gas passed over, without mentioning a part which was dissolved by the water of the receiver. 256 grains of pure cinnabar were sublimed, and the neck of the retort was coated with a mixture of humid ethiops and metallic globules, from which 317 grains of mercury were mechanically separated.

b. The residue in the retort was of a charcoal black; it weighed 39 grains. When incinerated it left 16 grains of a grayish powder, which ascertains the charcoal consumed to be 23 grains.

c. This earthy residue, treated by the muriatic acid, left 6½ grains of silex.

d. The muriatic solution, of a greenish yellow, was hypersaturated by ammonia, which produced a brownish precipitate: the liquor was of a clear blue.

e. The precipitate, treated by potash, left two grains of oxide of iron. The same alkaline liquor furnished 5½ grains of alumine by the muriate of ammonia.

f. Into this ammoniacal liquid, after having hypersaturated it with muriatic acid, a plate of zinc was dipped, which separated from it 0·20 of copper.

Result of the above Analysis.

Mercury - - -	818·
Sulphur - - -	137·50
Charcoal - - -	23·
Silex - - -	6·50
Alumine - - -	5·50
	990·50

 Oxide

Brought over	990·50
Oxide of iron - -	2·
Copper - - -	0·20
Water, which served for the formation of the sulphuretted hydrogen gas, and other loss - - -	7·30
	1000·

Those who are of opinion, with Messrs. Kirwan and Sage, that the mercury in this cinnabar is only partly combined with the sulphur, will see by this analysis that the two substances are in round numbers, as 1 and 6; and if there was any mercury not combined, the nitric acid would attack it. The idea entertained that this ore contained oxidated mercury besides cinnabar, may have arisen from the appearance of part of the mercury when distilling. But it arises entirely from the charcoal, which decomposes a part of the cinnabar, whether it takes from it its necessary quantity of oxygen, or forms at a high temperature carburetted sulphur. A distillation of artificial cinnabar with lamp-black absolutely presents the same phænomena.

Whether the mercury in the cinnabar is entirely exempt from oxygen, is a problem which is not yet resolved.

There is some appearance that the mercury exists in it in so very low a degree of oxidation, that it has hitherto escaped observation. In the examination of this subject it must not be lost sight of, that the mercury in cinnabar (analogous with some other metals oxidated at the *minimum*) resists the nitric acid; that in the making of cinnabar the passage of the ethiops to the state of cinnabar is always accompanied by an inflammation, and each inflammation seems to be an oxidation.

XXVI. *Upon*

XXVI. *Upon the Affinities of Bodies for Light; and particularly upon the refractive Powers of different Gases*.*

After some preliminary reflections upon the utility of studying the radical properties (if we may be allowed the expression) of different substances, taking them in the state of gas, on account of their greater simplicity in that form, and because they then belong to three sciences—physics, chemistry, and astronomy; the authors of the above memoir proceed to detail the object of their experiments, and their method of investigation.

We know that light, on passing from one transparent medium into another of a different density or chemical nature, experiences a deviation known in physics and astronomy by the name of *refraction*. Newton proved that this change of direction was owing to an attraction which bodies exercise upon the element of light, and which acting only at very small distances completely resembles the chemical affinities. The principal idea of the authors of the memoir was to employ the physical fact of the deviation of light in different gases, in order to study its connection with their chemical nature; they conceived the idea of applying this subtle element to the aëriform substances, nearly as the chemists apply their reagents to liquids; an ingenious idea, and which became very fertile in their hands.

The first and most interesting of the aëriform substances is, without doubt, that which envelops the terrestrial globe by the name of *atmosphere*, and preserves heat and life at its surface. It was also the first aëriform substance examined by the authors. Two general processes present themselves in studying the refractions of the common air: the one (astronomical) consists in comparing the positions of the stars affected by refraction with their true positions, that is to say, such as found by calculation: the difference is the effect of refraction. The other process is physical, and completely analogous to that in use for determining the refracting power

* Extracted from a memoir read at the National Institute by Messrs. Biot and Arrago.

of solids and liquids; they are disposed in the form of prisms, *i. e.* they are terminated by surfaces oblique from each other; and upon looking through them, we observe the deviation experienced by the rays of light, occasioned by their entrance and exit under a certain obliquity.

But how can we make a prism of air? In the same manner as a liquid prism, by inclosing air in any cavity, terminated, at the points where the light enters and departs, by transparent plates of glass, the respective inclination of which, as well as the refracting angle, is well known. If the apparatus is so constructed that what is called a vacuum in physics may be made and maintained round the glass plates in which the air is contained, the atmospheric air may then be considered as a prism; because, the essential conditions being the *difference* of the mediums which the light ought to traverse, and the obliquity of incidence, these are obtained if the hollow prism is left empty; or if it be filled with an aëriform substance different from the ambient air; or, lastly, with common air, but of a different density.

Newton induced Hawksby to pursue some experiments of this kind; but Hawksby's prism having only a very small refracting angle, and the means of observation of the visual angles being at that time very much below the degree of precision which may be obtained at present, these pursuits were left unfinished; and they could not have fallen into better hands.

At the suggestion of M. de la Place, the Institute charged the authors of the present memoir with the further development of the above pursuits; and in detailing their results they do becoming homage to the illustrious authors of the Static Chemistry and the *Mecanique Céleste*: "The subject of all our researches," they say, "was indicated in the above works, and the conversation and advice of their authors furnished us with the means of making our researches."

Borda had undertaken the same researches with instruments, to the perfection of which he had greatly contributed; he died, however, before having terminated his labours, and we have only recovered some slight traces of them. But his

his instruments and his method still exist, and the authors of the memoir availed themselves of them; adding thereto such improvements as were suggested by their own genius and the progress of science.

They employed the same prism as that of Borda. It is a brass tube, terminated by two planes of glass carefully luted to the tube, oblique to its axis, and making between them an angle of 143° 7′ 28″ (sexagesimal degrees), determined by processes which ascertained it within a few seconds. This tube is sustained by a support with a stop-cock, by means of which the vacuum is made in its interior by an air-pump. It is furnished with a barometer communicating with the interior, and acting the part of a proof to ascertain the degree of vacuum made in it. In the results an account was kept of a very small error in the parallelisms in the two surfaces of each of the glasses at the ends of the tube; an error which was discovered by observations, and which only rose to 16·6″: this quantity is constantly added to the angles observed; it is but a very trifling portion of the total deviation, which rises to 362·6″.

In order to determine this deviation the following process was resorted to:—The apparatus, being placed on the top of the Palace of the Senate (formerly the Luxembourg), immediately behind it was placed a repeating circle, the lower eye-glass of which was constantly pointed to the thunder-rod of the observatory, 1400 metres distant. The lower eye-glass was directed upon the same object, but traversing the prism; and the visual ray was affected with all the deviation due to the refraction. The angular quantity of it was very exactly observed; then, after having half turned the prism upon its vertical axis (being on a horizontal plane), which this turning had doubled, it was again observed, and was found so great, that the bent ray passed from one extremity to the other of the façade of the observatory. This whole angle was observed a great number of times, in order to take advantage of the characteristic property of the repeating circle; and thus a perfect exactitude was obtained. An account was always kept of the variations of the barometer and of the hygrometer during the operations.

The

The barometer connected with the interior of the prism, compared with that in the open air, always gave the exact account of the density of the contained fluid compared with that of the ambient air. The prism was filled at pleasure with any given gas, by previously exhausting it of air and then connecting it with a reservoir filled with the gas, which was ascertained to be the purest of its kind by several precautions. This apparatus was constructed by M. Fortin, to whom the authors pay the compliment of having fulfilled their expectations in point of convenience and precision in his execution of that apparatus.

The specific gravity of the gases employed was an essential requisite in this kind of experiments. The authors ascertained it by the common process of weighing a globe of glass previously emptied, and afterwards filled with the gas: all the corrections were made for the state of the barometer, the thermometer, and even of the hygrometer; because, the vapour of the water being lighter than the air, as 10 to 14, at equal pressure, it was necessary to have regard to this circumstance, and it was done according to a formula which M. de la Place deduced from the experiments of De Saussure and Dalton. All the weights were reduced to what they had been in the vacuum at the freezing point, and under the constant pressure of 0·76 of a metre. Regard was even paid to the dilatation of the glass, supposed equal to 0·0000262716 of its volume for each degree of the centigrade thermometer; a result discovered by Messrs. Lavoisier and De la Place in a work upon the dilatation of solids, which, unfortunately, has not been published. With all these precautions, the results obtained on different days, and in very different states of the air, scarcely deviated a few milligrammes from each other, when they were reduced to the same temperature and the same pressure.

In order exactly to compare the weights of the gases with that of water, the authors gauged very exactly the globe with pure water at the temperature of 4° (39·2° Fahr.), the point at which the greatest condensation takes place, and at which the absolute weight of water was very precisely determined by experiments made in France for the determination of the

gramme.

gramme. The mercury was weighed with great precaution, in order to compare its weight with that of water; and setting out with that of the air, a relation upon which several useful results rest, and particularly the measure of heights by the barometer, the metal was introduced into a matrass with a straight beak, and from which all air was carefully expelled by the ebullition of the mercury, and by afterwards putting it under the receiver of the air-pump. The same precautions were taken with the water; all the corrections were made in respect to the temperature and the density of the atmospheric air, as well as for the dilatation of the vessels; and there was found for the final relation of the weight of the mercury and of the air, *i. e.* for the constant coefficient to employ in the calculations of the measurement of heights by the barometer expressed in metres, the number 18332, at the freezing point and at the pressure of 0·76 of a metre.

We know that this same coefficient may be obtained by a very different method, *i. e.* by comparing the barometric heights observed in the open air in stations differently elevated, with the real differences of height of the same stations determined by levelling or by trigonometrical measures. De Lucs, Schuckburgh, Trembley, and other men of science, have proceeded in this manner; but no person has followed up this part of the research, or fathomed the different elements of it with more sagacity than M. Ramond, whose conclusions we shall shortly state.

The coefficient indicated by M. Ramond as the most convenient to apply to observations made in the open air is = 18336, and differs only by four unities of the fifth order of decimals, from that which the authors concluded from their experiments in close vessels; an agreement which forms a presumption very favourable to the exactitude of this fundamental determination. M. Ramond has also shown that this coefficient, introduced into the formula of M. de la Place, would give the heights of mountains in a manner so nearly corresponding, that the uncertainty which remains is within the limits of those errors of which the minutest and most accurate observations are susceptible.

The specific gravity of each of the gases employed may

 be

be ascertained, in some measure, from their purity; but the authors did not content themselves with this, and the gases were always submitted to chemical analysis. Messrs. Thenard, and Berthollet junior, assisted their friends in this part of their labour; and their names inspire confidence. It became still necessary that these gases, although produced very pure, should be introduced with all their purity into the prism of the experiment. The authors succeeded in this essential requisite by a particular disposition of the cocks of communication, difficult to be understood without the assistance of drawings.

"Knowing," they say, "the specific weights of our gases, and the refractions exercised by them upon the light, we concluded, by calculation, their refractive power compared with that of the atmospheric air.

"What we mean here by the term *refractive power,* is not only the deviation produced upon the luminous ray, nor is it the angle measured by this deviation. If the function of the distance which expresses the action of bodies upon the light was of the same form for all bodies, and did not differ relatively to each of them but by the produce of their density, and of a constant coefficient dependent on their nature, the quantity, which we call the *refractive power* of a body, would be proportional to the intensity of its attractive force for the light; but in every case it is the sum of all the actions exercised by the body multiplied by the element of space and by the density. These rigorously exact notions are conformable with the principles given by Newton and by the author of the *Mecanique Céleste:* it appeared necessary to recall them to observation, because it is only by attaching precise ideas to principles that we can employ them, and follow with certainty the consequences which may be deduced from them."

By applying these principles to their experiments the authors of the memoir have established the refractive power of various aëriform fluids. Oxygen of all these fluids, and indeed of all natural bodies yet observed, refracts the least, and hydrogen refracts the most,—it refracts $6\frac{1}{4}$ times more than common air.

Newton

Newton discovered in the refracting power of the diamond what suggested to him that it was combustible; and M. de la Place announced, in a printed memoir, the great refractive power of hydrogen. The refracting power of the other gases is intermediate between those of hydrogen and oxygen. That of the muriatic acid gas was tried, notwithstanding the great difficulties attending its manipulation, and it was found to surpass that of the atmospheric air.

But it is the study of the refracting power of this latter fluid, and of its modifications on account of its density, that ought particularly to occupy philosophers, since the rays of light never reach us, except more or less turned from the right line by this influence. We might vary at pleasure the density of the common air inclosed in the apparatus; that is to say, produce a given difference between this medium and the external air, on leaving the one and the other identical in their nature, and only changing the element of density. When we have brought the density of the interior and exterior air to the term of equality, there will remain no more refrangent cause than the error in the parallelism of the two surfaces of each glass; and it is thus the absolute quantity of this correction was determined, which did not reach 17 seconds of a degree. This series of observations made upon the atmospheric air and upon the other gases, taken at different densities, and always marking the state of the barometer, thermometer, and hygrometer, afforded the authors of the memoir a conviction of this important principle in the theory of refractions, namely, that from the most perfect vacuum to the ordinary degree of pressure of the atmosphere, the refraction of the light in any given gas is always proportional to its density, without this rule having any occasion for the slightest modification.

The authors propose to examine, by future experiments, the influence of a temperature more or less elevated, and of the condensation of the air beyond the ordinary atmospherical limits upon the refractive power of this fluid.

It did not appear to them that the state of the hygrometer had

had any appreciable influence upon the refractive power when the sky is clear, and the air transparent. They propose to themselves further experiments upon this subject, and upon the refracting power of vapours; and as these experiments require high temperatures, they are obliged to wait for the great heats in summer.

In order to reduce the gases to the same pressure and the same temperature in the calculation of the results, the authors made use of the law laid down by Guy-Lussac, namely, that by equal increases of heat there is the same law for all the gases, and that the dilatation of each is equal to 0·00375 of its volume for each degree of the centigrade thermometer. This number was determined by 25 experiments, which did not sensibly differ from one another, and were made with tubes perfectly dry, and of exact calibres. "This result," the authors say, "as given by Guy-Lussac, is one of the most useful in physics; it serves at the same time chemists, and even astronomers, to reduce their observations. It is probable that by adding the specific gravities of the gases and their refractive powers, as given in this memoir, an exact and complete knowledge will be obtained of all the physical properties of aëriform fluids."

We reserve for a future number the consequences deduced by the authors of the memoir from their experiments, and the development of the relations in which these results may be interesting to chemistry.

[To be continued.]

XXVII. *Memoir upon living and fossil Elephants.* By G. Cuvier*.

The fossil ossifications of elephants have excited not only the most lively interest in the minds of men of science, but also the curiosity of the vulgar in a high degree. Their enormous size has caused them to be collected and preserved wherever they were found; their frequency in every climate,

* From *Annales du Muséum d'Histoire Naturelle*, année iv. cahier vii. p. 1.

and

and even in places where they could not subsist at present, has struck every one with astonishment, and has given rise to a number of hypotheses in order to account for them: but it would have been highly proper if as much activity had been shown in determining the conditions and the nature of the problem, as there has been in resolving it; and perhaps this very negligence in fixing the bases, and even the terms of the question, has been one of the causes why the most of these solutions have been so unfortunate. It must be admitted, at least, that we have been rather too late in occupying ourselves with questions which ought to have been answered before trying our strength in resolving the problem.

Are the elephants of the present day of the same species? Supposing that there are several species, are the fossil elephants of different countries indiscriminately of all these different species? or rather, Are they divided into different countries, according to their species? or are they of species which are now lost?

It is evident that we can say nothing demonstrable upon the problem, before resolving all the preliminary questions; and we are as yet hardly in possession of the elements necessary for the solution of some of them.

The osteologies of the elephant hitherto published are so little detailed, that we cannot as yet say of several of them if they belong to the osteology of any of our living elephants; and of the innumerable quantity of fossil ossifications so much talked of by authors, we have scarcely obtained passable drawings of two or three. Daubenton, who had an African skeleton before his eyes, perceived none of the enormous differences of its grinders from those of a fossil skeleton; and he confounds a fossil thigh of the animal of the Ohio with that of the elephant. The comparisons made by Tentzelius, by Pallas, and so many others, of fossil bones with fresh ones, were never expressed, except in general terms; and were never accompanied with exact drawings, rigorous measurements, nor those abundant details which such important inquiries necessarily require.

The

The figure published by Allen Moulin * is copied into the *Elephantography* of Hartenfels, and into the *Amphitheatrum Zootomicum* of Valentin; but it is so badly executed that nothing precise can be learned from it, nor even the species to which it belongs.

That of Patrick Blair † belongs, it is true, to the species of the Indies; but although designed after a very young animal, the epiphyses of which were not cemented, it is very badly done. The shoulder blades are wrong placed, while six toes are given to the left fore-foot and only four to the hind ones, &c.

Those of Perrault ‡ and Daubenton § were both made from a skeleton, still in preservation, of the African species. The first of these is well enough, but the head is represented as too small; the second is not above mediocrity.

That of Camper ‖ is rather of the Indian species, like that of Blair; but although better designed than the others, it is done from a very young animal which had not acquired its full form, and from the bones of which the ligaments had not been removed.

I am now occupied, along with M. Huet, in the execution of some grand drawings on the anatomy of the elephant, which, I hope, will tend much to increase our knowledge of that branch of science when they are given to the public. In the mean time a detail of some of the observations I have made cannot fail to be interesting, particularly such of them as regard the increase and structure of the teeth. What I have to remark on that subject, however necessary to the history of fossils, is of a still more general importance in another respect, as explaining the history of the teeth in men and animals; and the immense size of the teeth of an

* Anatomical Account of the Elephant accidentally burnt in Dublin, &c. London 1682, 72 pp. 4to. with two engravings.

† Phil. Trans. vol. xxvii. no. 326, June 1710. Plate II.

‡ Mem. pour servir à l'Hist. des Anim. iii partie, Plate XXIII. It was published in 1734.

§ Hist. Nat. in 4to. tome xi. Plate IV.

‖ Descrip. Anat. d'un Elephant.

elephant

elephant renders things visible which it is extremely difficult to distinguish in those of men or animals.

But in the first place, according to my usual method, I shall describe those places where fossil ossifications have been found of that species which forms the principal subject of my present researches.

To describe every place where they have been discovered would be an endless task; suffice it to say, that every country, and every æra, has furnished fossil bones.

Traces of them are discovered in the writings of the antients. Theophrastus speaks of them in a work no longer in existence, of which Pliny has given us his testimony:—"Theophrastus autor est, et ebur fossile candido et nigro colore inveniri, et ossa è terrâ nasci, invenirique lapides osseos." Lib. xxxvi. cap. 18.

It is probable that the bones of elephants have been often taken for human ones; and this may have given rise to the pretended discoveries of the tombs of giants spoken of in antiquity.

Of this number the bones discovered in digging a well at Tegea certainly were, although taken for the bones of Orestes*; and also those seen at Caprea, according to Suetonius, and regarded as the bones of heroes or giants †.

As to the stories of bodies still larger, such as that of a skeleton 46 cubits in length thrown out of the ground during an earthquake at Crete, and regarded as that of Entellus or Otus ‡; or that of another, of 60 cubits, dug up near Tingis in Mauritania, when Sertorius commanded there, and which was taken for the skeleton of Antæus §; these accounts are certainly exaggerated, or else they have for their origin the skeletons of whales. Strabo, who relates the latter story on the authority of Gabinius, does not hesitate to regard it as fabulous.

These erroneous ideas, which arise from a total ignorance

* Herodot. lib. i. § 68.

† Suet. Aug. § 72.

‡ Plin. lib. vii. cap. 16.

§ Strabo, Geogr. lib. xvii. Amsterdam edit. 1707. p. 1185.

of anatomy, lasted to the middle age: even at that period mention is made of giants, and the descriptions of their bones are sometimes so exaggerated that they make them ten times larger than those of the largest elephants.

As more correct ideas have now dissipated these chimeras, one would think that the elephants whose bones were discovered had been buried by human beings. Thus, so far as these discoveries are confined to Italy, and those countries frequented by the Macedonians, the Carthaginians, and the Romans, they may be reasonably accounted for by reflecting on the prodigious number of elephants possessed by these people.

We know that the first Europeans who had elephants were Alexander and his Macedonians after the defeat of Porus *; and on that occasion some excellent notions were furnished by Aristotle on the subject of these animals: after the death of Alexander, Antigonus possessed the greatest number of elephants †. The Seleucides maintained them always, particularly after Seleucus Nicator received fifty of them from Sandro-Cottus in exchange for a whole canton on the banks of the Indus ‡. Pyrrhus was the first who brought them into Italy in the year of Rome 472 §; and as he disembarked at Tarentum the Romans gave these animals, which were then unknown to them, the name of Lucanian bulls. They were in very small numbers, and Pyrrhus had taken them from Demetrius. Curius Dentatus captured four of those from Pyrrhus, and brought them to Rome to grace his triumph. These were the first that were seen by the Romans; but they soon became quite common. Metellus, having conquered the Carthaginians in Sicily, in the year of Rome 502, conducted their elephants to Rome upon rafts, to the number of 120 according to Seneca, and 142 according to Pliny ‖: these were all massacred in the Circus. Hannibal also brought elephants with him into Italy. Claudius Pulcher and Lucullus introduced them to combat with

* Pausanias, Attic. lib. i. † Id. ib. ‡ Strabo, lib. xv.
§ Plin. viii. c. 6. ‖ Id. ib.

bulls

bulls in the Circus. Pompey had them yoked to his car at his triumph for Africa. Germanicus exhibited some which danced ludicrously; and under Nero, at the games which he gave in honour of his mother, they were exhibited dancing upon a rope, and performing a number of feats of activity. Ælian says expressly of those belonging to Germanicus, that they were elephants born at Rome; and they must have consequently propagated there:

"Cum Tiberii Cæsaris Nepos Germanicus gladiatorum spectaculum edidit, plures jam grandes utriusque sexus elephanti Romæ erant, è quibus alii plerique generati extiterunt: quorum artus interea dum committebantur et confirmabantur, et membra infirma conglutinabantur, peritus vir ad pertractandos eorum sensus animosque mirabili quodam disciplinæ genere eos erudiebat." Ælian. de Anim. lib. ii. cap. 11.

Columella asserts the same fact still more positively:—"India perhibetur molibus ferarum mirabilis: pares tamen in hac terra (Italia) vastitate belluas progenerari quis neget, cum inter mœnia nostra natos animadvertamus elephantes?" Col. de Re Rustic. lib. iii. cap. 8.

If our naturalists had paid attention to these two passages, they would not have credited so long the impossibility of domesticating the elephant, and they would have perhaps tried a little sooner the experiments which have succeeded so well when made by M. Corse on that animal.

Thus, though Italy presents a great quantity of fossil bones, they were for a long time attributed to such individual animals as had been brought there by mankind; some of them only, however, may be attributed to this cause.

The following is an account of the principal places in Italy where fossil bones have been found; but we are far from regarding it as complete:

The largest tusk was found by Messrs. Larochefoucauld and Desmarets near Rome: it was ten feet long, and eight inches diameter, although it was not entire*. We have

* Buffon, Epoques de la Nat. Notes justif. 9.

four pieces of it in our museum, which are very much altered. Some bones were also found at Rome since 1664, in digging some foundations at the entrance of the Vatican. Thomas Bartholin speaks of even prior discoveries made at Rome *; and it is probable that the supposed body of Evander, found in 1041 or 1054 †, was nothing else than elephants' bones.

M. Charles Louis Morozzo ‡ gives a drawing of a jawbone found in April 1802 in a vineyard without the gate Del Popolo, with several other fragments of bones and ivory. Bonanni speaks of several large bones, teeth, and lower jaws, dug up in his time near a castle named Guidi, on the Aurelian Way, twelve miles from Rome §.

The vale of Arno seems to abound with fossil bones. The grand duke Ferdinand of Medicis had an entire skeleton dug up in 1663 in the plain of Arezzo.

Doctor Targioni Tozetti deposited in the museum at Florence a humerus found in the caves near the upper valley of Arno, and to which oysters were attached.

Dolomieu says that the elephant bones of the vale of Arno are found at the bottom of hillocks of argil which fill the intervals of the calcareous strata; that the beds which contain them are filled with pieces of wood, some of them petrified and some bituminous, which he takes to be oak, and which are covered with beds of marine shells and immense banks of argil ||.

There is a remarkable depôt of bones upon Mount Serbaro, three leagues from Verona: they seem to be those of several other animals besides elephants.

Fortis has given a description of this depôt in his Natural History of Italy.

Among the elephant bones there was a tusk more than

* De Unicornu.

† Dom. Calmet, Dict. de la Bible, ii. 160.

‡ Mem. de la Societé Ital. tom. x. p. 162; and Journal de Phys. tom. liv. p. 443.

§ Mus. Kircher. p. 200.

|| Journal de Physique, tom. xxxix. p. 315.

nine

nine inches diameter, and which must have been at least 12 feet long. M. de Gazola has sent to our museum the half of a lower jaw and a metacarpal bone, which must have belonged to an animal 15 feet high.

Piedmont has furnished plenty of bones: I lately received for our museum, from M. Giorna, two large pieces of jaw-bones which were in the cabinet of Natural History at Turin. This gentleman informs me that there is also an elephant's *femur* in that cabinet.

Abbé Nazari speaks of a skeleton at least 18 feet long, dug up in 1665 at Toriolo, in Upper Calabria. It is, indeed, said that it resembles that of a human being; but we know how little credit this comparison is now entitled to.

Sicily possesses these curiosities in abundance. Two skeletons were discovered in the 14th and 16th centuries at Trapani and Palermo, and are described by the writers of that age as being those of giants. The account of one of them is shamefully exaggerated; it is described as being 300 feet high: but Kircher, who visited the cavern out of which it was said to have been dug, positively asserts that it could not have been more than 30 feet high.

Kircher mentions three other giant skeletons found in Sicily; but, as usual, almost all the bones were consumed, except the teeth *.

The state of oppression under which modern Greece has always groaned, does not admit of any correct anatomical accounts of the fossils it contains; but that country, also, has had its animal giants.

In 1691 there was found, about six leagues from Thessalonica, some bones which admitted the arm of a man into their cavities: one lower jaw was 7½ inches high, and weighed 15 pounds. Three other teeth weighed from two to three pounds each. The humerus was two feet eight inches in circumference.

Suidas speaks of giants' bones found in great quantities under the church of Saint Mena, at Constantinople; and

* Mund. Subterr. lib. viii. § 2. cap. iv. p. 59.

 adds,

adds, that the emperor Anastasius caused them to be deposited in his palace.

Some large bones were lately found at Demotica; and there is a jaw from the island of Cerigo deposited in the cabinet of Morosini, at Venice.

Although we scarcely ever heard of France having possessed any elephants at any period, yet fossil skeletons of these animals are not less numerous there than in any other country.

It is now pretty clear that the giant skeleton, said to have been found in 1456, in the reign of Charles VII., near Valence, was that of an elephant. It is also probable that the bones dug up near Valence, under Louis XI., were of the same description. They were said to belong to an animal 18 feet long.

It was also in Dauphiné that the skeleton was discovered which of all others has given rise to most controversy. The numerous pamphlets which were published on the subject throw little light on it.

From what we can judge at this distance of time, it appears that in 1613 some large bones were found in a sand-pit near the castle of Chaumont.

A surgeon of Beaurepaire, named Mazurier, exhibited these bones at Paris for money; and in order to excite greater curiosity, he distributed a small pamphlet, where he asserted that they were found in a sepulchre 30 feet long, upon which was inscribed "*Teutobochus rex.*" We know that this was the name of the king of the Cimbri, who fought against Marius. But Mazurier was accused of forging this inscription; and he does not seem to have justified himself from the imposture.

As for the bones he exhibited, they consisted of the following pieces:

1. Two pieces of the lower jaw, one weighing six pounds, containing two grinders and the cavities for two others; and a larger, weighing twelve pounds, with one whole and three broken teeth. Each tooth had four roots, and was as large as the foot of a bull; and they were petrified, and of the colour of gun flint.

2. Two

2. Two vertebræ, into the medullary canal of which a man might introduce his fist.

3. A piece of a rib six inches long, four broad, and two thick.

4. A fragment of the shoulder-blade, the articulary facette of which was twelve inches long, and eight broad.

5. A head of a humerus, as large as a man's head.

6. A femur, five feet long, and three feet round at the top; the trochanters were wanting. The neck had neither the length nor the obliquity of a human femur.

7. A tibia, nearly four feet long, and more than two feet round at the bottom.

8. An astragal, different from that of domestic animals.

9. Lastly, a calcaneum, which had facettes below for the scaphoid and the cuboid, but the posterior apophysis or tuberosity of it was not so strong as that of a man.

This posterior extremity was certainly that of an elephant; there is no other large animal, the astragal of which resembles that of a man so much as to be mistaken for the human astragal; but the teeth cannot be those of an elephant: that animal never has so many, nor have they any roots. It appears, therefore, probable that elephant and rhinoceros bones were buried promiscuously together in the place where the above bones were found.

The nearer we approach to our own times, the descriptions of these bones become more reasonable. A true elephant's jaw has been described by M. de la Tourette in the ninth volume of *Savans Etrangeres de l'Academie des Sciences*, p. 747. It was found in 1760, at St. Valier, near the Rhone, and 80 feet above the level of that river, in a gravelly soil mixed with flint.

M. Faujas describes a tusk found by M. Lavalette in the commune of Arbres, near Villeneuve de Berg, in the department of Ardeche, at the foot of the Corion mountains, and five feet deep, in a volcanic mass*.

M. Cordier, engineer of the mines, has furnished me with a remark on this position, which he carefully examined.

* Annales du Muséum d'Hist. Nat. tome ii. p. 24.

The tusk was encrusted in the heart of a solid volcanic breach, which not only forms the summit of the hill of Arbres, but stretches in horizontal strata under all the Coirons, of which it forms the base. M. Cordier knows several other places where the fossil bones are enveloped in volcanic matters.

On approaching the Pyrenees, great collections of bones are met with. The Black Mountain in particular contains a great quantity.

If we turn towards the north, they are not less abundant. There is in the Museum a piece of a shoulder-blade, discovered near Chalons-sur-Saone. The labourers employed on the grand centre canal have recently discovered a depôt of bones in the same province. By means of M. Gerardin, in the employment of the Museum, I have received a very large jaw from this depôt. There was a rhinoceros jaw found near it.

The environs of Paris abound with fossil ossifications. In digging the canal which brings the waters of the Ourcq to Paris, two tusks and two jaws were dug up, larger than any I ever saw. M. Girard, the director of this canal, sent them to me for the purpose of being deposited in the museum.

As I carefully examined the ground where they were found, along with M. Girard, and M. Alexander Brogniard the mineralogist, I think a short description of it here will be extremely acceptable.

The canal is dug in the plain of Pantin and Bondy, the soil of which is 70 or 80 feet above the level of the Seine, and which embraces the foot of the gypsous hills of Montmartre and Belleville. This plain is formed to the depth of 40 feet of different layers of sand, marle, and clay; but no calcareous stone has been met with, although there is plenty of it at the level of the river at St. Ouen. The canal in some places passes through beds of gypsum. In some places the beds of marle and clay are hollowed, as if they formed basons or pits filled with foreign matters. There are, in fact, at these places heaps of blackish earth, which fill these crevices, and which are covered in their turn by yellowish sand.

It was in this blackish earth, at the depth of 18 feet, that the elephant's jaws and teeth were found. There was also a skull found, but it was broken by the workmen, and I am in possession of the pieces of it, as well as other bones of the ox kind, and, in particular, a very remarkable skull of a large and unknown species of antelope, which I shall describe afterwards. The upper yellow sand contains plenty of fresh-water shells; but the black earth only contains green clay and yellow marle. The ivory is much decomposed; the jaws less so, and the other bones not at all. The most part of them, indeed, do not seem to have been rolled about at all.

[To be continued.]

XXVIII. *Letter of Dr.* De Carro *to Professor* Pictet *of Geneva, on the Mineralogy of the Island of Ceylon**.

Vienna, Feb. 17, 1806.

Having requested from the governor of the English settlement at Ceylon, the favour of some information upon the mineralogy of that island, accompanied if possible with some specimens of minerals, his excellency governor North had the goodness to transmit to me the subsequent note of M. Jouville, the only mineralogist in the island.

"Columbo, July 5, 1805.

"The only person who is occupied in the mineralogical department of Ceylon has not yet discovered the gems in their matrices. All those he ever saw were found in currents. There are no others in the market. It is very rare to meet with well preserved and determined crystallizations; we saw, however, the oriental stone corundum in the state of a lengthened pyramid, and the spinel rubies in very small octaëdral crystals. The tourmaline and the schorl are also

* From *Bibl. Britan. des Sciences et Arts*, vol. xxxii. p. 281.

found

found in very good preservation: but even these stones have never yet been found in the matrix in which they had been formed. It is easy to explain why the Europeans are not permitted to penetrate into the interior of the country belonging to the king of Candy, where the high mountains are from which the rivers flow that furnish the precious gems; but even if access were permitted, still great difficulties stand in the way of any researches. These difficulties principally arise from the great strength of vegetation through all the island. The mountains are every where covered with thick woods, which it is almost impossible to penetrate. No crevices or breaks whatever are any where to be seen, which may direct the mineralogist in his researches. Nevertheless, it may not be altogether impossible to discover some naked crevices, which, when well examined, may lead to the discovery of the gems in their matrices. The Candians themselves, however, will never submit to the risks and trouble experienced by those who work mines of precious stones or metals, and they content themselves with raking them up at great expense in the beds of currents, after the rainy season.

"We have never seen primitive granite or any porphyry at Ceylon, although we penetrated to the city of Candy, situated nearly in the middle of the island. The apparent base (i. e. so far as is visible) of the mountains is gneiss, or secondary granite, sometimes in layers and sometimes in rows, some feet thick. It sometimes contains large plates of mica two or three inches in diameter, and often no mica at all. The feld spar is often very abundant, but never in a state of determined crystallization. The white and milky quartzes are very common, and sometimes in masses of two or three feet. Rock crystal is found at Candy in large and clear pieces; we never found it ourselves, however, in that state.

"The stone which covers the gneiss towards the sea-coast, and often several miles in the interior, is an argillaceous compound, friable, and susceptible of being diluted in water in a great measure; it sometimes gives signs of effervescence

fervescence by the action of the acids, and contains a good deal of black and red oxide of iron: this friable stone is called *kaboc*, and it is employed in building.

" This is all we can say in answer to the questions upon the mineralogy of Ceylon. It is with much regret that we cannot better satisfy the curiosity of the learned on the subject: indeed, we have not yet been able to gratify our own, notwithstanding a stay of nearly seven years in the island.

" Before concluding this short notice, we shall hazard a conjecture, which to us has the appearance of probability at least: it is, that the gems are formed in argillaceous veins of secondary matters. If these stones were attached to the rocks, is it not probable that they would be sometimes found in the torrents attached to fragments of a stony nature? This is what we have never seen, although we have been often present at the rakings made by the natives in the beds of torrents."

XXIX. *On Music.* *By Mr.* JOHN FAREY.

To Mr. Tilloch.

SIR,

I HAVE carefully perused the folio treatise on the *Theory of Music*, &c. by Mr. William Hawkes, published in 1805 by Clementi and Co. of Cheapside, which you obligingly sent me a few days ago; which, though it may prove a useful work to theoretical musicians, contains little of novelty, except its engraved examples, but what is to be found in an anonymous pamphlet, price 1*s.* (perhaps from the pen of the same author), entitled *A Treatise on the Theory and Practical System of Music*, published by Cawthorne in 1798. The object of both of these works is, to recommend a temperament of the diatonic scale on keyed instruments, as organs, harpsichords, piano-fortes, &c., in which *each ascending* FIFTH *is flattened by one-fifth of a comma* as the instrument is tuned, except that the fifth above ♭E and the fifth

fifth below * G are directed to be tuned *perfect*; but why these anomalies in the system are introduced I am at a loss to guess, especially as * G is thereby made $\frac{1}{3}$ c the worse by it.

It is to be lamented by those who study the philosophy of musical sounds, that among the various ways in which musical intervals can be expressed, no one mode has yet been generally adopted by the writers on this subject, particularly by those who treat on the *temperament*, or deviation from truth and nature, which is necessary for adapting the harmonic intervals to our imperfect instruments and common notation of music, wherein only 12 intervals of sound are admitted in an octave; and this occasions the necessity to the student, on the appearance of every new scheme of temperament, for reducing the intervals resulting therefrom to some one standard or measure, before he can compare or judge of its merits and defects. Among the several ways of expressing these intervals, none is so general, or convenient, as the logarithms of their corresponding ratios; and, in order to save the readers of Mr. Hawkes's Treatise, and of your Magazine, a repetition of the trouble which I took in 1798, as also to compare this with the *Stanhope temperament*, described in your October number, and with Dr. *Thomas Young's* progressive temperament: Phil. Trans. 1800; Supplement to Ency. Brit. 3d edit. ii. 663; or Young's Syllabus, p. 95, I beg to present

A Table

A Table of Musical Intervals as adapted to Keyed Instruments.

Intervals in half Notes.	Letters.	Marks.	Ratios	Logarithms.	Logarithms.	Differences from Diatonic.	Differences in 5ths of a Comma.	Logarithms of Lord Stanhope's System.	Logarithms of Dr. T. Young's System.
			Diatonic System.		William Hawkes's System.				
12	c	VIII.	$\frac{1}{2}$	.6989700	.6989700	- -	- -	.6989700	.69897
11	B	VII.	$\frac{8}{15}$	.7269987	.7269987	- -	- -	.7269987	.72610
10	♭ B	7th.	$\frac{128}{225}$	.7550275	.7479646	.0070629	+ $1\frac{3}{10}$ c	.7501225	.74921
9	A	VI.	$\frac{3}{5}$	.7781513	.7759933	.0021580	+ $\frac{2}{5}$ c	.7763529	.77580
8	* G	6th.	$\frac{16}{25}$	.8061800	.8029430	.0032370	+ $\frac{3}{5}$ c	.8010300	.80036
7	G	V.	$\frac{2}{3}$	.8239087	.8249877	.0010790	— $\frac{1}{5}$ c	.8239087	.82492
6	* F	IV.	$\frac{32}{45}$	.8519376	.8530165	.0010790	— $\frac{1}{5}$ c	.8521825	.85151
5	F	4th.	$\frac{3}{4}$	.8750613	.8739823	.0010790	+ $\frac{1}{5}$ c	.8750613	.87461
4	E	III.	$\frac{4}{5}$	.9030900	.9020110	.0010790	+ $\frac{1}{5}$ c	.9030900	.90174
3	♭ E	3d.	$\frac{64}{75}$	.9311187	.9240558	.0070629	— $1\frac{3}{10}$ c	.9259687	.92330
2	D	II.	$\frac{8}{9}$	.9488475	.9510055	.0021580	— $\frac{2}{5}$ c	.9506458	.95087
1	* C	2d.	$\frac{128}{135}$	.9768762	.9790342	.0021580	— $\frac{2}{5}$ c	.9771212	.97645
	C	Key.	1	.0000000	.0000000	- -	- -	.0000000	.00000

The

The established practice of musicians in reading the musical notes upwards, makes it more convenient, in computing and using tables like the above, to follow the same order; which is the reason that the title of the table is at bottom instead of the top; and it is to be read, and its intervals reckoned, upwards. It is necessary here to remark, respecting the 4th and 5th columns entitled the *Diatonic System,* that this term has too frequently been restricted by authors to the ratios set against the notes C, D, E, F, G, A, B and c, respectively; while others, who were writing on a temperament of the scale, have introduced ratios answering to ♯ C, ♭ E, ♯ F, ♯ G, and ♭ B: these in the above table constitute the *chromatic octave,* example No. 4, in Mr. Hawkes's treatise. But the student must not be surprised, on looking into different authors who treat on the scale of music, to find the finger-key ♯C bearing a ratio of $\frac{15}{16}$, or even $\frac{9}{10}$; ♭ E $\frac{5}{6}$, or $\frac{27}{32}$; ♯ F $\frac{45}{64}$; ♯ G $\frac{5}{8}$; and ♯ A $\frac{5}{9}$, or $\frac{9}{16}$, instead of those in the above table, and yet to find the same denominated diatonic intervals; because, on consulting Maxwell's *Essay on Tune,* a work of great merit, printed in Scotland, (which was advertised in 1794, and a few copies vended in London, with a spurious title-page and date,) he will find that 56 notes or intervals, at the least, are required in each octave of the diatonic system to render modulation into each of the twelve finger-keys, major and minor, practicable without false intervals, or such as a good ear would pronounce to be out of tune. Indeed, the diatonic system, when limited to the seven notes, C, D, E, F, G, A, and B, has but little of the perfection in practice, which is usually ascribed to it; for, except in sounding the notes E, F, G, and A, with their fundamental note C, if the base note moves into D, E, F, G, &c., it will be found that the harmonies marked in the piece of music to accompany it, are but few of them to be found among these seven notes, or even among the five intermediate or half notes, which are set down by the authors above alluded to, except Maxwell; but a false note, or one differing in an offensive degree from the true one, must be substituted for it.

By adding the logarithm of the octave .6989700, successively to those of the notes ♯ C, D, ♭ E, &c. in 5th,

6th,

6th, 9th, or 10th columns of the above table (rejecting or borrowing 1· when necessary) the logarithms of * c, d, b d, &c. in the next octave above will be obtained: for example, if to .9030900 the diatonic E, .6989700 be added, we have e = .6020600; which ought to be a true Vth to A, and such we accordingly find it, on adding the logarithm of a Vth, or .8239087, to that of .7781513 = A. The true, or harmonic 3d, is $\frac{4}{5}$ = .9208188; this deducted from E = .9030900, gives * C = .9822712, differing 53950, or a comma, = $\frac{81}{80}$, from * C in the table. The true 6th is $\frac{5}{8}$ = ·7958800, which added to b E = ·9311187 gives B = ·7269987, as we find it to be in the table. In this manner may the curious reader examine all the harmonies, or any particular ones, in the systems contained in the above or any similar tables, and decide on the pretensions to truth and exclusive advantages claimed, by the advocates for each system.

Mr. Hawkes, in the work before us, has left his readers without any directions for tuning *fifths* $\frac{1}{3}$ of a comma flatter than perfect, as required in the tuning of his system, except the judgment of the ear, which is incompetent to the purpose. To those who wish to adopt or try this, or indeed any other tempered system, I recommend a careful study of that excellent work *Dr. Smith's Harmonics*, and the article *Temperament* in the Supplement to the third edition of the *Encyclopædia Britannica*, where the correct and elegant method of tuning any system by *beats*, is thoroughly and clearly explained.

I am somewhat surprised to find a nobleman of lord Stanhope's degree of information, laying so much stress upon four false intervals, which he calls *wolves*, occasioned by the particular series of perfect *fifths*, which he recommends to follow each other in the tuning, without duly considering, whether any perfect *fifths* can be introduced between the twelve sounds of a tempered system for our present keyed instruments, without doing mischief, and particularly, by occasioning transitions during performance *from a better to a worse harmony*; which Dr. Smith, and I think justly, considers as the principal cause of the disagreeable effects which nice ears experience, in our best concerts where keyed instruments are used. If, either the tuning of an organ &c. was begun on any other note than C, as in tuning the violin, &c., or tuning progressions were carried on, wholly or in part by perfect IIIds, 4ths and VIths, or even by 3ds and 6ths, which are alike practicable, with using Vths alone, wolves might shortly be found on each of the 12 notes, and even three and sometimes four of these jarring elements would be found, inherent in the same finger-key; as an inspection and trial, upon Maxwell's complete diatonic scale, would readily satisfy any one.

Much labour would attend the collecting, of what I have conceived to be the requisite data, for determining the best general system of tones for our keyed instruments, viz. a very general and extensive search into the music, both antient and modern, serious and gay, which is now performing, to ascertain the relative or proportionate frequency of occurrence, of the several chords or harmonic intervals, which occur upon each finger-key as a bass or lower note; that, if possible, the chords most frequently occurring, may be made proportionally nearer to perfection; for none can, with propriety, it is plain, be made absolutely perfect.

I am, sir, your obedient servant,

JOHN FAREY.

12, Upper Crown-Street, Westminster,
Nov. 4, 1806.

XXX. *Notices respecting New Books.*

A Treatise on Plain and Spherical Trigonometry: with their most useful Practical Applications. By JOHN BONNYCASTLE. 8vo. pp. 419. Johnson. 1806.

MR. Bonnycastle has for several years been well known to the public, as the writer of various useful elementary works on mathematical topics; and one of those treatises in particular, we mean that on astronomy, displays so much chasteness and propriety of style, as to rank its author among the most elegant scientific writers of the present times: but we conceive none of his publications will tend

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so fully to establish his reputation as a mathematician, and to exhibit his dexterity in analytical investigations, as the work now before us. Convinced as we are that the higher parts of the doctrine of trigonometry are admirable instruments in many philosophical researches, and especially in those which relate to physical astronomy, we trust we shall be benefiting many of our readers by introducing this valuable performance to their notice.

The work is preceded by an introduction of twenty-eight pages, containing a judicious, though concise, history of the principal writings relative to trigonometry. The logarithmic and algebraic rules for all the cases of plane triangles, whether right or oblique, and a great variety of practical examples, several of which are wrought out at length, occupy about seventy pages. These are succeeded by the doctrine of spherical trigonometry, and its application to astronomical problems, comprised in two hundred pages. The distribution of this part of the subject is as follows: General properties of spherical triangles; on the ambiguous cases of spherical triangles; the affections and other properties of right-angled spherical triangles; solutions of the six cases of right-angled spherical triangles, by construction, by calculation, and instrumentally; a similar division with respect to the six cases of quadrantal triangles; and, a similar one relative to the six cases of oblique-angled spherical triangles; logarithmic and analytic solutions of all the cases of right-angled, quadrantal, and oblique-angled spherical triangles; miscellaneous problems for exercise; application to the solution of astronomical problems; tables of right ascension, &c. useful in the preceding solutions; and, miscellaneous astronomical problems. This part of the work cannot fail to be of the highest utility; for in the exhibition of the general rules of spherical trigonometry, the author has struck into the happy medium between the fatiguing prolixity of most writers on this branch of the subject, and that abstracted analytical process by which Euler, Gua, Lagrange, and others, have deduced all the practical theorems from one fundamental formula; a process which, though it is so conducted as to be both gratifying

tifying and serviceable to the advanced mathematician, is not in our opinion perfectly calculated for the initiation of a student, at least in the present state of other branches of mathematics in this country. Besides this, there is a very important advantage in exhibiting the algebraic formulæ as Mr. Bonnycastle has done, together with the logarithmic rules; because by properly attending to the signs (+ and −) of the various expressions for the sines, tangents, &c., of arcs or of angles, and particularly by adopting those formulæ which furnish results in cosines, or cotangents, or the tangents of half arcs, or tangents of 45° + half arcs, or angles, every ambiguity which would otherwise arise on the resolution of spherical triangles may be kept clear of, except those which appertain to the two cases that are *necessarily* ambiguous: even in these two cases the student may proceed without difficulty by attending properly to this author's observations at pp. 77, 78, 79. Had he, indeed, inserted in his work, Bertrand's table, given by Lacroix, to which he refers in a note at p. 79, a table which, though it comprises all the possible varieties of these two cases, does not fill a page, there would have been little or nothing wanted to render this part of his treatise complete.

After the doctrine of spherical trigonometry, Mr. Bonnycastle treats of the mutations of the signs of trigonometrical quantities; and then presents, in fifty-four pages, a most copious and interesting collection of trigonometrical formulæ, relating to what is usually termed the arithmetic of sines and cosines, the values of sines, cosines, &c. in terms of circular arcs, &c., and vice versâ, exponential quantities, logarithmic series, the series for logarithmic sines, tangents, secants, &c. This part of the work alone is sufficient to stamp its value, did not every other part bear evident traces of the same hand.

The demonstrations of the principal theorems in plane and spherical trigonometry, made use of in the earlier part of the volume, succeed the trigonometrical formulæ; and these are followed by demonstrations of the leading theorems in the stereographic projection of the sphere,—some miscellaneous problems relative to spherical areas,—solutions of

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all the cases of plane triangles, independently of any tables,—formulæ respecting the increments and fluxions of the sines and tangents of arcs and angles,—the solutions of quadratic and cubic equations by tables of sines and tangents,—and rules for the admeasurement of altitudes by the barometer and thermometer.

Such are the principal topics discussed in the volume before us: our mathematical readers will at once see their value and importance. We could have wished that some of these had been treated more at large; especially the different kinds of projection, and the application of the fluxions of trigonometrical quantities to astronomical and other problems; and we should also have been gratified to see the demonstrations accompanying their respective rules, instead of being separated from them,—and to find no theorem, whether simple or complex, but what was demonstrated. We are, however, aware that all this could not be comprehended in a single volume; and we therefore hope the present edition will experience the encouragement it so richly deserves, that Mr. Bonnycastle may be enabled in a future edition to extend the utility of his performance, by making the additions we now suggest. Taking the work, however, as it is, we think it by far the best on trigonometry that has yet been published in the English language; and we are conscious, that in recommending this treatise to general favour, we are equally discharging an act of justice to the author, and of service to the public.

XXXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

On Thursday the 6th of November the Royal Society assembled, after the summer vacation, at its apartments, Somerset-House, when the right honourable colonel Greville, vice-president, took the chair. The reading of the Croonian Lecture on muscular motion, by J. Peirson, Esq. commenced.

On the 13th the right honourable Sir Joseph Banks, Bart. president, in the chair, the reading of the lecture on muscular motion was resumed. Mr. Peirson entered into a long and rather amusing detail of the relative heat and pulsations of animals in different latitudes, with a view to ascertain their effects on the muscles. In this country, he observed, horses pulsate 36 times in a minute, cows 48, and men 72; in Lapland and the northern provinces of Russia, men pulsate only from 45 to 50 times in a minute. From these observations on pulsation, however, no positive conclusion relative to its effects on muscular power could be drawn. It appeared, indeed, that all excess either of heat or cold is immediately followed by a sensible diminution of this power; and a fellow of this society is so affected by swimming in water only 10 minutes, that it occasions such a prostration of muscular power, as cannot be completely re-established in 24 hours after. Mr. P. made numerous experiments on the muscles of frogs, in all of which he found the muscular irritability completely destroyed by plunging them in water at the temperature of 96°: electricity, after such immersions, sometimes gave slight symptoms of excitability, but no human effort could ever again restore the muscular fibre to its proper tone and vigour. Cold produced precisely similar effects on the muscular fibre, by instantly destroying its irritability. Here Mr. P. observed that great care was necessary in applying warm water to the surface of bodies recently immersed in water in cases of suspended respiration, as the heat might be equally as bad as the cold, with regard to its effects on the muscular fibre, which he considered in some degree the organ of life. Blood, he alleged, was essential to life only as a necessary stimulus to muscular irritability; and the abstraction of blood occasioned death, not from destroying the continuity of that fluid, but from the want of its great stimulating powers to the muscles. The effects of laurel water and the vegetable poisons were next examined. A small quantity of laurel water was thrown into the stomach of a frog: it occasioned instant death, and on examining the stomach, it was found that the muscular motion of this most important organ was totally destroyed.

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The excessive irritability, or rather sensibility, of the stomach, indeed, accounts for the instantaneous and fatal effects of poison, as a blow on that organ immediately destroys life, whilst the heart can support a wound for some days. All vegetable poisons, it appears, act by destroying the irritability * of the muscles, or the power of the muscular fibre. The word *power* he defined to be the appellation of an indefinable quality inherent in muscles, as magnetism in steel or electricity in the torpedo. This definition is perhaps worthy the attention of the northern professors who have said so much about the term *power*.

Nov. 20. The president in the chair.—The reading of the Bakerian Lecture, "On some chemical effects of Electricity," by H. Davy, Esq. commenced. The preliminary experiments related chiefly to the production of fixed alkali in water by means of a powerful Galvanic battery, having the positive and negative wires inserted in different inoxidizable vessels containing distilled water. From these experiments a very obvious, but very important, fact resulted; namely, that even distilled water is never chemically pure, but that it still retains frequently both vegetable and animal matter combined or dissolved in it, and always nitrogen gas or some salt. Hence, after reapeated experiments, he found that electricity did not *generate* fixed alkali, as supposed by Pacchiani, but only *evolved* it. "Water," observed Mr. D., "when chemically pure (a state which at present scarcely appears practicable by all our art), is decomposed by electricity, and resolved into pure oxygen and hydrogen." We have to lament that this ingenious experimentalist, with such peculiar advantages, should still continue to operate on so small quantities as single ounces of water.

SOCIETY OF ANTIQUARIES.

This society also assembled after the vacation, on Thursday evening of the 6th November, the Rev. Dr. Hamilton,

* Mr. P.'s experiments certainly do not prove that these poisons act by robbing the system of its necessary portion of oxygen, as supposed by some philosophers.

vice-president, in the chair.—After the usual business of the society in reading the minutes of last meeting was performed, a short but well-merited eulogium was pronounced from the chair on the late secretary, the Rev. John Brand, A. M., the loss of whose talents, integrity and industry was sensibly felt by all the members. As a singular coincidence, a reverend gentleman of the same name was elected a fellow on that evening.

Nov. 13. Sir H. C. Englefield, bart. vice-president, in the chair.—A letter was read from ——— ——— Boydell, Esq., on the antient use of the word *burgh* or *borough*, and the modern signification of this term, which has been applied to a castle or fortified town. The real meaning of this appellation was illustrated by a reference to, and local descriptions of, several places now bearing this particular termination, as Peterborough, Harborough, Loughborough, &c. It was alleged that the word *burgh*, signifying borough, castle, or declivity of a rock, was derived from the Saxon; but when applied, as it often was, to designate low marshy tracts with slight elevations, it had its origin from a Gaelic word, signifying a flat country on the banks of rivers or estuaries. Instead, however, of having recourse to the Gaelic for the significations of names used in countries where that language was certainly never known, it is much more rational to conclude that the term signifying the banks of rivers, &c., is from the Anglo-Saxon *burn* or *bourn*, whence, probably, the names Eastbourn, Woburn, Sherbourne, Redburn, &c. The term *bury* is evidently of the same origin as *burg*, although not noticed by this writer, whose letter was so controversial, declamatory, and dogmatical, that it was sometimes very difficult to comprehend his real meaning.

Nov. 20. Craven Orde, Esq., vice-president, in the chair. —Several letters were read from J. H. Thornton, esq. and others, relative to some barrows or tumuli which were opened last October in Gloucestershire. Drawings of these barrows were exhibited; they occupied an extent of land from 40 to 55 yards long, and from 19 to 30 yards broad each, and contained several kistraens which presented nothing singular, except

except that of their being of an oval figure, and composed of calcareous stones, some of which were 14 feet long, and would weigh about five tons. In one of these kistraens eight skeletons were found, the bones of which were sufficiently perfect and entire, but were deranged in the act of opening. Others of the kistraens contained one, two, four and six skeletons, in general all of them well preserved; and a jaw-bone containing all the teeth white and perfect, was submitted to the inspection of the society. In one of the most distinguished kistraens some things, resembling the beads worn by the savages in the South-sea Islands, were found in a position as if they had been suspended round the neck of some of the bodies. From this circumstance, as well as the oval figure of the kistraens, it was conjectured that these tumuli must have been erected prior to the invasion of Britain by the Romans, and at a period when the people were in a state almost totally savage.

FRENCH NATIONAL INSTITUTE.

At the meeting of the Institute, on the 7th of July last, M. Cuvier, the perpetual secretary, read the following analysis of the labours of the class of physical and mathematical sciences, from the 20th of July 1805 to the first of July 1806.

The productions of nature have such an intimate connection with the climates which produce them, and are so essentially modified by the variety of climate, that no branch of natural history can make any solid progress without a correct acquaintance with geography: thus the latter science ought to be cultivated as assiduously by naturalists as by astronomers. We know well how much we are indebted to naturalist travellers; and M. Olivier has given us new proofs of their utility in a topography of Persia with which he has presented us.

He describes the chains of mountains, the course of the rivers, and he explains the nature of the productions by that of the climate. The almost absolute aridity prevents any more than a twentieth of this vast empire from being cultivated. Whole provinces have not a single tree which has not been planted and watered by the hands of man. The evil is continually increasing, from the destruction of the canals which carry the water from the mountains; and the districts which are abandoned are impregnated with salt, which renders them for ever barren.

But the meditations of the sedentary naturalist may also contribute to the perfection of geography by views proper to direct the researches of travellers.

M. Lacepede, by examining what is already known of Africa, by comparing the volume of the rivers which fall into the sea with the extent of ground upon which the rains of the torrid zone fall, and with the presumable quantity of evaporation, and, lastly, judging of the number and direction of the chains of the interior by those which have been visited on the sea-coast of this great division of the globe, has offered some conjectures upon the physical disposition of the countries of the interior still unknown, and particularly upon the seas and large lakes which he presumes to exist there. He has also pointed out the routes which appear the most proper for conducting travellers most speedily to the countries which remain to be discovered.

There is another kind of conjectural geography, which endeavours to determine the antient state of countries by what we observe in them at present.

M. Olivier with this view has examined what truth there was in the communication said to have formerly existed between the Black Sea and the Caspian. He thinks that it was in fact formed on the north of the Caucasus, and that it was the heaps of earth formed by the waters of the Couban, the Volga, and the Don which interrupted it.

As the Caspian has not since then received from the rivers which fall into it enough of water to allow for its evaporation, it has always lowered its level, and is at present 60 feet lower than the Euxine.

It was in this manner that it separated itself from the great lake Aral, and left uncovered those immense plains of salt sand which surround it to the north and the east.

M. Dureau de la Malle, son of a member of the Institute, has found in the Greek and Roman writers numerous testimonies

testimonies of this antient extent of the Caspian Sea and of its communications with the Euxine and with the Aral, and has collected them into an elaborate memoir, which he has presented to this class and to that of history and antient literature.

The antients attributed the separation of the two former, and the great diminution of the Euxine itself, to the rupture of the Bosphorus, which they supposed had caused the deluge of Deucalion; the Euxine being thrown with violence by this aperture over the Archipelago and Greece. Some of them even thought that at this æra the Mediterranean, suddenly augmented by the same cause, had broken through the Pillars of Hercules, and formed the streights which unite it to the great ocean.

But M. Olivier thinks that if the Euxine had ever been higher than at present, it would have naturally found an outlet by the plain of Nicea, and the other valleys which lead to the Propontis and the Archipelago; that, at all events, the strait channel of the Bosphorus could not have furnished enough of water to inundate the high mountains of Greece, which are more elevated than any on the shores of the Euxine, and far less could it have produced a sensible effect upon the immense expanse of the Mediterranean.

He is of opinion, therefore, that the stories of the antients in this respect, have their foundation neither in observation nor in tradition, but merely in conjectures which the physical situation of the places entirely reverses.

It is not less true that the part of the Bosphorus nearest the Euxine sea presents traces of volcanic revolutions, but the rest of its extent is a natural valley: it is the same with the Hellespont.

Some other researches tend also to show the utility of the alliance between the sciences and erudition.

M. Monges, upon the occasion of two mill-stones being dug up near Abbeville, collected together all the passages in the antients relative to the stones of which they made their mill-stones. It results, that they were almost always made of porous basaltic stones: those dug up at Abbeville being

made

made of pudding stone, appeared to M. Monges to have belonged to the Gauls or the Franks.

M. Desmarets having examined the vestments interred in an old tomb at the abbey of St. Germain-de-Pres, discovered that almost all the processes at present employed in weaving our different stuffs were known so far back as the tenth century: from this he takes occasion to explain in a new manner the passages in Pliny upon the weaving of the antients.

The position, the nature, and the boundaries of a country being once clearly ascertained, it then belongs to descriptive natural history to make known its productions; and the researches of the members of the class in this branch of science have been very productive.—The botanical department continues with increasing success the publication of important works.

The *Flora of New Holland*, by M. de la Billardiere, and the magnificent *Description du Jardin de la Malmaison*, by M. Ventenat, have arrived at their 19th number each. The *Flora d'Ovare et de Benin*, by M. de Beauvois, is at its 5th number. A fifth volume has appeared of the *Botaniste Cultivateur*, of M. Dumont-Courset: and M. de Lamarck has given, in conjunction with M. Decandolle, a third and greatly enlarged edition of the *Flora Française*.

M. de la Billardiere has made us more particularly acquainted with six new genera of New Holland.

The three first are ranged naturally among the myrtles, a very numerous family in New Holland, and from which medicine and the arts may derive an advantageous use, on account of the aromatic oils furnished by the trees and shrubs belonging to them.

The first genus, named *pileanthus*, is very remarkable for an envelope of one entire piece which incloses every flower; the petals of the latter are five in number; and the calyx is divided into ten equal stripes: the fruit contains several seeds.

The second has received the name of *calothamnus*, on account of the elegance of the flowers, the numerous stamina

mina of which are supported on a large filament divided into two at each extremity, while the two other filaments are barren: the fruit is quite similar to the *metrosideros*.

The third is named *calytrix*, and is known by its tubulous calyx above the germ, and divided into five parts, each terminated by a long bristle. The capsule only contains one grain.

The fourth has received the name of *cephalotus*, and belongs to the rosaceous tribe: the species named *follicularia* is perhaps still more remarkable than the *saracenia* and the *nepenthes*, by the form of some of the leaves, which represent very distinctly a purse full of shoots, surmounted by an operculum edged by crotchets directed towards its interior.

The fifth, named *actinotus*, has all the appearances of a plant of the family of the *corymbifera*, although in reality it belongs to that of the *umbellifera*. The two stigmata are swelled out towards the top, and are surmounted in the internal side by a bristle, which gives them the appearance of the antennæ of insects, as in the *lagoecia*: it has only one seed.

The sixth, named *prosanthera*, belongs to the numerous family of the *labiatæ*. The calyx is formed of two entire divisions; the largest of which inclines towards the other, and covers it as soon as the corolla has fallen. A thready-like appendage issues from below each of the antheræ. The fruit is the same as that of the *prasium* genus; but it is very remarkable in this family, that the embryo or corculum is inclosed in a fleshy and thick albumen, while in the other *labiata* hitherto observed, it is uncovered.

[To be continued.]

AGRICULTURAL SOCIETY OF TURIN.

Two memoirs lately presented to this society have been well received. The one is by M. Freylino, relative to the extraction of a saccharine matter found very abundantly in the fruit of the black mulberry tree, and which may be œconomically extracted, either in the state of syrup or concrete sugar. The author confined himself to the extraction of the syrup,

syrup, which he effected by means of extracting the juice, clarifying it with the whites of eggs, and afterwards evaporating it to the consistence of syrup. The other memoir is by M. Gogo, who has extracted a sweet and agreeable oil from the kernel of common hazel nuts.

ACADEMY OF USEFUL SCIENCES AT ERFURT.

At the meeting of the above society, on the 5th of April last, professor Bernhardi afforded some new ideas upon the double refraction of the rays of light by means of gypsum. M. Haüy, and still more recently M. Brisson, had observed this property, but not with sufficient accuracy: in general, the surfaces were not parallel to each other; the angle they formed, and the direction taken by the images, were not carefully determined.

M. Bucholz communicated the results of his researches upon the seeds of *lycopodium*. His experiments present the following results:

1. These seeds contain a sixteenth part of a fat oil of a brownish yellow, and soluble in alcohol. 2. A portion of real sugar. 3. A viscous extract of a brownish yellow, and an insipid taste. 4. The residue, after being treated with alcohol and water, may be regarded as a peculiar product of the vegetable kingdom. 5. The yellowish aspect of the seed in this latter state indicates the union of a species of pigment with the first principle of the seed, or at least a very intimate union of the constituent parts of this seed. 6. The oily part which enters into the composition of this seed occasions its lively combustion, and its constant separation from water.

ROYAL ACADEMY OF SCIENCES OF BOHEMIA.

The above academy has announced a prize of 100 ducats of gold, for the best memoir upon the processes for ameliorating the races of Tartar, Moldavian, Transylvanian, Hungarian, Polish and Bohemian horses: a second prize will be also adjudged to the best memoir upon the advantages or disadvantages attached to the employment of Hungarian horses and mares in the cavalry.

XXXII. *In-*

XXXII. *Intelligence and Miscellaneous Articles.*

PROGRESS OF VACCINATION IN FRANCE.

The central vaccine committee of Paris on the 12th of July last made their report upon the exertions made in France for the propagation of vaccine inoculation, during the last twelve months. The number of individuals vaccinated in 42 departments during that period amounts to 125,992, which gives a total of nearly 400,000 for all France; and by supposing, as in the former year, the number of births at 1,088,157, it follows that a third at least of the infants born last year in France have been vaccinated.

Numerous tests have been tried in order to ascertain the preservative effect of vaccination; and whether inoculation for the small-pox was resorted to, or an intimate and habitual commerce between small-pox and vaccine patients, and the latter also subjected to the influence of variolous epidemies, and even when all these three kinds of proofs were united together, the small pox never had any effect upon those who had gone regularly through the vaccine infection. The most important result of the report of the committee is the certainty of the progressive diminution of mortality wherever vaccination was introduced, and the increase of mortality in those places where vaccination was neglected.

New and fortunate experiments have been made upon the manner of simplifying still more the insertion of the matter, and also the best means of preserving the infectious substance.

The central committee have not confined themselves to the human species alone in their inquiries. They endeavoured to ascertain if vaccination introduced among sheep would preserve them from the scab. They found, however, that it is more advisable to inoculate these animals with scabby matter than to vaccinate them, although the latter process was often completely successful.

The report concludes with the names of those who have distinguished themselves by their zeal in promoting vaccination

nation in France, and by recommending them to the notice of the government.

LITERATURE.

Russia.—The late M. Hadsi Niku, an eminent Russian, founded a school at Cronstadt for the education of the modern Greeks, and it already contains thirty-four students. The objects of instruction are, religion, reading, writing and arithmetic, and the ancient Greek according to the grammar of Constantine Lascaris. The professors are monks of Mount Athos. Cronstadt has also a very good school for the Walachians, with three professors.

Austria.—A school of philosophy has been established at Bruix in the circle of Saatz in Bohemia. Besides the university of Prague, the kingdom of Bohemia has seven other schools of philosophy and theology, the chairs of which are filled by ecclesiastics.

The Austrian government is more and more persuaded that it is not so much necessary to punish crimes as to prevent their commission by removing the causes of the evil.

As crimes have been more frequent in the Bannat and in the countries of Transylvania inhabited by the Walachians, seminaries of education, and schools of theology and philosophy, are established in these districts.

During the last war the book trade in Austria was completely at a stand, and there was no bookseller from Vienna at the last Leipsic fair.

Hungary.—Several works have issued from the press at Pesth, within the last year, in the Hungarian language. Among these are translations of the Letters of Cicero, Tasso's Jerusalem Delivered, and of M. Chaptal's work on the Culture of the Vine.

A journal also appears under the title "*Ungrische Miscellen,*" (Hungarian Miscellanies) of considerable interest to the learned.

MISCELLANEOUS.

There is in the press at Edinburgh an "Account of a Tour through the Orkney and Shetland Islands," by Mr. Neill, secretary to the Natural History Society of Edinburgh. The author, after describing the objects of natural history which occurred in his progress, treats fully of the

state

state of agriculture and the fisheries in those much neglected but interesting islands. The tour is to be followed by a Mineralogical Survey of Shetland, from the pen of Dr. Traill, of Liverpool, who lately visited those northern islands.

LIST OF PATENTS FOR NEW INVENTIONS.

To Robert Bowman, of Leith, in Scotland, manufacturer, for making hats, caps, and bonnets, for men and women, of whalebone; harps for harping or cleansing corn or grain, and also the bottoms of sieves and riddles, and girths for horses; and also a cloth or webbing fit for making into hats, caps, &c., and for the backs and seats of chairs and sofas, gigs, coaches, and other similar carriages and things, and the bottoms of beds, as also reeds for weavers. Dated October 30.

To Robert Vazie, of the parish of St. Mary, Rotherhithe, in the county of Surry, civil engineer; for improvements in the measures and in the machinery to be used in making bricks and earthen ware, and also improvements in the carriages for removing the said articles. Dated November 6.

To James Royston, of Halifax, in the county of York, card-maker; for his improvement on the system of card-making, by a method of cutting teeth for carding of wool and tow. Dated November 6.

To John William Lloyd, late of Brook-street, Grosvenor-square, in the county of Middlesex, but now of Bishop Wearmouth, in the county of Durham, esq.; for antifriction rollers or wheels, to assist all sorts of carriage wheels. Dated November 20.

To John Henckell, of the city of London, merchant, in consequence of a communication made to him by a French emigrant residing abroad; for certain improvements on a machine for dressing coffee or barley, or any other corn, grain, pulse, seed, and berries. Dated November 20.

To William Nicholson, of Soho-square, in the county of Middlesex, gentleman; for various improvements in the application of steam to useful purposes, and in the apparatus required to the same. Dated November 22.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CAREY, OF THE STRAND,

For November 1806.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Oct. 27	52°	63°	51°	29·96	25	Fair
28	52	61	48	·85	10	Cloudy
29	47	55	46	30·22	11	Fair
30	45	55	45	·12	12	Fair
31	46	57	50	29·76	10	Fair
Nov. 1	50	59	49	·64	12	Fair
2	51	53	49	·15	0	Stormy
3	50	54	48	·10	0	Stormy
4	49	50	46	·16	0	Rain
5	47	55	38	·52	25	Fair
6	38	48	36	·78	20	Fair
7	35	48	44	30·20	15	Fair
8	45	55	46	·30	22	Fair
9	45	52	44	·20	18	Fair
10	40	47	46	·09	5	Cloudy
11	46	48	45	·12	16	Fair
12	41	45	40	·02	2	Cloudy
13	46	56	47	·12	12	Cloudy
14	49	53	51	·20	3	Cloudy
15	50	53	51	29·82	7	Cloudy
16	41	49	45	·76	19	Fair
17	46	53	51	30·00	16	Cloudy
18	51	55	50	29·80	0	Cloudy
19	47	53	40	·60	0	Fair
20	38	47	40	·42	17	Fair
21	46	48	37	·02	2	Fair
22	34	45	37	·50	12	Fair
23	38	47	46	·65	0	Rain
24	49	55	55	·90	3	Cloudy
25	55	57	53	·71	0	Small rain
26	54	50	47	·38	0	Rain

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

XXXIII. *A Memoir on the best Method of measuring Time at Sea, which obtained the double Prize adjudged by the Royal Academy of Sciences; containing the Description of the Longitude Watch presented to His Majesty the 5th of August* 1766. *By* M. LE ROY, *Clock-maker to the King. Translated from the French by Mr.* T. S. EVANS, *F.L.S., of the Royal Military Academy, Woolwich.*

[Concluded from p. 146.]

PART IV.

Further observations on the construction of the new watch, by which we confirm the advantages of the methods which are used: difficulties in some of these methods removed: recapitulation, &c.

I PROPOSE, in this fourth part, to clear up some articles that I could not give with sufficient extent in the preceding without removing one object from another, which when brought together mutually render each other more intelligible.

One of those which most requires to be explained is the motive force.

This part of my watch, perhaps, may to some persons appear neglected: *it has no fusee*; nor have I used the methods which Messrs. Leibnitz*, Hook †, Huygens ‡, Sully §, Harrison ||, and others, have applied to render the magnitude of the vibrations, and the force which maintains them, constant. According to this method, the watch has, we know, two motive forces; of which one that only moves the last, or the two last wheels, is wound up by the other, which being successively stopped, or put at liberty by means of any detent, becomes foreign to the regulator.

I answer, that by supposing these methods to possess any advantage, nothing would prevent their application to my chronometer; they are known to men of science and artists.

* *Journal des Savans* 1675.
† Ibid.
‡ *Horologia oscillatoria.*
§ *Descrip. d'une Pendule Marine.*
|| *Gazette du Commerce.*

artists, and the public have been in possession of them for a long time.

I might have copied the able gentlemen just now cited, if I had thought them necessary, or even favourable for my machine; but various experiments, and the following reasons, have prevented me from making use of them.

1st, This method renders the machine more complicated, and of more difficult execution; it requires, besides the common constructions, a detent, a spring, a wheel, or a fly, &c. whose adjustments are difficult. There are also few workmen capable of executing these in such a way as to be certain. *They augment*, says M. le Roy, *the friction; and the risks of stopping are greater in proportion to the number of pieces.* But if there are found so many inconveniences in *remontoirs* * applied to clocks whose size is arbitrary, and besides are made to remain in a temperature that varies very little, on land, and in cities where we find workmen to repair them; what may not be objected against this practice in works whose size is confined, destined besides to receive continual motions, to be removed into all climates, to experience the extremes of different temperatures, and to be always either at sea, or in places destitute of skilful workmen?

With regard to the fusee, I only think it useful in watches where the vibrations of the regulator are not isochronous, and where it is necessary that the balance, when stopped, should be put in motion by the motive force. I think it would be superfluous and even disadvantageous in mine, where the very powerful regulator makes its vibrations isochronous: in effect, it does not remedy the losses of elasticity of the main spring, nor the clogging of the wheelwork: besides, when the vibrations of my balance are so nearly equal in duration that they are isochronous, the fusee

* It is usual in clocks to place a wheel underneath the barrel round which the cord is wound that sustains the weight. This barrel has the liberty of turning in a contrary direction to its usual motion in the clock. To the barrel is attached a rachet and click, which prevents it from turning the way it is drawn by the weight, and of course the weight can therefore only descend by the motion of the whole train of wheels that give action to the clock. This contrivance is called a *remontoir* by the French artists. See Alexander, *Traité gen. des Horloges*, p. 140.—T. S. E.

would

would not become the less useless if we wound up the machine at the same hour; which it would be easy to confine ourselves to. There would not even be a difference at all sensible in the arcs of vibrations, if we wound it up every twelve hours; and in the twenty-four hours the difference in the arcs actually amounts to but one-sixth. This fusee, whose inutility in my watch appears to me evident, would besides be disadvantageous: it would complicate the machine, and render it more subject to stop, by the breaking of the chain; the watch could not go when winding up (an indispensable thing in a watch where two seconds is a considerable object) without having recourse to complicated methods, the greater part defective, especially in the present case, where nothing can be too simple for sailors.

The omission of the fusee gives likewise to my watch a very essential property, which it would have been deprived of by the methods of Messrs. Leibnitz, Sully, Harrison, &c. He who makes use of it is always by this means able to see whether the fundamental principle on which this kind of work ought to be constructed is found there*; I mean the perfect isochronism of the long and short vibrations, which will be verified by observing the rate of the watch during the whole course of the spring.

Observation II.

On the suspension wire.

This wire is absolutely necessary to avoid friction, which, without it, would take place in the extremity of the lower pivot of the balance, and to preserve its freedom, on which, as has been demonstrated, depends all the regularity of the clock. An experiment, which I have repeated several times, suffices to show how essential it is that so powerful a balance, whose mass is so considerable, should be thus suspended. I took away the suspension wire, and I suffered the lower pivot to rest on a plate of tempered steel, well po-

* To receive this advantage more completely, I leave the spring to open thoroughly, so that the watch goes about 38 hours. In common practice we stop this spring at about one turn.

lished and thoroughly hard: three days afterwards this plate was worn at the place of the pivot; the arc of vibration was considerably diminished; the freedom of the balance, consequently, very much altered; and the accuracy of the machine destroyed. I substituted for the steel plate a polished agate, and the same effect again took place. Lastly, to see whether when the weight of the balance was diminished on the agate the wear would not cease, I replaced the suspension wire, I attached its upper extremity to a lever, and I put a weight on the other arm of this lever, so that the balance, exceeding the weight a little, rested very gently on the plate of steel or the agate. Notwithstanding this precaution, the freedom was again very much altered by this slight friction; the plate and the agate both wore, although much less than before; whence arose the inconveniences above mentioned. It appears, therefore, absolutely necessary in these kinds of works that the regulator should be suspended by a harpsichord wire, as the *foliot* was formerly by two threads of hemp or silk.

In the first attempts which I made with this machine, nearly twelve years ago*, instead of using a harpsichord wire to suspend it, I used a piece of thin narrow spring. Several experiments (by which I found that the different vibrations of a body thus suspended were much more isochronous than those procured to the same body by a spiral spring) induced me to make the regulating spring of my watch of this suspension spring, and to omit the spiral spring: but I soon perceived that to approach isochronism nearly, it would be necessary that this regulating and suspending spring should be very long; this would render the machine very unwieldy in a ship. At last I arrived at the isochronism of the vibrations, by combination and a certain proportion between the spiral spring (of which I found the long vibrations slower than the short) and the suspension spring; which gave me, on the contrary, the short vibrations slower than the long. This method, like the preceding, required a very long suspension spring; whence arose various inconveniences.

* See the sealed paper which I left with the secretary to the Academy in 1754. The *Exposé succinct*, &c. p. 40 and 42.

veniences. It was at first difficult to be assured that the spring was sufficiently straight, and that the balance was attached in such a manner, that its weight acted in a line along the middle of the breadth of the spring throughout its whole length: without this, however, it produced a very disadvantageous friction, and a difficulty in each vibration. Moreover, the weight of the balance was not sufficient to stretch this spring perfectly; it was hardly possible for it not to be bent a little in some part of its length: these curvatures diminishing by shocks and heat, or augmenting by cold, there arose irregularities difficult to prevent. Lastly, the distance at which the elastic force acted, being only equal to half the width of the spring, the least differences which might happen in the situation of this spring, whether by the small play of the balance in its holes, or by other causes, could not but have some influence on the manner in which it acted; this does not happen in the spiral spring, which acts always at a considerable distance from the axis of the balance.

All these inconveniences are prevented by the harpsichord wire: it is so small that it can have but little influence on the vibrations; it may be made much shorter; it is exactly stretched, and without curvature throughout its whole length; and being round, we may be certain, at first sight, that all its parts agree with the axis of the balance.

To this it may also be added, that, by the operation of drawing the wire, we are assured that the substance of which it is composed is homogeneous and pliant, such as it ought to be for this suspension.

I have said it is necessary for the wire to be fine: experience has proved to me that without this it would require to be very long, which would render the machine cumbersome. Having taken to suspend a balance a thicker harpsichord wire, of about four inches long, I remarked that the motion of the regulator lost with the greatest readiness until it was reduced to describe only four or five degrees, and then it remained as long a time in motion as if it had been either very long or very small; whence I concluded that the motion is not lost so readily in a large arc, that because the parts touched they formed an obstacle in some degree insur-

 mountable,

mountable, or experienced a considerable friction of the parts one against the other; whence it is evident, that, a suspension spring being necessarily much more extensive in its width than a wire in its size, it can but be very long, and consequently very embarrassing. Independently of the defects which we have before remarked in it, the necessity of using a very fine suspension wire (by having recourse to the means which I used to place this wire out of the way of accidents, to which its fineness exposes it) is therefore proved. (See Article VIII. Part III.)

Observation III.

On the substance of the regulator.

Steel appears to me preferable to construct the balance of; being a substance less dilatable, more solid, and less variable by the effect of heat, than brass, &c. The fears of magnetic influence are not, in my opinion, of any consequence. For them to have any foundation it would be necessary for this balance to acquire poles, which can never happen in a body that is continually changing its position; every effect which only increases its weight, or gives to its mass a tendency towards one side, would produce nothing in the vibrations, the balance only acting by its inertia.

Observation IV.

On the motion of the balance.

The friction on the pivots of the rollers which contain the balance of the new watch is almost nothing; for the following reason: The pivots of these rollers have necessarily a little play in their holes; whence it happens that, when these rollers describe a very small arc, their pivots only rest on the edges of their holes without rubbing. To receive the full advantage of this, and to have besides more freedom, a less resistance on the part of the air, &c., I have only rendered the arcs of vibration as great as the effect of shocks appeared to me requisite to be prevented. Each vibration of the balance is half a second, and the watch beats seconds; this appeared to me the most convenient and the most advantageous. I could not have increased the number

of

of vibrations in a given time without increasing the operations of the escapement also, and without the freedom of the balance suffering some diminution.

Observation V.

On the compensation for the effects of heat and cold.

According to the *Gazette du Commerce*, and the report signed Ludlam, sent to the Academy, to remedy the irregularities produced in marine watches by heat and cold, *Mr. Harrison uses a bar composed of two thin pieces of brass* and steel, two inches in length, riveted together in several places, fixed at one end, and having at the other two pins across, through which passes the balance spring. If this bar remains straight in temperate heat (as brass receives more impression from heat than steel), the side where the brass is becomes convex by heat, and the steel side becomes so by cold. Thus the pins, one after another, fix the parts of the spring according to the different degrees of heat, and lengthen or shorten it; whence follows the compensation for the effects of heat and cold.*

If I had known this ingenious method before I thought of my thermometers, probably I should not have hesitated to have made use of it in my machine.

I considered some time whether I should not give it the preference. I even made some attempts with this view. I shall speak of them presently; but, after having thought of them seriously, and after having put aside, as much as I could, that prejudice which we have in favour of our own productions, my thermometers appeared to me preferable. The following are the reasons which induced me to judge so:

The first, which would have prevented me from making use of it without some considerable change, was, that by Mr. Harrison's method the regulating spring does not re-

* The author makes use of the word *cuivre*, which is commonly used to express copper; but in Mr. Harrison's pamphlet it is *brass*; we have therefore given it so. From this it appears probable that the author in other places may mean brass, although that is commonly distinguished from copper by *cuivre jaune*.—T. S. E.

main always of the same length, which I have proved, Article III. Part V. to be absolutely necessary: likewise, when I endeavoured to compensate the effect of heat and cold by pieces of brass and steel, riveted together as Mr. Harrison's, I endeavoured not to change the length of the spiral, but to make a considerable part of the circumference approach or recede by this means from the centre of the balance. For this purpose I used a balance (fig. 4 and 5. Plate I.) composed of two semicircles, each formed of a piece of brass and steel, united as in Mr. Harrison's thermometer piece.

The effect sufficiently answered my intentions: I even observed, by means of the index *i*, which was moved by similar pieces *ll* (fig. 2. and 3. of the same plate), that by heat and cold the motion of these pieces would follow very exactly the motion of the thermometer: there resulted from this a compensation for heat and cold, whose effect might be increased or diminished at pleasure by putting a greater or less mass at the extremities of these semicircles; but by this construction the balance does not appear to me to have sufficient solidity: besides, in the different degrees of heat and cold, it would be difficult for it to preserve its equality of weight in all points of the circumference. Lastly, having exposed the machine, provided with this regulator, to a heat of 35 degrees * of the thermometer, after having replaced it in a common temperature, I saw that it would advance about six seconds in 24 hours: this appeared to me a consequence of what we have observed to happen in pieces of metals heated and kept in a state of constraint; for, here, the piece of brass dilating more than that of steel, neither one nor the other is in a state of freedom; besides which, it is almost impossible for them to be riveted together, and adjusted in such a way that their figure does not arise from their mutual constraint; inconveniences to which our thermometers are by no means subject.

Before finishing this article I shall make a remark which may be of some utility to those who would construct similar thermometers; which is, that it is necessary, before using the watch to which they are applied, that it be placed in the

* 110¾ of Fahrenheit.

greatest

greatest degree of heat that can happen to it in different climates; without which (the metal being by this heat a little annealed, and metals forged more or less and then annealed, dilating unequally, and besides, acquiring more extent by this annealing) the machine would gain after having experienced great degrees of heat; the parts of the metal, however little they may be annealed, do not return to the first contraction which had been given them by tempering, forging, &c.; and the same may be said of other parts of the regulator.

Observation VI.

On the size of the machine.

I think it ought not to exceed much the size of a variation compass, of which we can never complain.

This watch in itself is of a sufficiently small size; but it appeared to me, that to give greater security to all the parts, to put them more out of the way of rust, of excess of heat of short duration, and other accidents which may happen in a ship, it would require a case; and it would be right that all the parts of suspension, as well as the watch, should be shut up in a strong box with a lock. Besides, to diminish the effect of shocks, it would be necessary, as I have explained Article VIII. Part III., that it should be stopped in its motions by the cushions, which can hardly be adapted but to the sides of such a box.

Recapitulation.

After having explained the principles which have directed me in the different parts of my work, being the fruit of twenty years researches and labour, I shall attempt to show that by means of it we shall have the best measure of time at sea.

The following is the way I prove it:

It is evident by the first part, that this measure of time must consist in the most advantageous application of the balance and its spiral spring to the clock; that is to say, in a watch perfected as much as possible. To prove, therefore, that we are arrived at the best measure of time at sea, it is necessary to show that we have used the best methods to perfect the watch, that is to say, to correct its defects, which consist, as we have explained in the Second Part, in the non-isochronism of the vibrations of the regulator; 2dly, in its little power and freedom; 3dly, in the multiplicity of friction which it experiences; 4thly, in the variations to which it is subject by heat and cold; 5thly, in the bad effect of shocks.

Now it is, I believe, demonstrated in the Third Part, that the method which I have found to give the balance a perfect isochronism by the length of the spiral spring* is incontestably the most simple, the most certain, and the most exact. 2dly, That it is not possible to render the regulator more powerful, more free, and more disengaged from friction†, than I have made it by my suspension, my escapement, the double spiral spring, the rollers, the moveable pieces, the situation of the balance, its size, &c.; which is proved by the experiment related in Article V.‡ 3dly, That the method I have used to correct the effect of heat§, which is not subject to the inconvenience of giving way like metallic compensators, which are made securely and without play, which leave the length of the spiral spring constant, and do not destroy the isochronism of its vibrations, &c. is incontestably the best.

4thly, That the methods recommended Art. VII. Part III.‖ to correct in a marine watch the effects that heat leaves after it, that is to say, the gain or loss which sometimes follows, are probably the most efficacious. 5thly, That the expedients to which I have had recourse to render the motions impressed by shocks¶ less abrupt, of less magnitude, and less durable, are, without contradiction, the most certain, and likewise those which I have used to regulate the watch to the smallest quantity**, without changing the length of the spiral spring, or destroying the isochronism of its vibrations.

All these methods, therefore, appearing to me indisputably the most certain, the most simple, and the best that can be used to perfect the watch, I have some hopes of having, conformably to the demands of the Academy, *determined the best method of measuring time at sea.*

* See page 61 of this volume. † Page 60. ‡ Page 131.
§ Page 138. ‖ Page 141. ¶ Article VIII. Part III. p. 142.
** Article VI, Part III. p. 137.

APPEN-

Appendix.

I think it proper to add a word or two here on pocket watches, which may accompany the marine watch; they can never be so perfect, on account of their small size, which would not permit all the resources to be used which we have applied in our watch for the diminution of friction, &c. I believe, nevertheless, we may render them more exact: 1st, By giving to the vibrations of the balance a more perfect isochronism, by the method explained (Article III. Part II.*): 2dly, By compensating the effects of heat and cold by an expedient similar to that which Mr. Harrison has made use of in his time-keeper†: 3dly, By applying a dead escapement where the friction is much less than in the cylinder, &c. I do not here propose the detent escapement, because a watch appears to me too small for us to apply this mechanism easily to it. That of M. Sully, where the wheel is perpendicular to the plates, being perfected, appears to me the most proper to procure this diminution of friction. I have finished several watches in this way: for this purpose I have given to the balance wheel such a size, that it reaches as far as the dial plate one way, and to the spiral spring the other; I have also given to its teeth the form of radii, that this wheel might be very slight; and by means of some other corrections, I believe it may be demonstrated, considering the diminution of friction which results from the place where the wheel remains at rest being made very near the axis of the balance, that this escapement is the most perfect of all.

*** At the word "anchor-escapement," p. 63, line 27, the following should have been added as a note:—The anchor escapement is represented in Plate V. fig. 1. of Berthoud's *Essai sur l'Horlogerie*; and that with a double lever in Plate III. fig. 5 and 6 of the same work; also in Plate XLIII. fig. 30, of Thiout's *Traité de l'Horlogerie*; but the first figure is the best. An ingenious watch-maker has lately considered this as a detached escapement, and sup-

* Page 60 of this volume. † Page 199.

poses

poses it to be the first that was invented; but it does not appear how it can be reckoned of that kind.—T. S. E.

THE following is the opinion given of this watch by the French Academy of Sciences at their public sitting the 5th of April 1769:

" The Academy has adjudged the prize to the memoir which has for its device *Labor improbus omnia vincit*, and to the watch that accompanies this memoir. The author of both of them is M. Le Roy, clock-maker to his majesty. The rate of M. Le Roy's watch, observed at sea in several voyages (one of which was from the coast of France to Newfoundland, and from Newfoundland to Cadiz) has appeared in general sufficiently regular to merit this reward for the author, the principal intention of which is to encourage him to new researches; for the Academy must not dissemble, that in *one* of the observations which have been made on this watch, it appeared, even while on land, to gain rather suddenly 11 or 12 seconds per day: from which it appears that the desired degree of perfection has not yet been obtained."

XXXIV. *Memoir upon living and fossil Elephants.* By *M.* Cuvier.

[Continued from p. 169.]

The fossil elephants of Belgium have been long known. In the 16th century Garopius Becanus combated the prejudices which attributed to giants the large fossil bones formerly found in the neighbourhood of Antwerp; and he mentions the bones of two elephants dug up near Vilvorde, in a canal which the inhabitants of Brussels dug from that city to Rupelmonde, to avoid the trouble attending the conveyances by the canals of Malines.

John Lauerentzen, in his edition of the *Museum Regis Daniæ* of Jacobæus, part i. § 1. no. 73, relates the history of a skeleton which Otho Sperling saw dug up at Bruges in 1643, the thigh of which is preserved in the above cabinet. It is four feet long, and weighs 24 pounds.

M. de Burtin, in chap. i. § 2. p. 25. of his Prize Dissertation

sertation "Upon the Revolutions of the Surface of the Globe," published at Haarlem in 1787, says that he possesses an elephant's tooth found in Brabant. He adds, that a very large fossil head of this kind was dragged out of a river two leagues from Louvain by some fishermen.

The pretended bull's horn, so long suspended from one of the pillars of the cathedral of Strasburg, is merely a fossil tusk which had been formerly dragged out of the same river.

In general, the whole banks of the Rhine swarm with these bones.

In the canton of Basle, in Swisserland, they also abound.

The landgrave of Hesse Darmstadt's cabinet has a lower jaw of great size, found near Worms.

There is a particular dissertation of Charles Gotlob Steding upon the fossil ivory in the environs of Spires. It represents a jaw of thirteen distinct laminæ, weighing three pounds and a half, and was found four feet deep, near a fragment of a tusk of four pounds weight.

Merk mentions a cranium found near Manheim, a plate of which exists; but I cannot procure a sight of it. Its two jaws weighed 200 pounds.

M. Hammer possesses a tooth dug up in an island of the Rhine opposite Manheim, and a fragment brought out of the Rhine near that city. M. Gmelin, an apothecary at Tubingen, has a lower jaw found in the Rhine also near Manheim.

Germany is certainly the country where the largest quantities of fossil bones have been discovered; not, perhaps, because it contains more than any other country, but because there is not in the whole empire any district which does not contain some learned man capable of collecting and publishing whatever is remarkable.

Every body knows the history of the elephant discovered at Tonna, in the country of Gotha, in 1696, and which has been described by Tentzelius and Hoyer *.

* Tentzelii Epistola ad Magliabecchium, de Sceleto Elephantino, Tonnæ nuper effosso. Phil. Trans. vol. xix. no. 234, p. 757—776. I. G. Hoyer de Ebore fossili, seu de Sceleto Elephantis in colle sabuloso reperto, *Ephem. Nat. Cur.* dec. 3. an. 7—8. p. 294, obs. clxxv. See also Act. Erudit. Lips. Jan. 1697; and Valentini Amphitheatr. Zootomicum, p. 26.

A second

A second skeleton was dug up in 1799 about 50 feet from the place where the former had been found: M. Baron Zach, upon this occasion, gave a circumstantial description of the soil, to which we shall resort in order to give the details of the discovery.

There are two places called Tonna (Græffen Tonna and Burg Tonna), both situated in the valley of Unstrut, below Langensalza, and to the right of Salza and Unstrut. All this valley, like most of the low valleys in Thuringia, is filled by horizontal layers of a tender calcareous sandstone, which contains bones, deers' horns, impressions of various leaves which are thought to proceed from the aquatic plants and trees of the country, and shells which apparently belong to the *helix stagnalis*, and other fresh-water shells. This sandstone in some places resolves into a marly sand, which has been employed for this century past in manuring land. It is partly obtained by subterraneous and irregular trenches; those of the commune of Burg Tonna are 40, 50, and 60 feet below the surface.

The workmen find, from time to time, elephants' bones and teeth, and the bones of the rhinoceros, animals of the stag kind, and that of the tortoise.

These depôts of sandstone are mixed alternately with others of clay, where these bones are also found, although more rarely.

The two skeletons of 1696 and 1799 were found 50 feet deep.

Of the former there was collected a femur weighing 32 pounds; and the head of the other femur as large as a man's head, and weighing nine pounds; a humerus four feet long, two spans and a half broad; vertebræ, ribs; the head with four grinders weighing twelve pounds each, and two tusks eight feet long: but a great number of these pieces were broken.

We shall not detain our readers by giving an account of the disputes occasioned by this discovery. The medical inhabitants of the country, when consulted by the duke of Gotha, unanimously declared that these bones were lusus naturæ, and supported their opinions by several pamphlets: but

but Tentzel, the librarian to this prince, thought differently; he compared each bone, taken separately, with its analogous bone in the elephant of that period, such as it was in the description of *Allen Moulin*, and by some remarks of Aristotle, Pliny, and Ray; and showed the resemblance. He went further, and proved, by the regularity of the stratum under which this skeleton was found, that it could not be said that it was interred by human hands; but that it must have been brought there by some general cause, such as the deluge has been represented.

The second skeleton, that of 1799, was in a compressed and crooked position: it occupied a length of 20 feet; the hind feet were near the tusks. The latter were 10 feet long. They were tender, but entire; their cavity easily admitted a man's arm. A part only of the lower jaw of the head was preserved, and the two largest grinders. The greater part of the other bones and the ribs were broken as they were detached from the soil; but larger or smaller portions of all the bones were found. The cellulous parts of them were filled with crystals of spar.

The corona of one of the grinders was nine inches long, by three broad; its height was six or eight inches; an entire tibia was two feet four inches, and six or eight inches in diameter: a head of a femur was six inches in diameter.

At a small distance, and in similar strata, stags' horns were found, or what is called the fossil elk; and at Bœllstadt, a neighbouring village, some rhinoceros' teeth were found.

The valley of Unstrut has furnished fossil elephants in several other places of it, particularly a tusk, weighing 115 pounds, and 10 feet long, near Vera.

Another place, not less celebrated than Tonna for the number of fossil elephants, and bones of other strange animals which it has furnished, is the little town of Canstadt, in the kingdom of Wirtemberg, upon the Neckar. The principal discovery was made in 1700; and David Spleiss, a physician of Schafhausen, gave an account of it in a particular dissertation, entitled "*Œdipus osteolithologicus; seu*

 Diss.

Diss. histor. phys. de Cornibus et Ossibus fossilibus Canstadiensibus, 1701, 4to; in which Spleiss has inserted an account written by Solomon Reisel, physician to the duke of Wirtemberg. This discovery is also treated of in the *Medulla mirabilium de Seyfried*, and in the *Descriptio Ossium fossillum Canstadiensium de Reiselius*, 1715; and John Samuel Earl has given a chemical analysis of it, very correctly considering the period in which he lived, in his *Lapis Lydius philosophico-pyrotechnicus*, &c. Francfort 1705.

I am indebted to the friendship of M. Autenrieth, professor of anatomy at Tubingen, and of M. Yæger, keeper of the cabinet of natural history at Stutgard, for a still more circumstantial account of the above discovery.

These two gentlemen have still the bones before their eyes; they know the place where they were found; and they are in possession of the proces-verbaux which were drawn up at the time of the discovery.

The spot is on the east of the Neckar, about a thousand paces beyond the town of Canstadt, on the side of the village of Feldbach. Riesel says that there are the remains of an antient wall there, eight feet thick, and eighty round it, which seems to have been the inclosure of a fort or temple; and, in fact, some more remains of the same description are to be seen. Spleiss concluded that these bones were those of such animals as were sacrificed; but they were, for the most part, by far too deep for this supposition: besides, they have been found much nearer the Neckar in a natural soil, and quite similar to that where they are usually dug up. All that can be concluded from their abundance within this inclosure is, that they have been once before dug up in great quantities in that neighbourhood, collected together by some curious people, and again covered over.

The soil is a yellow clay mixed with small grains of quartz and small shells. M. Autenrieth has sent me drawings of five of the latter, which appear to me to be freshwater shells. This clay fills the various cavities of the calcareous hillocks in regular rows, and these hillocks are interspersed with larger ones of a reddish marl.

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These marly hillocks sometimes present us with petrified plants and beds of coal, and their summit is covered with old marine petrifactions, such as ammonites, belemnites, &c.

It was a common soldier who first remarked some large bones above the ground in April 1700. The reigning duke continued digging for them for six months, and such bones as were most entire were carefully preserved. The remains, being a prodigious quantity, (for, according to Reisel, there were more than sixty tusks,) were sent to the laboratory to be employed as fossil ivory.

The bones themselves were without any order, for the most part all broken; some few of them were as if they had been rolled about. There were whole cart loads of horse teeth, and there were not bones in proportion to the tenth part of these teeth. The elephants' bones seem to have been uppermost, and the others buried lower. In general, they were never found deeper than twenty feet. A part of them were entangled in a kind of rock formed of clay, sand, flint, and ochre, agglutinated together; and the workmen were obliged to have recourse to gunpowder in order to separate them.

The elephant bones still in the royal cabinet at Stutgard consist of the following pieces; viz. part of an upper jaw with two parallel grinders; two upper fore teeth almost entire, and fragments of two others: the enamel on the used part of the teeth was, as in almost all fossil teeth, slender and thin; four upper back teeth; two lower teeth; a very crooked tusk of five feet and a half long, and another four feet and a half, measured on the convex side; fragments of several other tusks; pieces of vertebræ and ribs; four shoulder blades, and pieces of some others; a piece of a *humerus*; three cubitus; six nameless bones of the right side, and seven of the left, for the most part incomplete; four heads of femurs; three femurs without the heads; a rotula; two tibias. There is also, at an apothecary's in the same city, a lower jaw and a portion of a tibia.

These bones are accompanied in the cabinet with plenty of bones of the rhinoceros, the hyæna, and animals of

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the horse kind, the stag, the ox, the hare, and small carnivorous animals. Some very large epiphyses of vertebræ might incline us to think they were of the cetaceous class of animals. There are also some fragments of human bones, to which I shall recur. Unfortunately, the different depths at which each bone was found were not accurately enough ascertained; neither were the bones which were found in the entrenchment mentioned by Reisel, sufficiently distinguished from those found out of their limits.

Canstadt is not the only place in the vale of the Neckar where similar discoveries have been made.

Near the village of Berg, above Canstadt, there is a singular mass of calcareous earth which consists of nothing else than incrustations of aquatic plants: I have often visited this place myself, and I learn from M. Autenrieth that he found a fossil skeleton of a horse there. In 1745 a tusk of fifty pounds weight was dug up in the same place; and M. Jœger found a lower jaw four years ago.

About eighteen months ago there was found, very near the walls of Stutgard, upon digging a cave, a considerable part of a large elephant's skeleton, two large tusks and a smaller one, in reddish and blueish clay.

The narrow valley of the Kocher, near Halle, in Suabia, furnished some tusks in 1494 and 1605; the latter discovery, which is still suspended in the church of Halle, weighs 500 pounds. An inscription below it informs us that there were a great many very large bones found near it. A fire having destroyed one third of this city in 1728, upon digging the new foundations plenty of fossil ivory was found, and in particular a tusk seven feet and a half long. A grinder, from the same place, is represented in the *Museum Closterianum*, fig. 8.

All the valleys of the great rivers in Germany have furnished fossil bones, as well as the places we have mentioned. In the valleys of the Danube, and through all Hungary, they particularly abound.

To return to Germany. We find a skeleton was dug up in 1722 at Tide, in the valley of the Ocker, between Wolfenbuttel

buttel and Stetlerburg: Leibnitz had previously given a drawing of a jaw-bone found at the same place.

In 1742 there was an entire skeleton discovered by Dr. Kœnig, at Osterode, at the foot of the Hartz, and at the same place where a shoulder-blade and a radius of a rhinoceros had been dug up in 1773.

In the valleys of the Elbe, besides the entire skeletons of the vale of Unstruth, mentioned above, we find the numerous collections of bones at Esperstædt, in the county of Mansfeld, between Halle in Saxony, and Querfurt, and in the vales of the Sala. It is very remarkable that some part of them was found in a quarry of hard stone, as if the animal had fallen into some crevice.

Some fossil bones have been also lately found at Dessau, upon the Elbe, and at Potzdam, at the confluence of the Havel and the Spree.

As far as concerns the valleys of the Oder, we may consult the *Silesia subterranea* of Volkman, who speaks of a humerus suspended in the church of Trebnitz, a femur in the cathedral of Breslau, and of a pretended giant dug up at Liegnitz on digging the foundations of the church, the bones of which were distributed through the different churches of the country.

The banks of the Vistula in Prussia and Poland, although much less examined than those of the rivers of Germany, also furnish us with fossil bones, which have given rise, as in other countries, to stories of giants. The banks of the Dniester also supply great quantities of these phænomena; and in 1729 great quantities were found near Kaminiek.

[To be continued.]

XXXV. *Memoir upon a Process employed in the ci-devant Maçonnais of France, to avert Showers of Hail, and to dissipate Storms. By M.* Leschevin, *chief Commissary for Gunpowder and Saltpetre at Dijon**.

It is more than five-and-twenty years since the consideration of the mischief produced by storms accompanied with hail, induced several philosophers and friends of humanity to ascertain the method of averting this destructive plague. The celebrated coadjutor of Buffon, M. Guenaut de Montbeillard, thinking that hail is only formed after violent claps of thunder, suggested, in 1776, the establishment of a great multiplicity of conductors, which, by drawing off the electric fluid, would prevent the explosion of thunderbolts, and the consequent formation of hail. His memoir, in the form of a letter to M. Guyton Morveau, was read by the latter to the academy of Dijon, and will be found at length in the *Journal de Physique*, tom. xxi. p. 146. M. Montbeillard, in support of his opinion upon the causes of hail, brings forward the observations of the first-rate natural philosophers, and suggests the most scientific and œconomical method of executing his projects.

This circumstance induced M. Guyton de Morveau, who never allows any opportunity to escape of being useful, to investigate the theory of the production of this meteor. He seconded the philanthrophic views of his fellow-countryman by developing this theory in an excellent memoir, published in the *Journal de Physique* for January 1777, p. 60, by the title of, "Letter of M. de Morveau to M. de Montbeillard upon the influence of the electrical fluid in the formation of hail."

Some years afterwards, M. Buissart, of the academy of Arras, without knowing any thing of M. Montbeillard's work, read a memoir to that society on the various advantages that might be derived from a multiplicity of electrical conductors or thunder-rods. This memoir will also be found in the above journal, vol. xxi. p. 140.

* From M. Millin's *Magazin Encyclopedique* for 1806, tom. ii. p. 5.

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Although these various works had called the attention of the public administration and of the affluent landholders to this important subject; and although, since the first publication of the ideas of Messrs. Montbeillard, Guyton de Morveau, and Buissart, innumerable disasters caused by hail in various parts of France had demonstrated the extreme utility of the measures proposed, or of any other more efficacious or simple which might be suggested; yet not a single landowner, that we know of, has to this day put in practice any method to avert this plague.

In an interesting memoir, presented to the Academy of Dijon, an 11, by M. Denize, member of the learned Society of Maçon, and containing inquiries upon the means of dispersing storms and preventing hail, we find a curious account of the custom, established in several places, of firing off powder-boxes on the approach of storms, in order to prevent the production of hail. This account, however, was not accompanied with any detail upon the process, nor even with the name of those places where it is practised; and the schemes proposed by M. Denize appeared to the academy to be accompanied with too much difficulty in their execution: his memoir, in which all the phænomena analogous to his subject are presented with much clearness, and explained according to the principles of sound philosophy, excited much interest, but did not meet with that degree of attention it seems to deserve.

I learned accidentally, some time ago, that the process indicated by M. Denize is in use in most of the communes of the ci-devant Maçonnais, and that a part of the mining powder which I send into this district of the department of Saone and Loire, is employed for the purpose of dissipating storms and preventing hail. The desire of ascertaining such an interesting fact induced me to profit by my connections with that department, in order to procure circumstantial details upon this process, and its analogy with the principles established in the memoir of M. Denize; and the conclusions he draws induced me to revise his memoir, and to examine the various methods he suggests to check a hail storm in its birth.

 M. Denize,

M. Denize, after having examined the history of antiquity to ascertain if the antients knew any thing on the subject, concludes, if they did, that they have left us nothing instructive on the subject. Among the moderns he finds no other practice resorted to than that of ringing the church bells; and he observes, with great reason, that this method of averting a storm may be regarded as purely superstitious, and as affording no physical preventive whatever.

The author then proceeds to an examination of the process resorted to of exploding gunpowder; a process, as he says, only adopted within *these few years*, and the adoption of which he ascribes to two causes: "On the one hand, the suppression of the ringing of bells since the revolution; on the other hand, some observations which lead us to think that the commotions excited in the air by considerable discharges of artillery are sufficient to prevent hail; storms being far less frequent, or at least very moderate, in the track of camps or armies*."

The investigations I procured to be made taught me, that so far from the suppression of bell-ringing having had any influence in encouraging the gunpowder process, the latter has been in use for upwards of five-and-thirty years in one of the communes of the ci-devant Maçonnais, as I shall afterwards demonstrate. I shall add to the observations which appear to M. Denize to be the second cause of the adoption of the above method, a fact which convinced me of the influence exercised upon dense clouds by strong and reiterated explosions. I was led personally to make this remark at Grenoble, where there is a school of artillery established.

The sky was pure and serene, when about nine o'clock in the morning numerous clouds began to extend over all the valley in which Grenoble is situated, and covered the mountains by which it is bounded. The instant the discharges of the field-pieces commenced, between nine and ten o'clock, the clouds opened away before us, and the sky resumed its serenity. They did not again collect until the exercise of the guns was finished.

* For some curious remarks on this subject see Philosophical Magazine, vol. iv. p. 333.

Next

Next follows in the above memoir the indication of the only preservative against hail, suggested by naturalists, being the establishment of a multiplicity of thunder-rods.

Before entering into the detail of the new methods he submits to the judgment of the learned, M. Denize proceeds to lay before them the results of his own observations upon the formation of hail. It will not be out of place to follow him in this branch of his subject.

I think we may thus state the principles upon which the author's opinion is founded:

The elements which enter into the composition of storms are, the atmospheric air, water, electricity, and caloric.

Water dilated by caloric is formed into vapours, and becomes specifically lighter than the atmospheric air; it ascends, and carries with it a quantity of electricity proportioned to the capacity it has just acquired.

If the air in which these vapours are suspended is of a temperature lower than theirs, they condense, by the disengagement of their caloric, into clouds more or less thick.

Under this new form, their dimensions being diminished, they contain a superabundant quantity of electricity, which they may get rid of by communication either with others or with the earth by means of conductors; but after a time they will be less electrified, if, by traversing some streams of air abounding in caloric, they resume their former state of dilatation.

It is from the contact of clouds variously electrified that storms are produced, the electric fluid darting successively from one cloud to another in order to obtain an equilibrium.

As the author explains the formation of drops of rain during storms, and afterwards drops of hail, in a manner peculiar to himself, that is to say, by referring their formation to the concussion occasioned in the atmosphere of the clouds by claps of thunder, I shall quote his own words on the subject:

"As soon as the thunder begins to explode in the heart of the storm, the explosion shakes every part of the surrounding air, at the same time that it suddenly diminishes its density. This concussion occasions violent vibrations in the smaller molecules of the air; consequently it detaches from them the heaviest humid particles, and forces them mutually to approach each other. They unite in virtue of their attraction, and they are immediately precipitated in drops of rain, the size of which is proportioned to the quantity of particles of water which have been united to them during their fall.

"It is generally at this moment that the formation of hail takes place."

The theory of the author for explaining the phænomena that successively take place during a storm, is precisely the same with the theory ascribed to the same phænomena by M. Guyton de Morveau in his above-mentioned memoir.

The following, according to M. Denize, are the characters according to which we may judge when it is time to put in practice the preservative processes:

"From the moment that the thick and obscure clouds begin to accumulate, if we perceive that violent and impetuous winds tend to compress them the one against the other, and to condense them strongly, and as soon as we hear the thunder roaring in the middle of these clouds, and when they appear isolated in the air, and not communicating with the earth by means of any mists or undulations, it is then that the danger threatens, and we cannot too speedily establish the most powerful and energetic conductors between these clouds and the surface of the earth."

I think we may class under three divisions the methods recommended by the author for dispersing storms: and it seems to me that, although he has followed no order in the indication of these methods, they may be reduced to the following three propositions:

1. Excite in the air strong commotions capable of shaking, if we may use the expression, the particles of water adhering to it, so as to produce an abundant rain.

We may attain this object by the sound of great bells, the reiterated noise of guns or drums, &c.; by the detonation of the fulminating powder, and by the explosion, in the middle

middle of the clouds, of rockets directed towards the place where the clouds are thickest.

2. Establish energetic conductors between the clouds and the earth, either by fires lighted from distance to distance, and kept burning by supplies of dry substances, or by the disengagement of humid vapours, or the combustion of resinous matters.

3. Draw off the electric fluid, which is in superabundance in the clouds, by a multiplicity of thunder-rods. As the storms in our country are accompanied by the east and south-east winds, it would be proper to establish in every canton, on those sides of their horizon, these conductors, which might be placed both on elevated places and on high trees: this consideration would lead to the multiplication of large trees in the above quarters, and immense advantages would consequently result in respect of the increase of fire-wood.

Such is the succinct analysis of this memoir, which evinces that its author is a philosopher familiarised in the explanation of the grand phænomena of nature: it belongs to learned men and enlightened ministers to decide how far the means proposed are practicable in the country, and compatible with the security of the inhabitants and the principles of a good government. To the hopes held out in this memoir, that one of our greatest earthly plagues may be successfully averted, the unhappy reflection is added of the dreadful accidents that may arise from the inconsiderate employment of most of the preservatives recommended by M. Denize. However that may be, it seemed to me to be interesting to lay the above analysis before my readers previous to giving the details of the process employed in the ci-devant Maçonnais for preventing storms.

It was at Vaurenard this process originated, 35 years ago. The marquis de Chevriers, a naval officer, retired upon his estate at Vaurenard, having often witnessed the ravages occasioned by hail, and recollecting to have seen the explosion of guns resorted to at sea in order to disperse stormy clouds, resolved to combat this plague by an analogous method.

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For this purpose he made use of boxes of gunpowder, which he caused to be fired off from the heights on the approach of a storm; and his attempts had the happiest effects: he continued until the period of his death, which happened at the commencement of the revolution, to preserve his lands from the ravages of the hail-storms, while the neighbouring villages frequently experienced their baneful effects. He consumed annually about 200 or 300 pounds of mining powder, which was furnished to him from the magazine at Maçon.

The inhabitants of the communes where the marquis de Chevriers's estates were situated, convinced, by the experience of a great number of years, of the excellence of this practice, continued to employ it. Their example was imitated by the surrounding communes; and the practice gaining ground, it is at this moment in use in the communes of Vaurenard, Iger, Azè, Romanèche, Julnat, Le Torrins, Touilly, Fleury, Saint Sorlin, Le Viviers, Les Boutteaux, and many others. The largeness of the powder-boxes, their charge, and the number of times they fire them off, vary according to circumstances and the position of the places. In the commune of Fleury they make use of a mortar which carries a pound of powder at a time; and it is generally upon the heights, and before the clouds have had time to accumulate, that they make the explosions, which they continue until the stormy clouds are entirely dissipated. According to the account given me by the keeper of the magazine at Maçon, the annual consumption of mining powder for this purpose is from 400 to 500 kilogrammes.

The extension of this process for these some years past, and the success with which it has been constantly accompanied, makes it desirable that it should be more generally known wherever hail-storms extend their ravages. It belongs to the learned societies to propagate, by premiums and experiments, a practice by no means so costly as to produce inconvenience, and which from its simplicity of execution is open to every country inhabitant.

XXXVI. *On*

XXXVI. *On Canal Track-Boats.* *By* ROBERTSON BUCHANAN, *Esq. Civil Engineer, Glasgow.*

To Mr. Tilloch.

DEAR SIR,

WHEN lately in Ireland I made a short excursion on the Grand Canal, which joins the Liffy with the Shannon. I was much pleased with the arrangements and punctuality observed with regard to the canal packets, or track-boats. The accommodation on board and at the hotels for the use of the passengers does very great credit to those under whose care the whole is conducted.

The construction of the vessels, both with regard to their general plan and the mechanical contrivances connected with them, seems to me to merit the attention of those who take an interest in what relates to the improvement of our inland navigation. I am therefore induced to lay before you the following details:

The general appearance of the vessels is like that of those on our canals, but the number of passengers and the length of the voyage require additional accommodation; an idea of which may be formed by inspecting the accompanying sketches, although not accurately drawn to a scale.

Fig. 1. (Plate V.) is a sketch of the plan of the vessel.

A, the principal cabin.

B, the second cabin.

C, a board which is occasionally raised up to prevent the water from the sluices from filling the vessel; an accident which I am informed once occasioned the sinking of one of these vessels and the loss of many lives, and which probably gave rise to this improvement.

Fig. 2. is a stern view.

DD, are pieces of plate iron which are let down, when necessary, to retard the vessel's motion, and are frequently of great use in entering the locks.

E, the cab-house or cooking-place, the smoke-pipe of which seems well contrived to prevent the bad effects of the wind.

Fig. 3, the smoke-pipe on a larger scale: the same contrivance

trivance is applied to the cabin stove, and I should think might, with equal advantage, be applied to dwelling-houses in situations where eddy winds are apt to occasion smoke.

But what attracted most of my attention was the manner in which the track-rope is attached to the vessels; and which, I am informed, has prevented many dangerous accidents, to which they were formerly liable. There is a contrivance by which it may be instantaneously disengaged by a kind of trigger, a plan and profile of which are sketched in figures 4. and 5.

I, represents the end of the track-rope.

K, a pin firmly connected with the frame, which is bolted to the deck above the cabin.

KL, a lever, the end K of which preponderates, and is so formed as to allow the loop on the end of the track-rope I, to remain on the pin K, while the horses are pulling properly forward; but should any accident occur, and endanger their being dragged into the water, all that is necessary is for a person to put his foot on the end L of the lever; which end being thus depressed makes the end K to rise, and along with it the loop of the track-rope is raised, which, getting to the top of the pin K, is instantly disengaged.

These contrivances (figs. 4. and 5.) are placed one on each side of the roof, at HH, so as to leave the roof of the cabin quite free and safe for the use of the passengers; and here seats are placed, where they may enjoy the air, and the beautiful scenery which so often presents itself in the progress of the voyage.

I remain, dear sir, yours, &c.

ROBERTSON BUCHANAN.

Glasgow,
November 20, 1806.

XXXVII. *Analysis of the Substance known by the Name of Turquoise.* By *M.* BOUILLON LAGRANGE*.

SEVERAL mineralogists have placed the turquoises among the calcareous bodies and those bodies which are called the

* From *Annales de Chimie,* tome lix. p. 180.

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opake gems; others, on account of their blue or green colour, have ranged them among the ores of copper.

"The turquoises," says M. Chaptal*, "are merely bones coloured by the oxides of copper. The colour of the turquoise often passes to green, which depends on the alteration of the metallic oxide: the turquoise of Lower Languedoc gives out a fetid smell on the action of fire, and is decomposed by the acids; the turquoise of Persia gives out no smell, and is not attacked by the acids. Sage supposes that in the latter the osseous part is agatised."

Plenty of turquoises are found in Persia, and none in Turkey, as the name would incline us to believe: they are dug out of two mines†; the one is called the Old Rock, three days' journey from Meched, to the north-east, near Nichabourg; the other is five days' journey from Meched, and is called the New Rock. The turquoises of the latter mine are of a bad blue inclining to white; and they are sold at a very low price. Since the end of last century, however, the king of Persia has prohibited all digging at the Old Rock except on his own account; because the Persian goldsmiths work only with the file, and do not understand the art of enamelling upon gold, and therefore they make use of the turquoises of this mine for decorating sabres, poniards, and other pieces of workmanship, in place of enamelling them. They shape the turquoises into flowers and other figures, and then insert them into enchasements.

I shall add some other details extracted from different works, and for which I am indebted to the politeness of M. Haüy, the celebrated mineralogist.

Turkis (*Turquoise*), *Reuss*, p. 511, part ii. vol. iii. "The turquoise has been always regarded as the tooth of an unknown animal, the sky-blue colour of which arises from the oxide of copper, and according to some from the oxide of iron: this has caused it to be classed in the calcareous as well as in the coppery order of bodies, as an animal petrifaction (*odontalite*).

Lommer, in the "*Abhandlungen einer privat geschell-*

* Elements of Chemistry.

† See l'Abrégé des Voyages, par M. de la Harpe, tom. vi. p. 507.

schaft

schaft in Boëhmen," vol. ii. p. 112 and 118, thinks that the turquoise is a product of art: he relates that a tooth, found in the environs of Lissa, in Bohemia, having been exposed to a violent fire under the muffle of an assay furnace, was converted into turquoise: he strongly recommends the precaution of increasing the fire gradually, so as to avoid causing the tooth to fly off in splinters.

Bruckman has given a complete history of every thing which has been written upon turquoises from the days of Pliny to those of Lommer: he names Mount Caucasus, about four days' journey from the Caspian Sea, as the native place of turquoises, where there exists, according to Chardin, a quarry of them. They are also found in Persia, Egypt, Arabia, and in the province of Samarcand.

Dombey brought some of them from Peru; a few contained native silver.

The turquoise of the western world is found in France at Simore in Lower Languedoc, in Bohemia, Silesia, and Hungary.

Demetrius Agaphi, who visited the place where the turquoises are found near Chorossan, in the neighbourhood of the town of Pischpure, relates, in the fifth volume of the *Nordischen Beytræge* 1793, p. 261, that turquoise is found in a stone as a matrix, in fragments and small pieces, and that it ought to be regarded as a particular mineral, which lies in the same beds as the opal, the chrysoprase, and the resin-formed quartz.

M. Bruckman, in Crell's *Journal de Chimie* 1799, vol. ii. pages 185 to 189, according to the account of its situation in the mine at Chorossan, and according to the analysis of Lowitz, thinks that the turquoise is not a petrification of the parts of animals, but a particular mineral.

Lowitz produced from it, by means of analysis, a good deal of argil, a little copper and iron; but he found no lime nor phosphoric acid *.

According to Meder, the oriental turquoise is found in

* I know not whether the substance analysed by M. Lowitz ought to be considered as turquoise, or rather as a particular mineral. I am inclined to think it was the latter, as I always found, in the turquoises I examined, both lime and phosphoric acid.

 a primitive

a primitive argillaceous schistus of a blueish gray or grayish black colour, which excludes all idea of petrifaction: in the same place graphic schistus and quartz are found: in the argillaceous schistus the turquoise is disseminated through it; it is the same with it in the quartz and graphic schistus.

In order to remove every idea of the turquoise being regarded as a malachite or green copper (*kupfergrün*), Meder has given the following character of it:

Its colour is the grayish green of the celadon apple: when the turquoise begins to soften in the acids, it is decomposed, and assumes a mountain green colour: when entirely decomposed it is of a yellowish greenish white, and even straw coloured.

It is generally met with disseminated in small scattered fragments, and rarely in a large mass; it is dull interiorly; its fracture is compact, the fragments indeterminate, with sharp edges, opake when it is decomposed, and more or less translucid at the edges; it becomes softer and softer according to the degrees of decomposition, and latterly becomes brittle*: its specific gravity, according to Kirwan, is from 2·500 to 2·908.

The turquoise in the mass is sometimes mixed with earthy oxidulated copper (*ziegelerz*) of a brick red.

M. Meder thinks, according to all these characters, that the turquoise ought to be placed between the opal and the apple-green chrysoprase, with which it seems to agree the most.

Lastly, the celebrated Cuvier, in the *Journal de Physique*, tome lii. p. 263, thinks that the turquoises which are found near Simore in Languedoc, and near Trevoux, are cupreous teeth of an animal which resembles that found at the river Ohio, or the mammoth of the English and the Americans, the carnivorous elephant.

M. de Reaumur is the only person who has given any details upon the mines of turquoise, and the nature of the substances found in them.

* The turquoises are not all of an equal hardness: this may be attributed to the difference of the osseous substances which serve as their base. The degree of petrifaction of these bones ought also to have an influence upon their hardness.

His

His memoir is printed among the proceedings of the Royal Academy of Sciences for the year 1715. We may therefore consult his observations for every thing that relates to the position of the mines and the extraction of the turquoises.

As to the experiments made by M. Reaumur with the view of depriving them of their colour, although not altogether conclusive, yet it will be useful to mention them here, as they are connected with the means I resorted to in order to ascertain the nature of the turquoise. I shall therefore present my readers with that part of M. Reaumur's memoir.

" The colouring matter," he says, " which fills the cellules of the turquoise, and which tinges the whole stone, is certainly a distinct substance. But is it a simple mineral substance like cobalt, or the substance of which azure and sapphire are formed, and which give such a fine blue colour to china and earthen-ware; or is it a metallic substance?" This is a question which I have not been able to resolve to my own satisfaction.

" I supposed at first that our turquoises derived their colour from copper, this metal yielding a blue and a green colour; but I saw that we might discharge the colour from them as we do from coral: of all the solvents I tried, distilled vinegar succeeded best with me. If a thickish piece of turquoise is steeped for an hour or two in this vinegar, its corners become white; and in two or three days all the surface of the stone, and even almost all its interior, assumes the same colour.

" The vinegar, besides taking away the colour, also dissolves the stone, which is always covered with a kind of white cream composed of the particles which had been detached from it. Citron juice also dissolves these kind of stones, but it only weakens their colour; and that part which is below the sort of cream above mentioned is blue when the stone has been steeped in this juice.

" As to aqua-fortis and aqua-regia they are not proper for extracting the colour from our turquoises: they dissolve very speedily the whole substance of the stone; but they supply

supply us with a method of distinguishing the turquoises of Persia from those of Europe. Aqua-fortis does not act upon the Persian ones; whence it follows, that these two kinds of stones, although similar in appearance, are nevertheless of a very different nature: it would be wrong, however, to draw an inference disadvantageous to our turquoises from this circumstance, or to think them more brittle.

" Aqua-regia, also, acts differently upon these two sorts of stones. It entirely dissolves the European, and reduces the Persian to a kind of paste whiter than the stone itself was, but which, however, is not deprived of all the blue colour.

" In general, these stones have a singular fault; their colour changes through time, without any solvent: their blue colour imperceptibly assumes shades approaching to green; and they latterly become entirely green; while the colour of all the other precious stones is unalterable. When the turquoise has become green, it is no longer of any value; for it is not yet the fashion to esteem this stone when it becomes green."

Chemical Examination of the Turquoise.

Physical Characters.—Specific gravity, 3·127. Colour, clear green and blue. Hardness, a slight degree harder than glass; very difficult to pound; its powder is of a greenish gray, and its fracture is smooth.

Chemical Characters.—Exposed to the blowpipe it loses its colour and becomes of a grayish white, but does not melt. Heated in a platina crucible it assumes the same colour, but becomes friable and is easily reduced into powder.

In this experiment it loses six per cent. of its weight. The nitric and muriatic acids dissolve turquoise entirely. The solution made by the muriatic acid is yellow; that by the nitric acid is colourless.

The nitric solution presented the following phænomena:

1. With lime water, a white flaky precipitate.

2. With ammonia in excess the precipitate was of the same colour, but more abundant. The liquid above it acquired no blueish tint.

3. Carbonate of ammonia formed also a precipitate in it.

4. With oxalate of ammonia the precipitate is very light, and very minutely divided.

5. The precipitate by the prussiate of potash is of a deep blue.

We see that these preliminary experiments already give an approximate knowledge of the constituent parts of turquoise, but they are not sufficient to lead us to a regular classification of it; for which reason, out of a large quantity of turquoises, I made choice of such as were highest coloured and hardest, and I submitted them to the following experiments:

A. 100 parts of turquoise reduced into powder were introduced into a small retort, and 300 parts of nitric acid at 36° were poured upon them. In a short time a slight effervescence took place, which continued until the solution was completed. The gas was collected by the hydrargyro-pneumatic apparatus, and it presented all the properties of carbonic acid gas.

B. This nitric solution is white, and of a syrupy consistence; it was evaporated to dryness, and the matter remaining was afterwards made red hot in a platina crucible.

C. Calcination scarcely changed its colour.

This substance was again dissolved in very dilute nitric acid, for the purpose of separating from it the iron which might have existed in it in the state of an oxide; but the whole was entirely dissolved; which evidently shows that the iron was neither in the state of a red oxide nor as a nitrate, but rather in the state of a phosphate.

D. Ammonia in excess was poured into the liquor C, which gave an abundant white precipitate. After being washed and dried, this precipitate was treated with a strong solution of potash, which dissolved a certain quantity of it. The liquor was afterwards separated from the portion not dissolved, and muriate of ammonia was added, which separated a white matter from it that presented all the properties of alumine. This substance, after calcination, weighed a part and a half.

E. The

E. The portion dissolved by the potash was also calcined, and weighed 82 parts.

F. Being desirous of knowing if the liquor in the experiment D contained any lime in solution, we poured carbonate of ammonia into it; a precipitate was obtained which, when dried and slightly heated, was found to be carbonate of lime; its weight was eight parts.

G. The liquor floating above was afterwards evaporated, but no precipitatewas formed; which led us to conclude that it contained no magnesia.

H. Being persuaded that the precipitate E contained phosphates, it was treated with the sulphuric acid. The substance was afterwards washed; and the washings being collected, prussiate of potash was poured into them, which formed a precipitate of a deep blue, the weight of which, after calcination, was one part and a half: it was red oxide of iron. (Care must be taken to heat the liquor, in order to separate the precipitate entirely.)

The liquor floating above held in solution acid phosphate of lime; this was ascertained by obtaining phosphorus from it by the help of charcoal.

I. this oxide of iron was again made red hot with a little pure potash. The matter assumed a deep green colour; the cooled mass was afterwards dissolved in water, which assumed the same colour. A little muriatic acid was added, and it then became of a fine rose colour. This experiment was repeated upon several turquoises, and the phænomenon always took place; which evidently proves the presence of a very small quantity of manganese.

K. Wishing to ascertain if turquoises contained any phosphate of magnesia, as the experiments of Messrs. Fourcroy and Vauquelin upon bones might lead us to think, I treated this substance in the manner pointed out by these chemists (*Annales de Chimie*, tom. xlvii.*), and the result was, that 100 parts of turquoise contained two parts of phosphate of magnesia.

* For M. Fourcroy's abridgment of this paper see Philosophical Magazine, vol. xxiv. p. 262.

It results from all the above experiments, therefore, that 100 parts of turquoise contain,

Phosphate of lime - - - -	80
in place of 82, found in the experiment E, deducting the above quantity of phosphate of magnesia,	
Carbonate of lime - - -	8
Phosphate of iron - - -	2
——— of magnesia - -	2
——— of manganese (an inappreciable quantity)	0
Alumine - - - -	$1\frac{1}{2}$
Water and loss - - -	$6\frac{1}{2}$
	100

Although I obtained similar products on examining several turquoises, yet we cannot decide if they are identical. The turquoises that I made use of, were exactly similar to those in the cabinet of the Museum of Natural History; and M. Haüy, whom I consulted, could not say that they were true Persian ones. M. Guyton thinks that there is a difference between the Persian and European turquoises. In his lectures on mineralogy at the Polytechnic School he has for some years maintained that the former contains silex. It is possible they may contain this substance accidentally; but I never found any in those I examined. I do not think that this difference ought to hinder mineralogists from classifying turquoise. M. Guyton himself has already placed it among the fossil bones: this celebrated chemist has also made some comparative experiments: he saw that fossil bones assumed in the fire a colour analogous to that of turquoise; that digested in water containing potash they became blue, and that this blue varied in shade and passed to a greenish or deep blue; and lastly, that bones exposed to the air became white.

Messrs. Fourcroy and Vauquelin have also observed that bones calcined strongly often assume a blueish tint; this colour appeared to them to be owing to the presence of a little phosphate of iron.

Thus

Thus there exists no doubt upon the substance which colours the turquoise. If it was necessary to add to the facts already given, I would say, that having sent M. Vauquelin the same turquoises I had analysed, he found no particle of copper in them; lastly, I ascertained that, by pouring into a solution of muriate of lime phosphate of soda and some drops of muriate of iron at the maximum, a phosphate of lime and iron was obtained of a greenish blue colour: again, by decomposing the phosphate of soda by the muriate of iron at the maximum, we may obtain a phosphate of iron which is not white, as some chemists have said, but of a blueish green colour.

The above reflections may not be very important; but I offer them with the view of showing the possibility of imitating the colour of the turquoise, and as showing at the same time that iron, in several circumstances, may yield colours similar to those produced by copper.

XXXVIII. *The Bakerian Lecture on the Force of Percussion. By* William Hyde Wollaston, *M. D., Sec. R. S.**

When different bodies move with the same velocity, it is universally agreed that the forces which they can exert against any obstacle opposed to them are in proportion to the quantities of matter contained in the bodies respectively. But, when equal bodies move with unequal velocities, the estimation of their forces has been a subject of dispute between different classes of philosophers. Leibnitz and his followers have maintained that the forces of bodies are as the masses multiplied into the *squares* of their velocities (a multiple to which I shall, for conciseness, give the name of *impetus*), while those who are considered as Newtonians conceive that the forces are in the *simple ratio* of the velocities, and consequently as the *momentum* or *quantitas motûs*,

* From the *Transactions of the Royal Society* for 1806.

 a name

a name given by Newton to the multiple of the velocity of a body simply taken into its quantity of matter.

It cannot be expected that at this time any new experiment should be thought of by which the controversy can be decided, since the most simple experiments that have already been appealed to by either party have received different interpretations from their opponents, although the facts were admitted.

My object in the present lecture is to consider which of these opinions respecting the force exerted by moving bodies is most conformable to the usual meaning of that word, and to show that the explanation given by Newton of the third law of motion is in no respect favourable to those who in their view of this question have been called Newtonians.

If bodies were made to act upon each other under the circumstances which I am about to describe, the leading phænomena would occur, which afford the grounds of reasoning on either side.

Let a ball of clay or of any other soft and wholly inelastic substance be suspended at rest, but free to move in any direction with the slightest impulse; and let there be two pegs, similar and equal in every respect, inserted slightly into its opposite sides. Let there be also two other bodies, A and B, of any magnitude, which are to each other in the proportion of 2 to 1, suspended in such a position, that when perfectly at rest they shall be in contact with the extremities of the opposite pegs without pressing against them. Now if these bodies were made to swing with motions so adapted that in falling from heights in the proportion of 1 to 4, they might strike at the same instant against the pegs opposite to them, the ball of clay would not be moved from its place to either side; nevertheless the peg, impelled by the smaller body B, which has the double velocity, would be found to have penetrated twice as far as the peg impelled by A.

It is unnecessary to make the experiment precisely as here stated, since the results are admitted as facts by both parties; but upon these facts they reason differently.

One

One side observing that the ball of clay remains unmoved, considers the proof indisputable, that the action of the body A is equal to that of B, and that their forces are properly measured by their momenta, which are equal, because their velocities are in the simple inverse ratio of the bodies. Their opponents think it equally proved by the unequal depths to which the pegs have penetrated, that the causes of these effects are unequal, as they find to be the case in their estimation of the forces by the squares of the velocities.

One party is satisfied that equal *momenta* can resist equal pressures during the same *time*; the other party attend to the *spaces* through which the same moving force is exerted, and finding them in the proportion of 2 to 1, and convinced that the *vis viva* of a body in motion is justly estimated by its magnitude and the square of its velocity jointly.

The former conception of a quantity dependent on the continuance of a given *vis motrix* for a certain *time* may have its use, when correctly applied, in certain philosophical considerations; but the latter idea of a quantity resulting from the same force exerted through a determinate *space* is of greater practical utility, as it occurs daily in the usual occupations of men; since any quantity of work performed is always appreciated by the extent of effect resulting from their exertions; for it is well known that the raising any great weight 40 feet would require four times as much labour as would be requisite to raise an equal weight to the height of 10 feet, and that in its slow descent the former would produce four times the effect of the latter in continuing the motion of any kind of machine. Moreover, if the weights so raised were suffered to fall freely through the heights that have been ascended by means of four and of one minute's labour, the velocities acquired would be in the ratio of 2 to 1, and the squares of the velocities in proportion to the quantities of labour from which they originated, or as 4 to 1; and if the forces acquired by their descent were employed in driving piles, their more sudden effects produced would be found to be in that same ratio.

This species of force has been, first by Bernoulli and afterwards by Smeaton, very aptly denominated mechanic force; and when by force of percussion is meant the quantity of mechanic force possessed by a body in motion, to be estimated by its quantity of mechanic effect, I apprehend it cannot be controverted that it is in proportion to the magnitude of the body and to the square of its velocity jointly.

But of this quantity of force Newton no where treats, and has accordingly given no definition of it. If, after defining what he meant by the *quantitas acceleratrix*, and *quantitas motrix*, he had had occasion to convey an equally distinct idea of the *quantitas mechanica* resulting from the continued action of any force, he might, not improbably, have proceeded conformably to the definition given by Smeaton, and have added,

——quantitas mechanica est mensura proportionalis spatio per quod data vis motrix exercetur;

or, if speaking with reference to the accumulated energy communicated to a body in motion,

——proportionalis quadrato velocitatis quam in dato corpore generat.

But, if we attend to the words of his preface to the first edition of his *Principia*, he evidently had no need of such a definition:

"Nos autem non artibus sed philosophiæ consulentes, deque potentiis non manualibus sed naturalibus scribentes," &c.

And again, nearly to the same effect in the *Scholium*, which follows the laws of motion: "Cæterum mechanicam tractare non est hujus instituti."

In the third law of motion he has, on the contrary, been supposed to speak of this force from an ambiguity in the signification of the words *actio* and *reactio*. By these, however, Newton certainly meant a mere *vis motrix* or pressure, as he himself explains them: "Quicquid premit vel trahit alterum, tantundem ab eo premitur vel trahitur. Si quis lapidem digito premit, premitur et hujus digitus a lapide," &c. The same meaning is equally evident from his demonstration of the third corollary to the laws, in which he asserts

serts that the *quantitas motûs* of two or more bodies estimated in any given direction is not altered by their action upon each other. The demonstration begins thus:

"Etenim actio eique contraria reactio æquales sunt per legem tertiam, ideoque per legem secundam æquales in motibus efficient mutationes versus contrarias partes." Now, if he had considered the third law as implying equality of more than mere moving forces, there could have been no occasion to refer to the second law, with a view thence to deduce the equality of momenta produced.

Some authors, however, have interpreted the third law differently, and accordingly have expressed a difficulty in comprehending the simple illustration given by Newton. When they say that action is equal to reaction, they mean not only that the instantaneous intensity of the moving forces, or pressures opposed to each other, are necessarily equal, but conceive also a species of accumulated force residing in a moving body, which is capable of resisting pressure during a time that is proportional to its *momentum* or *quantitas motûs*.

If it be of any real utility to give the name of *force* to this complex idea of *vis motrix* extended through time, as well as that of *momentum* to its effects when unresisted, it would be requisite to distinguish this force always by some such appellation as *momental* force; for it is to be apprehended that for want of this distinction many writers themselves, and it is certain that many readers of disquisitions on this subject, have confounded and compared together *vis motrix*, *momentum*, and *vis mechanica*: quantities that are all of them totally dissimilar, and bear no more comparison to each other, than lines to surfaces, or surfaces to solids.

In practical mechanics, however, it is at least very rarely that the momentum of bodies is in any degree an object of consideration; the strength of machinery being in every case to be adapted to the *quantitas motrix*, and the extent and value of the effect to be produced depending upon the *quantitas mechanica* of the force applied, or, in other words, to the space through which a given *vis motrix* is exerted.

The comparative velocities given by different quantities of

mechanic

mechanic force to bodies of equal or unequal magnitude have been so distinctly treated of by Smeaton, in a series of most direct experiments *, that it would be a needless waste of time to reconsider them in this place. So also, on the contrary, the quantities of extended mechanic effect producible by bodies moving with different quantities of impetus, have been as clearly traced by the same accurate experimentalist †.

But there is one view in which the comparative forces of impact of different bodies was not examined by Smeaton, and it may be worth while to show that when the whole energy of a body A is employed without loss in giving velocity to a second body B, the impetus which B receives is in all cases equal to that of A, and the force transferred to B, or by it to any third body C, (if also communicated without loss, and duly estimated as a mechanic force,) is always equal to that from which it originated.

As the simplest case of entire transfer, the body A may be supposed to act upon B in a direct line through the medium of a light spring, so contrived that the spring is prevented by a ratchet from returning in the direction towards A, but expands again entirely in the direction towards B, and by that means exerts the whole force which had been wound up by the action of A in giving motion to B alone. In this case, since the moving force of the spring is the same upon each of the bodies, the accelerating force acting upon B at each point is to the retarding force opposed to A at the corresponding points in the reciprocal ratio of the bodies, and the squares of the velocities produced and destroyed by its action through a given space will consequently be in that same ratio. The momentum, which is in the simple reciprocal ratio of the bodies, might consequently be increased at pleasure by the means proposed, in the subduplicate ratio of the bodies employed; and if momentum were an efficient force capable of reproducing itself, and of overcoming friction in proportion to its estimated magnitude, the additional force acquired by such a means of increase might be employed

* Phil. Trans. vol. lxvi. p. 450. † Ibid. vol. lxxii. p. 337.

for

for counteracting the usual resistances, and perpetual motion would be easily effected. But since the impetus remains unaltered, it is evident that the utmost which the body B could effect in return would be the reproduction of A's velocity. and restitution of its entire mechanic force neither increased nor diminished, excepting by the necessary imperfection of machinery. The possibility of perpetual motion is consequently inconsistent with those principles which measure the quantity of force by the quantity of its extended effect, or by the square of the velocity which it can produce.

In estimating the utmost effect which one body can produce upon another at rest, the same result is obtained by employing impetus as ascensional force, according to Huygens; for if the body A were allowed to ascend to the height due to its velocity, and if by any simple mechanical contrivance of a lever or otherwise the body B were to be raised by the descent of A, it is well known that the heights of ascent would be reciprocally as the bodies; and consequently that the square of the velocity to be acquired by free descent of B would be in that ratio, and the quantity of mechanic force would be preserved as before unaltered.

It may be of use also to consider another application of the same energy, and to show more generally that the same quantity of total effect would be the consequence not only of direct action of bodies upon each other, but also of their indirect action through the medium of any mechanical advantage or disadvantage; although the time of action might by that means be increased or decreased in any desired proportion. For instance, if the body supposed to be in motion were to act by means of a lever upon a spring placed at a certain distance from the centre of motion, the retarding force opposed to it would be inversely as the distance of the body from the centre; and since the space through which the body would move to lose its whole velocity would be reciprocally as the retarding force, the angular motion of the lever and space through which the spring must bend, would be the same at whatever point of the lever the body acted. And conversely, the reaction of the spring upon any other

other body B, would in all positions communicate to it the same velocity.

It may be remarked, however, that the times in which these total effects are produced may be varied at pleasure in proportion to the distances at which the bodies are placed from the centre of motion; and it should not pass unobserved, that, although the intensity of any *vis motrix* is increased by being placed at what is called a mechanical advantage, yet, on the contrary, any quantity of mechanic force is not liable to either increase or diminution by any such variation in the mode of its application.

Since we can, by means of any mechanic force consisting of a *vis motrix* exerted through a given space, give motion to a body for the purpose of employing its impetus for the production of any sudden effect, or can, on the contrary, occasion a moving body to ascend, and thus resolve its impetus into a moving force ready to exert itself through a determinate space of descent, and capable of producing precisely the same quantity of mechanic effect as before, the force depending on impetus may justly be said to be of the same kind as any other mechanic force, and they may be strictly compared as to quantity.

In this manner we may even compare the force of a body in motion to the same kind of force contained in a given quantity of gunpowder, and may say that we have the same quantity of mechanic force at command, whether we have 1 lb. of powder, which by its expansion could give to one ton weight a velocity sufficient to raise it through 40 feet, or the weight actually raised to that height and ready to be let down gradually, or the same weight possessing its original velocity to be employed in any sudden exertion.

By making use of the same measure as in the former cases, a distinct expression is likewise obtained for the quantity of mechanic force given to a steam-engine by any quantity of coals; and we are enabled to make a comparison of its effect with the quantity of work that one or more horses may have performed in a day, each being expressed by the space through which a given moving force is exerted. In

the

the case of animal exertion, however, considerable uncertainty always prevails, in consequence of the unequal powers of animals of the same species, and varying vigour of the same animal. The information which I have received in reply to inquiries respecting the weights raised in one hour by horses in different situations, has varied as far as from 6 to 15 tons to the height of 100 feet. But although the rate at which mechanic force is generated may vary, any quantity of work executed is the same in whatever time it may have been performed.

In short, whether we are considering the sources of extended exertion or of accumulated energy, whether we compare the accumulated forces themselves by their gradual or by their sudden effects, the idea of mechanic force in practice is always the same, and is proportional to the space through which any moving force is exerted or overcome, or to the square of the velocity of a body in which such force is accumulated.

XXXIX. *History of Astronomy for the Year* 1805. *By* JEROME DE LALANDE*.

On the morning of the 20th of October 1805, M. Bouvard discovered a comet upon the paws of Ursa Major: it was small, had no tail, and was almost round, and so faint that it was scarcely discernible with a night-glass which magnified five or six times. At 4^h 19′ in the morning it had 166° 31′ of right ascension, which I call *equatude*, and 33° 30′ of northern declination. The same night it was discovered by M. Pons, at Marseilles. We know from the public journals that M. Huth saw it also at Frankfort on the Oder. Messrs. Bouvard and Arrago observed it with the great equatorial, made this year by M. Bellet for the observatory; and they followed it as long as the bad state of the weather would permit. M. Thulis observed it at Marseilles so late as the 6th of November. Messrs. Biot and

* From *Mag. Ency.* for 1806, tom. ii. p. 92.

Arrago

Arrago ascertained its elements as follows:—Node, $11^s\ 15^\circ$ $6'$; inclination, $11^\circ\ 53'$; perihelion, $4^s\ 28^\circ\ 45'$; distance, 0·3762: passage, 18th November $1^h\ 8'\ 6''$ mean time; motion direct. This is the 95th comet according to the catalogue in my astronomy, which I have continued in the various volumes of the *Connoissance du Temps*.

The 96th comet was discovered by M. Pons, on the 9th of November, in Andromeda: he was not certain of it until the 10th. Upon that day M. Thulis found it at $16^\circ\ 38'$ of *equatude*, and at $40^\circ\ 43'$ of northern declination; and he continued observing it till the 8th of December. His excellency M. Champagny, minister of the interior, has given M. Pons a present of 300 francs in consideration of the four comets he has discovered.

On the 14th of November M. Bouvard also perceived it, and on the 16th he determined its position. It was very small, and very difficult to see, notwithstanding its nucleus. As soon as M. Burckhard had received three observations, he sent me the elements of its orbits on the same day. Those who know the difficulty of this problem will be astonished at the readiness of this able astronomer: but this was only the first essay. Messrs. Legendre, Bouvard, and Biot, afterwards calculated it: they found the node $8^s\ 10^\circ$ $32'$; inclination, $15^\circ\ 34'$; perihelion, $3^\circ\ 19'\ 26''$; distance, 0·8916: passage, 31st of December, at 8^h; motion direct. On the 22d of November M. Huth also discovered it at Frankfort on the Oder. As it approached the earth it became more beautiful: on the 5th of December it was seen with the naked eye in Pisces. M. de Flaguergues observed it on the 7th at Viviers; but it rapidly advanced towards the south, and the bad weather prevented it from being seen any more. It was seen at Greenwich, however, on the 9th of December; and by Dr. Herschel at Slough.

The most important thing in the history of astronomy is the publication of the fourth volume of the *Mecanique Celeste* of M. de Laplace, which treats of satellites, comets, refraction, of the inequalities of Jupiter, Saturn, and the moon, and which on every subject contains the greatest efforts of theory and the last degree of perfection. M. de Laplace

place there gives some new results upon the masses of the planets; and he has latterly confirmed them by the calculation of an old Chinese observation on the obliquity of the ecliptic, made 1100 years before the vulgar æra, which gives 23° 54′, while he finds 52′ by his theory, (tom. iii. l. vi. ch. 16); another observation, 100 years before our æra, gives 23° 45′, while it is 44′ by the theory: this confirms the mass of Venus made use of by M. de Laplace, and the diminution of the obliquity of the ecliptic of 52″ per annum, although several observations have seemed to give only 36″.

We have received a book (196 pages in 8vo.) containing the exposition of the operations made in Lapland for the determination of an arc of the meridian in 1802, by Messrs. Osverbom, Svanberg, Holmquist, and Palander. They made use of the new decimal measures; a procedure which I think the duty of all those who interest themselves in the progress of truth.

The result (p. 187) is, that the degree, of which the middle passes at 66° 20′, is 111477·4 metres, or 57196·2 toises; but to make this reduction they have supposed the metre to be 443·2959 lines, as the commissioners of weights and measures have made it in France; and for that purpose they took the metre at the freezing point, and the toise at the 13° of the thermometer of 80°. It seems to me most natural to take them both at the mean temperature, which is 9½°. By a mean between the observations of several years, it is the zero of my new thermometer: for this we must subtract 0·064 lines from the metre, and add 0·046 lines to the toise, according to the experiments made by M. Lavoisier, and in which I took a part in 1782; and we may make this proportion: 863·954 : 448·360 :: 854 lines are to the metre; and thus it is found to be 443·435 lines in place of 296. It is to this value that I shall change all the measures in my astronomy, as I announced to the Institute on the 28th of October. Thereby I find that we must subtract 15½ metres from the number above given, and that the degree is 57200.

The degree of 1736 having been measured at 15°, there must be added to it three toises; that of 1802, having been measured

measured at 3°, but reduced to zero, we must take from it eight toises: this augments the difference by eight toises. The former, in place of 57419, becomes 57422; and the second reduced to 111462 metres, makes 57200 toises: thus there are 222 toises too much. As this agrees with the other degrees measured, and with the oblateness given by the pendulum and the moon's parallax, we cannot refrain from adopting the new result, although it is difficult to comprehend how our academicians in 1736 came to commit so great an error. The Swedes thought that the sector of nine feet was more liable to be deranged, and less certain, than the repeating circle which they made use of.

M. de Zach has examined the work of the Swedes: he compared the angles, and the reductions: he finds the reduction imperfect; but the work is not less important or beneficial.

In order to clear up the enormous differences between the measurement of the French and that of the Swedes, I observe four things.

1. The sector was not reversed: this was too difficult.

2. The wire was suspended by a stud upon a thickish cylinder; and there might be friction.

It was not then known how important it was that the optic axis of the telescope should be parallel to the plane; two lines of difference in the position of the glasses make six minutes upon a radius of nine feet, and this difference may thence arise; there may result from this upon the distance to the zenith an error so much the greater, as probably the glass was directed to the stars by their transits to the meridian, and not by a meridian wire.

4. I remember extremely well that Maupertuis told me that he was anxious to recommence the measurement.

We have also seen in the London Transactions that Mudge has measured three degrees in England. He found 111189 metres at 52°; this agrees with the French measurements: but he found for the north part 164 metres less than for the south part, and he ought to have found 23 metres more; which announces irregularities in the interior structure of the earth and in the attractions of mountains, and which

makes

makes us wish for other measurements of degrees of longitude which are not affected by these irregularities.—Phil. Trans. 1804.

M. de Zach has continued the measurement of his degrees of the longitude and latitude of Capel, at Gotha; he hopes next year to finish 4° of longitude.

M. de Zach has been making observations at Hieres, Aix, and Avignon, for these 17 years past: he has found 897 toises for the height of Mount Ventoux. He expects to finish in Provence the celestial arc between the mountain Saint Victoire, and the pillar of Cette in Languedoc, for the degree of longitude measured in 1739, and upon which there had been always some doubt entertained.

He observed at the Isle of Planier, the most southern point in France. General Roy had formed some doubt upon the longitude of Porquerolle: M. de Zach determined it.

In the month of March there appeared a large spot with two nuclei upon the sun, which I observed at 9° to the north of the solar equator: this differs little from the beautiful spots which I made use of in determining the rotation of the sun in the memoirs of the academy for 1776, and which were from 11 to 12 degrees. This seems to confirm the discovery I then made, proving that there are in the sun some points where great spots are formed in preference to others: perhaps these spots are mountains, which attract and retain the scoriæ of this immense furnace. The parallel, which is 9° to the south of the equator, abounds most in large spots.

These spots with two nuclei, which have appeared at different epochs (*Mem.* 1776 & 1778), seem to me to overturn the system of volcanoes proposed by Herschel.

The beautiful spot of the month of March had its middle at 10½ degrees of declination. That of which I calculated the appearance (*Mem.* 1776, p. 496) had from 11° to 14°; but a spot of a minute occupies near 4°: thus the mountain which, I suppose, served it for the foundation or obstacle to arrest and to fix it by, may rather, by taking it at a different point, draw it to 2° or 3° further in one appearance than in the other.

 M. de

M. de Flaugergues again saw this beautiful spot in the sun in the month of April, and this return gave him the rotation of the sun 25 days 10^h 6′, as I have found by many observations. (*Astronomie*, art. 3276.)

M. Piazzi, the celebrated astronomer of Palermo, writes me, that he has observed the principal stars in the two seasons of the year, when the difference of the situation of the earth in its orbit ought to produce a difference upon the situation of the stars. People have been disputing for these two centuries upon this effect of the movement of the earth, which has been called the annual parallax. M. Piazzi found it in three months 1·5″ for Aldebaran, 3″ for Procyon, 4″ for Sirius, from which it follows that the stars are not distant, as was thought, more than seven millions of millions of leagues; but he proposes to continue and verify these important observations.

One of the most important works of 1805 is that of M. Legendre, entitled "New Methods for the Determination of the Orbit of the Comets,—Firmin Didot;" where he has quoted the methods of Lambert, and *Insignores Orbitæ* in the memoirs of Berlin for 1771; and he gives a new method for determining an orbit by three observations; and he has applied it to the comets of 1769 and 1781. He makes use of a method he calls that of *moindres carrés*, which also served him to determine the 45th degree of latitude, and he concluded from it that the length of the arcs of the meridian is less proper than that of the pendulum for the determination of an universal measure.

M. Gauss, already known as one of our greatest geometricians, is occupied in computing the attractions of Jupiter upon the three new planets; but as there will be several hundreds of equations, he proposes to give only the methods with which our calculators will easily determine the quantities of these equations.

"*Lilienthalische Beobachtungen der neu entdeckten Ceres, Pallas, & Juno, Von Dr. Johann Hieronymus Schröter; Göttingen*, 1805, *pp*. 336, 8*vo. Treuttel*; price 15 francs." These are observed diameters, but which appear too great according

according to Herschel's memoir. He finds 3·5″ for Piazzi for distance; diameter 587 leagues: Olbers, 4·5″; diameter 760 leagues: Harding, 3·1″; diameter 515 leagues.

In the *Bibliothèque Britannique* of the month of August there is an ephemeris of the three planets up to the month of May 1806.

The astronomical medal founded by Lalande, which is adjudged every year towards the spring equinox, has been decreed by the Institute to M. Harding for the discovery of his planet.

The prize which M. Bode had been charged to give for the best memoir on astronomy, has been raised to 600 francs.

M. Laplace read at the Institute a memoir upon the capillary tubes, in which he gives the analytical calculation of their attraction perfectly conformable to experiments. There is an extract of it in the *Journal de Physique* for January 1806. I read one myself upon levelling, where I explained the table of the level made use of in going from the north to the south, and that which must be employed when we level from the east to the west, on account of the figure of the earth; a consideration which has hitherto escaped all the authors who have written of levelling.

The fifth volume of the Institute, which appeared on the 14th of January, contains fourteen memoirs on astronomy. I have there given researches upon the motions of Mercury, Mars, and Venus; some calculations of occultations of the stars, and a description of the zodiac of Strasbourg. M. Delambre there treats of the stereographic projection, and of the astrolabe of Senezius, which M. Gail had stated to the class. There is also a memoir of M. Messier upon the transit of Mercury; observations of M. Ferrers in America; on the oscillation of Mars, by M. Duc la Chapelle; and a notice of the grand tables of logarithms which M. de Prony has caused to be calculated; and some remarks upon the history of the trigonometric tables. M. Cassini there gives the description of his compass.

The London Transactions for 1803 contain a memoir of M. Herschel upon the transit of Mercury, where he has not

 perceived

perceived any ring; and another upon the causes which make the mirrors of a telescope change their form.

In the Transactions for 1804 he speaks of the double stars which for 25 years seem to have experienced some variations, particularly in respect to the angles of distance with the ecliptic, which makes him think that they turn but so slowly that it is difficult to affirm it. He has seen one part conceal the other in one of these double stars.

M. Delambre's tables of the sun, and M. Burg's tables of the moon, have been finished and presented. M. Delambre has revised the tables of the moon in such a manner that all the equations are additive; which will save time, and diminish the risk of making errors. These tables are about to appear.

The new table of refractions of M. de Laplace, which appears with the tables of the sun, gives six-tenths of the second at 45° more than that of Bradley; but M. de Laplace informs astronomers that discordances have been often found between the two solstices, from having placed the thermometer within an observatory, instead of placing it outside, but out of sunshine.

The observations of the two solstices and the two equinoxes, made by M. Delambre, with a repeating circle, during several days, have confirmed the epochs of the obliquity of the ecliptic, which he uses in his new tables.

Epoch for 1800, $9^s\ 10^\circ\ 23'\ 32{\cdot}6''$, less by two $2''$ than in his former tables. Mean obliquity, $23^\circ\ 27'\ 57''$; greater by $4''$ than in the tables of my third edition, which have been hitherto made use of.

M. Delambre has finished the printing of the first volume of the great work on the meridian, in 750 pages: it is entitled *Base du Système Métrique Decimal*; or, *Mesure de l'Arc du Meridien*. It contains all the triangles formed from Dunkirk to Barcelona. The second volume will contain the bases, the azimuths, the latitudes, and the calculations of the triangles. Perhaps there will be a third volume afterwards added.

The new tables of Jupiter and Saturn, calculated by M. Bouvard, have been finished, and they are about to be printed.

Those

Those of Mercury, Venus, and Mars, made by M. Lalande, my nephew, will immediately follow. M. Delambre revises the tables of the satellites; and we are about to have a new and complete collection of astronomical tables, to be published by the board of longitude.

We have received the Memoirs of Berlin for 1802, which contain some observations of M. Bode, and calculations upon the planets of Piazzi and Olbers.

M. Bouvard has continued at the observatory his course of observations with excellent instruments, and they will appear in the *Connoissance des Temps* for 1808; which will be printed in the form of the Gregorian Calendar, according to the senatus consultum of the 9th of September, suppressing the republican calendar, and which is printed in our small Annuary.

In the bulletins of the academy of Montpelier there are some observations of M. Poitevin and of M. de Flaugergues.

M. Vidal, director of the observatory of Toulouse, has sent us a large collection of observations which he made in 1804, and which evince the zeal of this excellent astronomer.

The Ephemerides of Vienna for the year 1806, contain a great many longitudes determined by eclipses, in continuation of the great work of M. Triesnecker; a memoir of M. Burg, to prove that it is necessary to increase Bradley's refractions; observations made at Vienna, Buda, Prague, Cremsmunster, Carlsbourg or Alba Carolina, Naples, Palermo, Ratisbon, Amsterdam, Gotha, Milan, Munich, and Brunn, and to the south of Olmutz.

The Ephemerides of M. Bode for 1808, also contain several observations made at Berlin, Vienna, Petersburgh, Bremen, Breslaw, Prague, Vilna, Cremsmunster, Palermo, Upsal, Huth, and Danzig; and computations of the three new planets. This volume, as well as the preceding ones, shows how important it is for astronomers to be acquainted with the German language. I see with pleasure, that M. Bode approves of the name of *equatude*, which I substituted

tuted for that of right ascension, that we might have a simple name, and one not indicating a thing never seen.

M. Poczobut and M. Treschka, astronomers in the university of Vilna, have sent us a great number of observations of the new planets, made in 1803 and 1804 with excellent instruments.

M. Scarpelini has sent from Rome observations made in the observatory of the duke of Sermonata (Gaetani) upon the eclipses of the sun and moon, and the transits of Mercury over the sun.

Pope Pius VII. Chiaramonte, whose stay at Paris gave us so much pleasure, has given me orders to procure a circle, a clock, and an achromatic telescope for the observatory of the Roman college; which Messrs. Calendrielli and Conti have rendered interesting, and which cardinal Litta, prefect of the studies of the Roman college, specially protects.

The astronomers of Florence have requested me to send them a chronometer of Berthoud.

M. Ciccolini, astronomer of Bologna, has published a memoir upon the eclipse of the sun of the 11th of February, 1804; which it was thought would have been total in Italy. But that was not, nor could it be the case, according to the diameters of the sun and the moon in my tables; but the observation has succeeded as ill in Italy as in France.

M. Ciccolini has made an useful addition to the reflecting circle. He has made the back part of this instrument a quadrant, the radius of which is the diameter of the instrument itself; and with a plummet and the telescope of the circle, he is thus able to ascertain, in half a minute of time, the altitude of a star within half a degree, and at the same time the degree to which the vernier of the telescope must be placed to observe with. In this manner we avoid the disagreeable method of trial in observations of altitudes, always complained of by the most skilful astronomers and navigators. By this method also we can give a greater magnifying power to the telescope than is usual; which will be extremely advantageous in these sorts of observations.

M. Lenoir has also made at Paris a stand, with which a single

single observer is enabled to make use of the repeating circle.

M. Augustus Pictet, of Geneva, has given a method of observing meridian transits, by means of the sextants which are used in the navy. It is sufficient to fix due west a mark which will be at 90° exactly from all the points in the meridian.

M. Julian Ortez Canelas, son-in-law of the late Tofino, and director of the observatory of Spain, has sent us the observations made in the isle of Leon from 1798 to 1801.

M. Tiscar has sent us observations of eclipses, and accurate calculations, from which to deduce the longitudes.

M. de Ferrers, a Spanish officer, travelling in America, has sent us an observation of the eclipse of the 26th of June, 1805, which was not visible in Europe. He settles New York in latitude 40° 42′, and 5^h 6° 0″ west of Paris. The beginning happened at 6^h 50′ 10″ apparent time. From this I concluded the conjunction at 11^h 24′ 42″ at Paris, and the error of the tables — 46″; but as it includes the supposition of the latitude of the moon, it may be something less. M. de Ferrers adds the positions of New York and Albany, and several other observations.

We have received from Portugal the Ephemerides of Coimbra for 1805; they are similar to those of 1804, which we mentioned last year. The author has banished the signs and the seconds: every thing is expressed in degrees, minutes, and hundredths; the time is in hours, minutes, and hundredths; and all the calculations are for mean noon. The article Planets contains all the longitudes, heliocentric and geocentric latitudes; their right ascensions, their declinations, their transits, and their parallax. Instead of configurations of the satellites, we find for the times of eclipses their situation relative to the centre of Jupiter expressed by two rectangular co-ordinates, one of which has for its axis the line of the belts. The distances of the moon from the sun and the stars are only given for noon and midnight; but at the end of the calendar we see, as in the preceding volume, subsidiary tables, intended to enable mariners to dispense with the tables of logarithms in the most ordinary calculations.

culations. The first volume contains tables proper for calculating without logarithms the horary angles, the azimuths, and the semi-diurnal arcs; the distances of the moon from the stars, to reduce the apparent distances to the true, and to find the longitude of a ship; formulæ for the calculation of eclipses, in which are employed the right ascensions and the declinations of the two stars; and lastly, tables of Mars, by M. Monteiro, and which give the perturbations in ten equations.

The volume of 1805 also presents several subsidiary tables, serving to calculate, without logarithms, the right ascension and the declination of a star whose longitude and latitude are known, and that according to two different methods: we there find a table of the horary angles of the stars when they have 8° of altitude, which has been constructed to find, among the eclipses of the satellites of Jupiter, any one which we cannot expect to be able to see; a table of the distances of the centre of Jupiter from the centre of the section of the cone of shadow through which the four satellites pass; tables of latitude for these same satellites; the abscissa of the shadow, *i. e.* the path of the satellite in the shadow during the semi-duration of the eclipse. These tables serve to compute the position of the satellites with regard to the centre of Jupiter, such as we see them every six days of each month in the Ephemerides; they also serve to ascertain whether the satellite is visible at the time of immersion or emersion. For the general tables, which the author had given in the preceding volume for the aberration, he has in this volume substituted others which are all similar to those of M. Delambre, with this exception, that the quantities, in place of being in seconds, are in minutes and decimals: some of these tables are founded upon very ingenious plans of calculation, and the author has skilfully avoided the necessity of having recourse to the tables of logarithms: this calculation is not always so short as that by the known methods; but we there find for the problems given in 1804, the advantage of not having occasion for any other ephemerides; for the problems which that for 1805 contains, this advantage is much diminished, since we may refer to the volume for 1804.
The

The author has omitted the formulæ upon which these tables have been constructed. In order to appreciate their exactness they must be decomposed, which is sometimes long and difficult enough when they are founded upon formulæ simply approximative.

M. Canelas has sent us from Spain the Almanack Nomtika for 1807. That for 1808 is calculating.

The Academy of Sciences in Norway, to which councillor Hammer bequeathed 80,000 francs, with a library and a cabinet of natural history, have not forgotten to devote a part of these resources to the service of astronomy. I have already had occasion to remark that astronomy was cultivated in these terrible climates, where Messrs. Pihl, Wib senior and junior, and M. Aubert, have made many useful observations.

M. Goldbach, having arrived at Moscow on the 1st of April, fixed upon the spot for the observatory in the Botanic Garden. He expects a three-foot circle, made by Berge, the successor of Ramsden; and a five-foot transit instrument from Carey. The senator Mouravieff, curator of the university of Moscow, favours this establishment, which cannot fail to procure us some excellent observations.

M. Goldbach has determined the latitude of the university to be 55° 44′ 32″. The observatory will be one minute more northerly: thus the latitude marked in the *Connoissance de Temps* 55° 45′ 45″, nearly approaches that which we shall have to use.

On the 28th of November the grand pensionary of Holland named M. Fokker astronomer to the republic. This leads me to hope that an observatory and instruments will be procured in that country, where astronomical observations have been so long wanted for the use of their navy. I have already spoken of the zeal of M. Fokker in the History of Astronomy for 1801. (*Bibl.* p. 856.)

The emperor, in passing through Turin, promised general Menou to grant 60,000 francs for the use of the observatory; and the academy will send from France a practical astronomer, in order to show theorists the method of making observations.

observations. M. Vasalli-Eandi of the Turin Academy has promised that he will not lose sight of this useful project.

At Milan the emperor gave 8000 livres as a pension to M. Oriani, the most celebrated geometrician and astronomer in Italy.

At Lyons the municipality, whom I had solicited to repair the observatory where I made my first observation in 1748, have come to the resolution of doing so; and M. Clerc has given them the designs.

We have received from Berlin three memoirs in French: First memoir containing the exact value of the radius of curvature for all azimuths upon the surface of an ellipsoid with three arcs, presented to the Royal Society of London by Rohde, captain in the Prussian service: Potzdam 1804, 15 pages in 4to. Second memoir, upon the famous deviation towards the south or north in bodies which fall from a great height, presented to the Academy of Petersburg by Rohde. Potsdam 1805, eight pages in 4to. Third memoir, upon the absolute attractive forces or masses of the planets without satellites, upon the masses of the satellites, and upon comets; laid before the Academy of Berlin (*Ephemerides* 1807, p. 248,) by Rohde: Potsdam 1805, 28 pages in 4to.

M. Biot has published Elements of Physical Astronomy, for the Use of Schools. As they are quite different from my Abridgment of Astronomy, they do not prevent mine from being still useful to beginners.

I have given a third edition of my Astronomy for the Ladies, improved and augmented. This small book, which in two days will give any one a satisfactory idea of our science, in my estimation will be extremely useful for many persons.

M. Raymond, professor of astronomical geography, has published lectures upon the system of the world, where he explains the machines of M. Loysel, and which have the advantage of several more figures than my Astronomy for the Ladies. He will give the development of them in his subsequent course *Traité de Geodesie*, or exposition of the astronomical and trigonometrical methods applied to the measurement of ground, as well as to the preparation of the canvas for charts and maps; by L. Puissant, professor of mathematics in the Imperial Military School: 400 pages in 4to. price 18 francs; Paris, chez Courcier.

We find in this book plenty of astronomical problems necessary for drawing of charts, tables for the spheroids, and, in particular, a complete description of the repeating circle, with excellent plates.

Manuel de Trigonometrie Pratique, by M. l'abbé Delagrive, of the Royal Society of London, and geographer in the city of Paris; revised, and augmented with tables of logarithms for the use of engineers, particularly land-surveyors: by Reynaud, professor of land-surveying in the Polymathic School; 1 vol. 8vo. 352 pages, with 6 plates; Paris, chez Courcier; price 7 francs.

Trigonometrie Analytique, preceded by the Theory of the Logarithms, by Reynaud; Courcier, 1805, in 18mo. There are here added tables of logarithms made upon my small stereotype tables, but which are probably far from being so exact as mine.

M. Benzenberg has published a book in German, where we find his experiments upon the fall of bodies, *Versuche uber das Gesetz des Falls*: I spoke of this last year. He found a deviation of 12 millimetres and a half for 86 metres; but the extremes differ by six millimetres, on account of the great difficulty of the observations.

M. Benzenberg has also sent us some curious observations upon the shooting stars; he observed 500 of them in one night: he shows how they may be made useful in determining longitudes. Having so concerted with M. Brandes, who was 25 leagues distant from him, he found the distance of these meteors to be from 5 to 60 leagues.

M. Adrian Duquesnoy has published the first two volumes of Asiatic Researches, or Memoirs of the Society established in 1784 at Calcutta in Bengal; translated by A. Labaume, with notes of Messrs. Langlès, Delambre, Cuvier, Lamarck, and Olivier. There are seven volumes of this collection published, but they contain scarcely any astronomical memoirs deserving of the trouble taken by M. Delambre to render them

them interesting. We there see the mistakes of Baillie in his History of the Astronomy of the Hindoos. There are none so well acquainted with antient and modern India as the academicians of Calcutta.

M. Marquey, in 1804, published at Rome a work of Gama, upon the astronomy, the chronology, and the mythology of the Mexicans, with curious figures and interesting researches.

The 39th number of the *Notices de l'Almanac* contains the greatest part of our History of Astronomy for 1804. This collection contains every thing important in the sciences for 40 years past, and each year only costs 24 sols. If there was a coadjutor in every science as accurate as the astronomical one, this would be a precious collection.

M. Lanulin, naval engineer, has published a work entitled "Theory of the Organization of Worlds," in which he explains the motion of projection of planets by the rotation of the sun, supposing them to have been hurled from the sun by volcanoes; but we have shown him that this is impossible, and that they fall back into the sun. M. Sigorgne, although 86 years of age, has published a refutation of M. Lanulin in 55 pages 8vo. Courcier 1806.

We have been still more astonished to see a work appear entitled "On the Impossibility of the Astronomical Systems of Copernicus and Newton; by L. S. Mercier, member of the National Institute of France; Dentu 1806, 318 pages, 8vo." Here an academician, celebrated for interesting works and some sentimental dramas, occupies himself with collecting the objections of the ignorant, and the difficulties of those who do not understand astronomy: M. Mercier would have required less time to comprehend the science than to write this book.

The Astronomical and Geographical Journal, published in German by Messrs. Bertuch and Reichard, has continued its seventh year. It contains figures, charts, and portraits.

This journal, as well as that of M. Zach, which we have often mentioned, is necessary for such as would wish to know completely the progress of astronomy. It is the same with the Ephemerides of M. Bode; but the German language

guage is by far too little cultivated in France: this deficiency in the knowledge of the language might be supplied by a periodical work if it was as carefully conducted as the *Bibliothéque Britannique* of Geneva.

Since the year 1796, M. Olivarius has published at Kiel the journal entitled "*Le Nord litteraire Physique-politique, Litteraire et Rural;*" *chez Levrault, rue de Seine.*

[To be continued.]

XL. *Report of Surgical Cases in the Finsbury Dispensary from the 1st of September to the 1st of November 1806; with Observations on two Cases of Hernia which proved fatal. Communicated by* John Taunton, *Esq. Surgeon to the City and Finsbury Dispensaries, and Lecturer on Anatomy, Surgery, &c.*

In the last report (see Philosophical Magazine, vol. xxv. p. 346,) there were 132 patients under cure; 117 of whom have been cured, 5 relieved, and 10 remain on the books: these are chiefly old persons, who have been afflicted with ulcerated legs for many years, and for whom little more is to be done by the art of surgery than to keep the parts clean, which might be accomplished with more convenience to the patients in other institutions; and the letters which these persons have occupied for so many years, and are still likely to keep, might be given to such patients whose cases admit of cure or relief, by which act the utility of the institution would be extended.

Since the above report there have been admitted into this dispensary 202 patients:

Cured - -	75
Relieved - -	3
Died * - -	1
Not known -	1
Under cure - -	122
	202

* This was a case of aneurism, which it is intended to notice in a subsequent report.

 It

It was observed in the last report, that in many cases of ulcers situated on the legs, more pain had been experienced than was usual in similar cases, and that the cures were considerably protracted: much good has evidently been derived in these cases by the free administration of preparations of steel, nitric acid, and the bitter vegetable infusions.

A case of hernia, which took place in consequence of an opening in the mesentery that existed, through which a part of the small intestines protruded, became strangulated, and produced death.

Mrs. Davis, æt. 31, on the 21st of May last, when she was very warm, drank about a pint and a half of new *table beer*, then in a state of fermentation; soon after which she became very uneasy, complained of being much swelled, and was occasionally sick. She was immediately removed to her apartments in Snow-hill, and visited by Mr. Skinner, who found her vomiting large quantities of bilious matter, accompanied with violent pain in the umbilical region; a quick but not hard pulse. The saline mixture was given, and a large blister was applied to the fore part of the abdomen.

22d, No relief, or ease, had been obtained during the night: an enema was administered, which was returned without effect: the medicine was also rejected from the stomach as soon as taken.

23d, The symptoms continuing, venæsection was had recourse to, another blister applied, the enema repeated, and the saline mixture taken occasionally.

24th, No mitigation of symptoms; the enema was repeated without effect; every thing was rejected from the stomach. In the afternoon she was ordered into the warm bath: pil. ex opii gr. $\frac{1}{4}$ quilibet hora: the enema nicotianæ was administered.

25th, No relief had been obtained: the matter now vomited was of a greenish hue; the pulse was quick, but low; the pain more violent; a little water was the only thing that remained on the stomach even for a short time: the enema nicotianæ was repeated. I was requested to see her this evening for the first time, when the pain was rather abated, and

and there was a moist skin over the whole body. The opiate was continued, and the saline mixture was repeated in small doses at short intervals.

26th, Every unfavourable symptom had increased; the pulse was quick and small, attended with a dry skin: an enema was given, which returned with a small quantity of thin feculent matter, extremely offensive, and perfectly green.

27th, The pulse was quicker and weaker than on the preceding day, but the pain had nearly subsided.

28th, The pain had considerably increased, but she appeared to be sinking fast, and died at 11 o'clock at night.

On examining the body after death, the thoracic viscera were perfectly healthy and natural.

In the abdomen the small intestines were distended with flatus, and much discoloured; the jejunum and ileum were in some parts in a gangrenous state, and their coats so tender as to give way to the slightest pressure; an opening was observed in the mesentery, through which many feet in length of the small *intestines* had protruded; the distension of which, added to the subsequent inflammation of the mesentery, in all probability produced the stricture on the intestinal canal. This malformation, from all appearance, must have existed from the first formation of the parts, as there was not the least trace of laceration or accidental injury. The other viscera were all natural and healthy.

A fatal case of strangulated hernia, in which the operation might have been performed with the greatest hope of success.

Mrs. ———, æt. 35, had been the mother of several children, and had always enjoyed good health, attended divine service on Sunday, the 12th of October last, and appeared quite well after her return home; but in the evening she was seized with pain in the abdomen, attended with sickness, hiccup, and vomiting: on going to bed she complained of a small swelling in the right groin, but which was not mentioned to the professional gentleman who was called in. The symptoms continued to increase during the two

following

following days, when she was asked if she had ever been subject to hernia, which question was answered in the negative.

In the afternoon of the 15th I was requested to see her, and found her then labouring under symptoms of strangulated *hernia*, situated under poupart's ligament, on the right side, between the femoral vessels and the ilium, not larger than a moderate-sized walnut. From the smallness of the tumour, and the pain being particularly referred to the umbilicus and region of the stomach, she could scarcely be convinced that the disease arose in consequence thereof. All attempts to reduce the hernia were unsuccessful; enemas had been repeated frequently during the preceding night without having obtained the desired effect; every thing was rejected from the stomach as soon as taken. It appeared from the length of time strangulation had existed, from the remedies which had been employed, and from the present alarming symptoms, that the operation ought to be immediately performed. From the earnest entreaties of herself and husband I was induced to postpone it for two hours, giving in the mean time one of the following pills every half hour, ℞ calom. gr. viii. opi. gr. iv. f. pil. iv. These were immediately rejected by the stomach; and the operation would have been performed but for the interference of another surgeon, who sent me word "that he had reduced the hernia in part, was going to give some medicines, and had no doubt of complete success." By his improper and very reprehensible conduct the patient was taken from under my care and placed in the hands of one who promised a cure on much easier terms than myself: but the event proved the fallacy of his pretensions, as the patient died on the morning of the 17th, leaving a young family and a disconsolate husband to lament the loss of a parent and wife no more.

It was asserted by the surgeon that he had succeeded completely in reducing the hernia in his second attempt; but on examining the part after death he acknowledged his error by saying, "that it was not, nor never had been, reduced;" which was the fact.

Had

Had the operation been performed as advised in this case, in all probability the patient would have recovered; and it is only in the repeated fruitless attempts at reduction, and the consequent delay of the operation, that so many valuable lives are lost to their families, to their friends, and to the community.

JOHN TAUNTON.

Greville-street, Hatton-garden,
December 22, 1806.

XLI. *Thirty-third Communication from Dr.* THORNTON, *relative to Pneumatic Medicine.*

Suspended Animation restored by Vital Air.

MR. B———, a tradesman in Duke-street, Manchester-square, from causes which I need not enter into, resolved upon self-destruction. He deliberately retired to a garret in his house, and wrote down his causes of distraction, which he left upon the table. He sent the maid-servant down stairs, and leaped, as he thought, into eternity.

It being the top of the house, and Sunday, the rest of the family at church, his wife, child, and servant below stairs, no one heard his struggles, and he was full twenty minutes before the wife entered the apartment, saw her husband suspended, and to all appearance dead. He was cut down, and every assistance sent for in the neighbourhood. When I arrived, all pulsation had ceased, and animation appeared fled. In all cases of apparent death, time presses, and the urgency of the case demands all possible expedition. The grand question is, what is *first* to be done, and by what means? Then it may be right to premise, that I consider hanging and drowning as the same effect, but only produced by a different cause. Both may be thus defined, "A stop put to the actions of life in the body, without any irreparable injury to any vital organ; but it is requisite to put the animated machinery into action in a given time, or the power of action will be irrecoverably lost." Cullen, Boerhaave, Mr. White, &c. &c. have, on the contrary, considered suspension and drowning as extremely different; for hanging, they represent as destroying by inducing *apoplexy*. This also is the common, and a very natural opinion; but I shall here prove it to be erroneous. The following experiment was long since made by professor Monro, at Edinburgh. A dog was suspended by the neck with a cord; in a few minutes he ceased to struggle. The same experiment, exactly, was tried upon another dog, but an opening was previously made in the wind-pipe below the cord, so as to admit of air being forced into his lungs. In this state he was kept alive three quarters of an hour, when the cord was shifted below the opening into the wind-pipe, so as to intercept the ingress of air into the lungs, and the animal died in a few minutes. Upon examining the head, there was found *no rupture of vessels* in the brain. To prove this point more clearly, I shall relate a still more decided experiment by the veterinary professor, Mr. Coleman. The carotids, or arteries, which carry blood to the head, may be secured without soon materially destroying the animal functions. This operation was first done, and then the animal was hanged, and he died in a few minutes. Upon the brain being examined, there was found a *less congestion of blood* than usual, and therefore *no apoplexy*. In *apoplexy* the irritability continues several hours, whilst in drowning or hanging, the animal functions are abolished in a few minutes. In *apoplexy*, respiration together with the action of the heart and arteries go on, and the pulse often vibrates more forcibly than in health. In hanging or drowning respiration is suppressed, and the pulse obliterated. In *apoplexy* there is the *stertor apoplecticus* very distinct;—this is not to be discovered in hanging: and, lastly, after recovery from *apoplexy*, the subject is generally paralytic; whereas no such event follows recovery from suspension: and where death ensues, the appearances in the brain, in the two instances, are entirely *toto cælo* different. I press this distinction forward, as the proximate cause being ascertained leads to the right mode of treatment. This *proximate cause* I have sufficiently proved to be, in both hanging and drowning, a stoppage of air to the lungs, by which the ve-

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nous blood ceases to imbibe *oxygen,* and hence the heart, unstimulated by *oxygen,* ceases to contract, the arteries to vibrate; and hence the whole machine, although sound and entire in every part, yet on a sudden, like a clock whose pendulum is stopped, remains entirely at rest.

In the watch, if we move but the pendulum, the wheels are immediately put into motion, the clock again correctly marks its hours and minutes as before: so likewise in the animal machine, if the blood can but be influenced by *oxygen,* the heart recovers its action, the brain its energy, and the nerves their sensibility; such is the wonderful harmonious consent of parts!

From this privation of *oxygen* in drowning and suspension, we can now explain why the blood grows dark, the lips and countenance livid, and why the body quickly loses its animal heat. That the *motion* of the *heart* depends upon the *air* thrown *into the lungs,* was established by the illustrious Hook, a century ago, before the Royal Society. He laid open the thorax of a dog, cut away the ribs and diaphragm, and removing the pericardium, he kept the animal alive above an hour by means of a pair of bellows. It was observed, that as often as he left off blowing, and the lungs were collapsed, the dog fell into convulsive motions, and revived again upon renewing the blast, and the heart began afresh to beat. John Hunter repeated the same experiment, and hence concludes, as with still-born children, in suspended animation, the *first* thing is *to force air into the lungs to restore the heart's action.* For this purpose, John Hunter invented a pair of bellows of such a construction, that by one action fresh air is thrown into the lungs, and by another it is pumped out again, to imitate artificial breathing. This invention may be seen at Savigny's. But in this case no such instrument could be procured; I therefore requested a common pair of bellows, and by inserting the nozzle up one nostril, while the mouth and opposite nostril were closed by a forcible pressure, the lungs were expanded with common air.

We are now come to the next process. The heart or nerves are to be roused into action. Volatiles, especially aromatic vinegar, &c. are recommended by John Hunter; but these are not always at hand. I requested the servant, at the time she brought the bellows, to bring some *mustard* and *boiling water*. As soon as the lungs were dilated, some mustard was put up the nostril and into the mouth, and a cloth dipped in boiling water, that is water scalding hot, was put over the region of the heart and thorax.

This had a double effect: it stimulated the heart by exciting the nerves, and by imparting heat gave it more susceptibility to be acted upon. "Some years ago," says Dr. Gardiner, "the heart and part of the large vessels of a turtle were removed, with a view to examine the structure of these parts, and the circulation of the blood of that animal. Having wiped off the blood and other moisture, the heart was wrapped up in a handkerchief; but engagements in the way of my profession obliged me to postpone my curiosity till about six or seven hours after it was cut out. When I examined it, there appeared not the least signs of life; but by putting it into water nearly *milk warm* it acquired a tremulous motion; laying it on the table, and pricking it with a large needle, it palpitated several times. The palpitations were renewed as often as the needle was pushed into its substance, until it became *cold*, when it seemed to be insensible to every stimulus. But after *warming* it again in the water, it recovered its irritability, and repeated its palpitations on the application of the needle. Though no movement could be excited in it by any stimulus when cold, yet it moved several times after being placed in the *warm* water." This proves the necessity of *heat* for maintaining the full powers of the contractile living fibre.

For the purpose of stimulating the heart, *electricity* is recommended by John Hunter. But this is often as unattainable as his bellows.

Immediately upon the inflation of the lungs, and the application of the stimulus of boiling water to the chest, our patient gave signs of returning life by an apparently painful struggle; his eyes dreadfully rolled, his countenance

nance became distorted, and the struggles of convulsive nature ensued.

The legs were next rubbed with mustard, as also the thighs and arms; he was also continually blown upon by the bellows, raised upon the bed. In order to oxygenate the air the more, and stimulate the olfactory nerves, six quarts of *vinegar*, in fine sprays from a hearth-broom, were dispersed over the apartment. The acid principle of the vinegar is the product of *oxygen*, the reader already knows. In order to keep up the nice sympathy which exists betwixt the *lungs*, *heart*, and *stomach*, a cordial of very weak *brandy and water* was forced into the stomach, sweetened with sugar.

In time of health, *cordials*, as they are called, on being received into the stomach, presently manifest their enlivening effects; even long before they can be supposed to enter the lacteals and pass to the heart, their stimulus is diffused to the most remote part of the system. The rationale of this appears to be as follows: *Alcohol* has a high affinity for *oxygen*, and by combustion it has been discovered, that sixteen ounces of the former, being burnt, produce eighteen ounces of water, which we know to be composed of *hydrogen* and *oxygen*. From this rapid combustion much *caloric* or *heat* is extracted. In order to give a distinct idea of the exact quantity, one pound weight of *hydrogen* melted 295 pounds of ice, whereas in similar circumstances a pound of wax candles only melted 133 pounds. Not only a great degree of animal heat, therefore, arises from the union of *hydrogen* and *oxygen*, but the nerves which supply the heart and stomach, the par vagum, are therefore stimulated, and act by sympathy, as snuff stimulating the olfactory nerves produces sneezing, or spasm of the abdominal muscles; and it is also found that the blood by possessing *hydrogen* becomes more attractive of the *oxygen*. Dr. Withering, writing to Dr. Beddoes, says, "The experiments you wished for have been in part made. The late ingenious Mr. Spalding, who did so much in improving and using the diving-bell, and had practised with the greatest success for many years, was a

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man of nice observation, and had he not fallen a sacrifice to the negligence of drunken attendants, would have himself informed you of the circumstance. He particularly informed me, that when he had eaten animal food, or drunk fermented liquors, he *consumed the air in the bell faster* than when he lived upon vegetable food and drank only water. Many repeated trials had so convinced him of this, that he constantly abstained from the former diet whilst engaged in diving."

John Hunter recommends "the conveyance of *some stimulating substance* into the stomach, to rouse this seat of universal sympathy. This operation should be performed with all possible expedition, for fear of inducing sickness."

What this *stimulating substance* should be, he has not informed his readers. The mode of conveyance is by means of a spoon, pressing down the tongue, the patient being partly elevated. Among the class of internal stimulants are hartshorn, rum, brandy, and usquebaugh, which are powerful stimulants. But here it should be remembered, that the cessation of the actions of parts predispose them to be affected by lesser stimuli, and thrown into inordinate action by any strong stimulus. Thus, in a frost-bitten limb, the actions have ceased but the power remains. Heat only is wanting to call this power into action. But this must be gradually applied, or the highest inflammation will ensue, ending in mortification. So with those starved nearly to death by hunger. Thus may the salutary efforts of nature be overpowered by the officiousness of art, a circumstance we may have frequent occasion to observe with regret. Thus, if our stimulants are too potent, they may prove destructive, by soon exhausting the living fibre. We must recollect here, that irritability is accumulated, and therefore *weak* brandy and water is to be preferred as the stimulant, as was employed in this instance.

I now ordered more blankets to be added to the former covering.

When respiration ceases in a drowned or suspended person, the power of generating heat is suspended, and the body

body loses gradually its natural heat, until it be reduced to the temperature of the surrounding medium. During this period, if we attempt to raise the heat to the natural standard by warm pans of coals passed over the bed-clothes, or heated bottles or bricks applied to the legs and feet, we are not only disappointed in our expectations, but do an actual injury. Thus if a snake be taken from his autumnal hiding-place, and exposed to the sun's rays, or in a warm room, he quickly shows signs of returning life, and even increased powers, but he will die from the experiment; whereas another, gradually stimulated by heat, according to his state, will be restored to full animation. Thus by an ill-judged artificial and continued heat, many destroy the patient they had wished to save. But the lungs being supplied with air, the blood receives the oxygen gas (oxygen and *caloric in its latent state*) which is diffused, to be decomposed, through innumerable arteries and veins, from the centre to the extremities. Thus is the *animal heat* restored, and communicated throughout the system, and with more certainty than from any other mode. The most efficacious method therefore of imparting *heat* is, to excite the generating causes, by renewing respiration, and by placing the patient betwixt blankets, which it escapes as it becomes generated, flannel being a bad conductor of heat. To produce a quicker evolution of *animal heat*, an eighteen-gallon cask of oxygen gas was procured, and the air was taken out of the barrel by means of a common bellows, and forced into the lungs of the patient.

Each time of this process there was an evident improvement for the better. Our patient became less convulsed, and the countenance visibly lost more and more of that ghastly lividness which was the most alarming symptom. All the attendants noticed, each time, these very salutary changes.

In order to relieve the head (for although the proximate cause of suspended animation is the pressure on the trachea, or wind-pipe, yet, as probably there is *some turgescence* in the brain, owing to former pressure on the jugulars), a dozen leeches were applied to the temples, but not before the powers of life were sufficiently established, and these bled very freely*.

Then also a stimulating enema was thrown up with some tincture of aloes in it, to invite the aorta descendens. Cataplasms were likewise, for the same purpose, applied to the feet.

At night I saw my patient again, greatly restored, but still very insensible; and having got down a draught with valerian, and the volatile tincture of valerian, mixed in some camphor julep and cinnamon water, the cataplasm and blister across the thorax I ordered to be removed; but another was placed along the nape of the neck, when I left him. I was informed by his nurse, that he was restless that night, occasionally convulsed, had some intervals of sleep: but when I saw him in the morning, he was perfectly himself, yet had no recollection of any thing that had passed; the whole narrative seemed to him a dream; he felt sore all over; was very thankful to find himself in existence, and even could smile at what appeared to him a strange event, and which had brought him actually on the very brink of eternity, but without any remembered sensation.

XLII. *Notices respecting New Publications.*

The late Mr. Russell, celebrated amongst men of science for the production of the lunar globe, left at his death two lunar planispheric drawings, the result of numberless telescopic observations scrupulously measured by a micrometer: one of which drawings exhibits the lunar disk in a state of direct opposition to the sun, when the eminences and depressions are undetermined, and every intricate part, arising from colour, form, or inexplicable causes, is surprisingly developed and exquisitely delineated; the other, of precisely

* John Hunter says, "I would by all means discourage blood-letting, which I think weakens the animal principle and life itself, consequently lessens both the power and disposition to action."—*Phil. Trans.*

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the same proportion, represents the eminences and depressions of the moon, determined as to their form with the utmost accuracy, producing their shadows when the sun is only a few degrees above the horizon of each part. The former of these was beautifully and most correctly engraved by Mr. Russell, who had likewise very considerably advanced in the engraving of the latter, when death terminated his labours: it is, however, left in such a forward state, that it will be finished with the greatest exactness, and all possible dispatch.

Mr. William Russell, son of the late Mr. Russell, proposes to publish by subscription these lunar plates, which have been long promised to the scientific world: and the first engraving is now offered for their inspection. The whole will be incomparably the most complete lunar work ever offered in any age—a work, the more carefully it is examined, either as to its accuracy or elegance (effected, indeed, by extreme labour during twenty-one years), the more it will excite the wonder and admiration of the diligent inquirer.

The utility of these engravings is best expressed in the author's own words: "The principal use of the moon to astronomers is, that of ascertaining the longitude of places by the transit of the earth's shadow when the moon is eclipsed. The shadow of the earth coming in contact with many known spots, if the observation be made in different places at the same time, the longitude of each place could by this means be ascertained with great precision, provided the spots to be made choice of be sufficiently represented and recognised; but there being no faithful delineation of the moon, and the edges of those spots which are known being undefined, the observations made have not been so useful as could be wished: for this purpose, it is believed, Mr. Russell's labours will be found very useful, and will very much add to the certainty and precision of the observations on lunar eclipses; as the chief design of his planisphere, representing the moon in a state of opposition to the sun, is directed to this end, and which he has spared no pains in bringing to perfection."

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These engravings, it is expected, will not only prove of great utility to the astronomer, but lead to very important speculations in natural philosophy. The remarkable changes of forms in various eminences, the different radiations of light observable at one age of the moon and not at another, with its numerous surprising phænomena, are in these plates faithfully and fully expresssed, so as to form a work, it is presumed, highly interesting in the departments either of astronomy or natural philosophy.

The price of the work, to subscribers only, is five guineas, half of which sum is paid at the time of subscribing, when the first plate of the work is also delivered. A description will accompany the second plate.

Dr. Herdman has in the press, his second discourse on the interesting subject of The Management of Infants and the Treatment of their Diseases—written in a plain familiar style, to render it intelligible and useful to all mothers, and those who have the management of infants.

We have great pleasure in announcing that the lectures which Mr. Landseer read at the Royal Institution on the art of engraving, are in the press. We believe Mr. L. is the first person who has lectured in England on that interesting and valuable art; and as his discourses were listened to with considerable approbation, and attended by numerous audiences at the Institution, we have reason to anticipate their favourable reception with the public.

XLIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

Nov. 27. The Right Honourable the President in the chair. —Continuation of Mr. Davy's Bakerean lecture on the "Chemical Agency of Electricity." The third and fourth sections were read, consisting chiefly of numerous isolated experiments, conducted with great accuracy, on the effects of of electricity on different chemical compounds, most of which it decomposed.

On the 1st December, the society celebrated its anniversary at their apartments in Somerset-place.—Afterwards the society proceeded to the choice of the council and officers for the ensuing year; when, on examining the ballots, it appeared that the following gentlemen were elected of the council:

Of the old council—The Right Honourable Sir Joseph Banks, Bart. K. B.; Mr. John Abernethy; sir Charles Bladen, knt.; Hen. Cavendish, esq.; Edward Whitaker Gray, M.D.; right honourable Charles Greville; William Marsden, esq.; reverend Nevil Maskelyne, D. D.; George earl of Morton; William Hyde Wollaston, M. D.; Thomas Young, M. D.

Of the new council—Right honourable Charles Abbott; John Heaviside, esq.; honourable Frederick North; sir John S. Aubyn, bart.; right honourable sir William Scott, knt.; Francis lord Seaforth; Charles Shaw Lefevre, esq.; George viscount Valentia; Roger Wilbraham, esq.; C. Wilkins, esq.

And the officers were—The Right Honourable Sir Joseph Banks, Bart. K. B. President; William Marsden, esq. treasurer; Edward Whitaker Gray, M.D. and William Hyde Wollaston, M. D. secretaries.

The Copleyan medal being adjudged to T. A. Knight, esq. for his numerous discoveries in vegetable physiology, the Right Hon. President pronounced a most able and animated discourse on the valuable philosophical inquiries of that gentleman:—with all that refined taste and elevated eloquence for which he is particularly distinguished, he proceeded to take a philosophical view of the interesting experiments and discoveries of Mr. Knight; of his researches and observations on the alburnous juice of plants, in its ascent elaborating the buds and leaves, and in its descent forming wood; and of his discovery of the natural decay of apple-trees, and of the grafts which decline and become unproductive at the same time with the parent stock *. The experiments

* This fact has been doubted by several practical horticulturists; but Mr. Knight supports it, both by experiments, and by reference to historical facts.

experiments proving that all vegetables radicate by gravitation only, and not by any instinctive energy, were also noticed; that new and superior species of apples may be produced from seed; that impregnating the pollen was found to be an advantageous substiture for grafting; that the author has produced a new and highly valuable species of pears by this means; and, finally, that in the course of his numerous experiments he had produced and cultivated a new species of vines, which bear grapes superior in flavour to any others hitherto known, and capable of arriving at perfection, even in the most adverse seasons, in our climate. These, and many other discoveries, which the learned and eloquent President most ably enumerated, with equal elegance and precision, were the reasons which induced the council of the society to award the Copleyan medal to Mr. Knight, whose successful labours in this branch of natural history have at least equalled, if not surpassed, those of any other philosopher, in developing the œconomy of vegetation, and the laws of vegetable life.

We have to regret that in this slight sketch we are unable to convey any adequate idea of the elegance, spirit, and philosophical acumen of this admirable discourse, which far excelled those of his predecessor, the late sir John Pringle, bart. It is hoped that the Right Hon. President's sense of public utility will overcome every other secondary consideration in this case, and that (contrary to his former practice) he will suffer his discourse, which is so important to agriculture, to be laid before the public, and not confine it to a few fellows of the society, who, from their habits of life, however conscious they may be of its merits, are yet incapable of facilitating the practical application of the discoveries and principles which it unfolds.

In the latter, he shows that many species of apples, once highly esteemed, are now become extinct; and that several others have degenerated both in size and quality, especially that kind denominated pippins, which in 1629 were represented as the largest apples then cultivated, but which at present do not exceed the dimensions of the wild crab. The scion he considers as possessing all the diseased juices of the parent stock, and therefore subject to decay in a similar manner.

Dec. 11. The right hon. C. F. Greville, vice president, in the chair.—The reading of Mr. Davy's Bakerean lecture was continued. The fourth and fifth sections detailed several original experiments on the effects of electricity on certain chemical menstrua, in all of which the negative pole disengaged oxygen, and the positive hydrogen.

Dec. 18. The right hon. C. F. Greville, vice-president, in the chair.—The reading of Mr. Davy's Bakerean lecture was concluded. The author took a view of the influence of electricity in the mineral kingdom, its action on carburet of iron (plumbago), and various other mineral bodies hitherto not sufficiently known or examined: and also its importance as tending in a great degree to elucidate many phænomena in geology, which are irreconcilable with received hypotheses. Mr. Davy likewise mentioned how the application of electricity might be found extremely advantageous in the preparation of acids for œconomical purposes, &c. &c. In this interesting lecture, the author particularly noticed the important discoveries of Mr. Peel, and of Pacchiani, relative to the formation of alkali and acid, by the electric influence, and which have assisted him in exploring a new and untrodden region of chemical science, that "holds up a quarry to the busy mind" of future philosophers.

A paper on "The Precession of the Equinoxes," by Mr. Robertson, Savilian professor of geometry in the university of Oxford, was also read; in which the author proposed some new methods of ascertaining, with greater accuracy than has been hitherto done, the calculations of compound rotatory motion. The paper contained seven mathematical tables, of a nature not to be read.

SOCIETY OF ANTIQUARIES.

Nov. 27. Craven Orde, esq. vice-president, in the chair.—Several antient records of the reign of Edward III. were read.

Dec. 4. The same as above.—Mr. Smith exhibited to the society a silver ring about an inch in diameter, with twelve points (resembling the teeth of a wheel in clock-work), in

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one of which was a rowel or spur, which projected a little more than the others. The learned antiquary supposed this ring to have been used as a chaplet or rosario in the days of the catholic religion in this country, and that each point was to indicate a prayer, as a help to the memory, or to those who could not read. These chaplets or rosarios are, however, always divided by tens, and at the end of every ten *Ave Marias*, the devotionist says a *Pater Noster*; in this manner he proceeds till he has said a hundred and fifty *Ave Marias* and fifteen *Pater Nosters*, which completes the number of his prayers, and is what is called "saying his rosario." The rosario is also divided into *thirds*, at the end of which the supplicant makes the sign of the cross, and then continues. The fact, however, that the points on this ring are not divisable by tens, militates somewhat against the opinion of its being a rosario or chaplet.

Dec. 11. Sir H. C. Englefield, bart. vice-president, in the chair.—The right hon. the earl of Egremont furnished some additional records of the expenses and equipages of the embarkation of the earl of Northumberland, on his embassy to France in the reign of Henry VIII.

Dec. 18. The right hon. the earl of Leicester, president, in the chair.—The reading of the antient registers of the household expenses, wardrobe, &c. of the earl of Northumberland, was concluded.

The reverend Mr. Freston exhibited another of his Greek coins to the society, which, in consequence of the approaching festival, adjourned till Thursday the 8th of January, 1807.

LITERARY AND PHILOSOPHICAL SOCIETY OF NEWCASTLE-UPON-TYNE.

This respectable society has published its thirteenth year's report, by which we are happy to learn that it continues in a prosperous state.

The papers this year have not been numerous, but some of them have been of considerable importance. In April (1805), Mr. Clennell read an essay on the expediency of disclosing the processes of manufactures, a subject which was

was afterwards discussed at some meetings of the society *.—In May, an essay was read on the nature of style, and the causes of its diversity, by Mr. W. Turner, jun.—In August, Mr. G. Gray gave an account of some experiments on the root of the crocus vernus, as a substitute for wheat flour, and presented some specimens of bread made from that substance.—In September, Mr. Turner read a sketch of the history of the society from its first establishment to the end of its twelfth year, which was ordered to be printed, as an introduction to a new catalogue now preparing of the books, philosophical apparatus, and other property belonging to the society.—At the November meeting was read a memoir, by Dr. Fenwick, on the life, character, and professional merits of the late Dr. Clarke, a worthy and zealous associate of this useful body.—In Dec. Mr. Turner read an outline of the lectures on optics and astronomy proposed to be delivered in the early part of 1806, in the new institution, established under the patronage of the society. At the January meeting he communicated several improvements in arts, manufactures, and agriculture, with which he had been favoured by an ingenious correspondent: and in Feb. a letter inclosing a copy of the preliminary discourse delivered to the society of antiquaries at Perth, by their president.

SMITHFIELD CLUB.

This club, which was instituted in 1798 for encouraging the œconomic feeding of animals of the best kinds for the London markets, at their late meetings, during the show of fat cattle, have determined on a material alteration of their premiums for oxen or steers, in consequence of the Hereford breed of those animals having of late years carried off so large a portion of the six prizes annually given for the best oxen of different weights. &c. without distinction of breeds, as probably to discourage other valuable breeds: for the ensuing year six prizes, of 20 guineas each, are offered for oxen or steers of the weight of 120 stone or upwards, of each of the following breeds, viz. *Hereford*, *long-horned*,

* This essay, we understand, will be published very soon.

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short-horned, *Sussex* or *Kent*, *Devon*, and any *mixture* of *breeds*; with an additional prize of ten guineas for the best ox or steer exhibited, in claim of the above six prizes. For the convenience of graziers attending Smithfield-market, printed conditions of the premiums of the next show are left for distribution with Mr. Mitchel, draper, No. 7, Cloth-fair, near the market. Mr. Arthur Young having resigned the offices of secretary and treasurer to the club, Mr. John Farey, and Mr. Paul Giblett, were elected thereto. Thirty-one new members were balloted for and admitted. Lord William Russell presided, and will continue to do so while his noble brother remains in his government of Ireland.

FRENCH NATIONAL INSTITUTE.

[Continued from p. 187.]

M. de Beauvois having followed some mushrooms through all their developments, perceived that they changed their form in such a manner, that some botanists have placed them in different genera, according to the age at which they observed them: thus the *rizomorphus* of Porsoon is only the second age of the mushroom, which becomes a true *bolet* mushroom in the third; the *dematrium bombianum* of the same author becomes at the end of some time his *mesanterica argentea*; then it thickens, assumes cells which make it resemble a morell, and also finishes by becoming a true *bolet* mushroom. But this plant has need of a little light to enable it thus to run through all its stages.

The researches on the natural history of animals have been less numerous than those on botany, but they have not been devoid of interest.

M. de Beauvois has begun to publish the insects he collected in America and on the coast of Africa. Two numbers of them have already appeared.

M. Cuvier has continued the two great branches of researches in which he has been occupied these some years past, upon animals without vertebræ, and upon the fossil ossifications of quadrupeds.

In the former of these branches he has this year given the anatomy of seven genera, the *scyllea*, the *glaucus*, the *eolide*,

eolide, the *snail*, the *Linnea*, and the *planorbus*. The two former were very little known even externally; and the author has rectified the false ideas of naturalists concerning them.

In the second branch he has treated of the fossil bones of the bear, the rhinoceros, and the elephant.

Two sorts of bears hitherto unknown have been buried along with tigers, hyenas, and other carnivorous animals in a great number of caverns in the mountains of Hungary and Germany.

Bones of the rhinoceros and elephant are found in abundance in every part of the globe. The author has collected the names of several hundred places of both continents where the fossil bones have been found. (See Philosophical Magazine, page 158 of the present volume.)

M. Fourcroy has given a new edition of his Chemical Philosophy; the shortest, most methodical, and most frequently used elementary book in the science. The two principal agents in chemistry,—affinity, which unites the molecula of bodies, and fire, which separates them,—have been this year the subject of new and important researches.

We know that ice is lighter than water, since it floats upon it: on the other hand, warm water is generally lighter than cold: But does this liquid always condense in proportion as it cools, in order suddenly to dilate itself at the instant of its freezing?

We may doubt this; and in fact it is not the case: it is some degrees above the freezing point that water is at its *maximum* of density. M. le Febvre-Gineau has directly proved it these some years past by means of the thermometer and the hydrostatic balance; and count Rumford has suggested an experiment which makes the fact very evident.

A thermometer has its bulb placed directly under a tube suspended by a strip of linen, and the whole is placed in water ready to freeze. The surface of this water, opposite to the opening of the tube, is touched with a body heated only to three or four degrees; the molecules of water heated by this contact descend into the tube and act upon the thermometer. Thus this water, being a little warmer, is also heavier.

This experiment rests upon count Rumford's own theory, regarding the manner in which heat is propagated in the liquids. He thinks that the latter do not conduct it as all the other solid bodies do ; the metals, for instance ; and that the contact of a warm body only heats the mass of a liquid in proportion as the heated molecules at first elevate themselves in virtue of the lightness they acquire, and allow the cold molecules to occupy their place and be heated in their turn.

He has recently given us upon this doctrine an experiment more delicate and still more precise than all the preceding. A portion of water, heated to 80°, was only separated from a thermometer placed above it by a layer of cold water of some lines in thickness : not one of the heated molecules was able to descend, and the thermometer did not rise one degree.

The same chemist has made some experiments upon a question in physics which nearly concerns the doctrine of affinity, or rather the adherence of the molecules of a liquid with each other. The following is the manner in which he renders it palpable. He places oil upon water, and drops into the oil some very small grains of tin, or some very small drops of mercury : these small bodies pass through the oil quickly enough ; but when they come to the water, they stop on the surface of it, although much heavier than the water. The adherence of the water here forms something equivalent to a kind of pellicle which supports them ; but if we accumulate them, their mass acquires a weight which surmounts that adherence, they tear this pellicle, and are precipitated. The appearance of a similar pellicle is also formed at the lower surface ; for if we place water over mercury, and then drop some globules of the latter into the water, they also stop at the bottom of the water without mixing with the rest of the mercury, until they have been enlarged and become heavy enough. M. Rumford adds to these experiments the striking remark, that without this adherence the least wind would carry off the water from

from the sea and rivers much more easily than it blows away the dust; that there would be dreadful inundations every minute; that the banks of rivers and shores of the sea would be uninhabitable, and navigation impossible.

As to chemical affinities, properly so called, M. Berthollet seems to have made that subject his peculiar domain, and has imposed new laws upon these affinities, of which we have often had occasion to give an account. His first memoirs upon this subject have been announced in our reports of *An* 8 and *An* 9, and his great work *Statique Chemique*, where he has consigned all his theory, in that of *An* 11.

We know that his principal idea consists in not considering affinity, as was formerly the case, to be an absolute power; nor combinations as always uniform in the proportions of their elements.

He shows, on the contrary, that many circumstances, foreign to the chemical nature of the substances placed in contact,—such as their more or less cohesion, pressure, temperature, and, above all, their relative quantity,—influence their combinations, both as to the species and as to the proportion of the elements which enter into it.

There is indeed very seldom any entire separation; but when we place three substances in contact, for instance, there is produced a mixture, the one with the two others, according to the power of the affinities of the latter; and when four substances are put together, if there is a precipitate formed, it must be referred to the indissolubility of the combination, and not to a calculation rigorously appreciable in the sums of the affinities taken two and two.

We may easily conceive that such new views, applicable as they are to phænomena so complicated, will be susceptible for a long time of ulterior developments.

[To be continued.]

ACADEMY OF USEFUL SCIENCES AT ERFURT.

In the sitting of the above academy of the 6th of January last, Dr. Thielow, dissector in the anatomical theatre, read a memoir upon the œsophagus of a man accompanied with a crop like that of a bird. He found this anomalous conformation in a subject of 52 years of age, on the left side of the lower part of the neck. The base or lower part of this sac, which was shaped like a pear, was below the left clavicle; and the upper part, which was wrinkled (*effilée*), had an aperture furnished with a kind of valve, which was near the commencement of the gullet. Its contexture had more density than the œsophagus. From the aperture to the extremity the length of this crop was two inches and a quarter in a relaxed state; its greatest diameter was an inch and three quarters; the aperture was three-eighths of an inch.

The size of this crop was considerably increased by inflation. It might contain about a pound of water; and when filled, the water could not be evacuated without a strong pressure of the hand.

The meat and drink taken by this man passed at first into this sac, and remained there for more than two hours before passing into the stomach.

M. Thielow has developed in this memoir the reasons why this sac may be considered as a crop. He has given a description and a drawing of it in the second edition of his work upon anatomy and pathology, published last Easter fair at Leipsic *.

XLIV. *Intelligence and Miscellaneous Articles.*

VACCINATION.

A Voyage round the World, undertaken with a View of imparting the Blessing of Vaccination, by Order of the Spanish Government. Communicated by Dr. Thornton †.

On Sunday, the 7th of September last, Dr. Francis Xavier Balmis, surgeon extraordinary to the king, had the honour of

* A case somewhat similar to this has been described by Dr. William Hunter, in the London Medical Observations and Inquiries; and the preparation still exists in the Windmill-street Collection. In the case to which we allude, the formation of the bag arose from the lodgment of a cherry-stone.—Edit.

† Translated from the Madrid Gazette, published October 14, 1806.

of kissing his majesty's hand on occasion of his return from a voyage round the world, executed with the sole object of carrying to all the possessions of the crown of Spain situated beyond the seas, and to those of several other nations, the inestimable gift of vaccine inoculation. His majesty has inquired, with the liveliest interest, into all that materially related to the expedition, and learned, with the utmost satisfaction, that its result has exceeded the most sanguine expectations that were entertained at the time of the enterprise.

This undertaking had been committed to the diligence of several members of the faculty and subordinate persons, carrying with them twenty-two children who had never undergone the small-pox, selected for the preservation of the precious fluid, by transmitting it successively from one to another during the course of the voyage. The expedition set sail from Corunna, under the direction of Balmis, on the 30th of November 1803. It made the first stoppage at the Canary Islands, the second at Porto-Rico, and the third at the Characas. On leaving that province, by the port of La Guayra, it was divided into two branches: one part sailing to South America, under the charge of the subdirector don Francis Salvani; the other, with the director Balmis on board, steering for the Havannah, and thence for Yucatan. There a subdivision took place: the professor Francis Pastor proceeding from the port of Sisal to that of Villa Harmosa, in the province of Tobasca, for the purpose of propagating vaccination in the district of Ciudad Real of Chiapa, and on to Goatemala, making a circuit of four hundred leagues, through a long and rough road, comprising Oaxaca; while the rest of the expedition, which arrived without accident at Vera-Cruz, traversed not only the viceroyalty of New Spain, but also the interior provinces; whence it was to return to Mexico, which was the point of reunion.

This precious preservative against the ravages of the small-pox has already been extended through the whole of North America, to the coasts of Sonora and Sinaloa, and even to the Gentiles and Neophites of High Pimeria. In each capital a council has been instituted, composed of the principal

cipal authorities and the most zealous members of the faculty, charged with the preservation of this invaluable specific, as a sacred deposit, for which they are accountable to the king and to posterity.

This being accomplished, it was the next care of the director to carry this part of the expedition from America to Asia, crowned with the most brilliant success, and, with it, the comfort of humanity. Some difficulties having been surmounted, he embarked in the port of Acapulco for the Philippine islands, that being the point at which, if attainable, it was originally intended that the undertaking should be terminated.

The bounty of Divine Providence having vouchsafed to second the great and pious designs of the king, Balmis happily performed the voyage in little more than two months, carrying with him, from New Spain, twenty-six children, destined to be vaccinated in succession as before; and, as many of them were infants, they were committed to the care of the matron of the Foundling Hospital at La Corunna, who, in this as well as the former voyages, conducted herself in a manner to merit approbation. The expedition having arrived at the Philippines, and propagated the specific in the islands subject to his catholic majesty, Balmis, having concluded his philanthropic commission, concerted with the captain-general the means of extending the beneficence of the king and the glory of his august name to the remotest confines of Asia.

In point of fact, the cow-pox has been disseminated through the vast archipelago of the Visayan islands, whose chiefs, accustomed to wage perpetual war with us, have laid down their arms, admiring the generosity of an enemy who conferred upon them the blessings of health and life at the time when they were labouring under the ravages of an epidemic small-pox. The principal persons of the Portuguese colonies, and of the Chinese empire, manifested themselves no less beholden when Balmis reached Macao and Canton; in both which places he accomplished the introduction of fresh virus in all its activity, by the means already related; a result which the English, on repeated trials, had failed to procure

procure in the various occasions when they brought out portions of matter in the ships of their East India company, which lost their efficacy on the passage, and arrived inert.

After having propagated the vaccine at Canton as far as was possible and the political circumstances of the empire would permit, and having confided the further dissemination of it to the physicians of the English factory at the above-mentioned port, Balmis returned to Macao, and embarked in a Portuguese vessel for Lisbon, where he arrived on the 15th of August. In the way he stopped at St. Helena, in which, as in other places, by dint of exhortation and perseverance, he prevailed upon the English to adopt the astonishing antidote, which they had undervalued for the space of more than eight years, though it was a discovery of their nation, and though it was sent to them by Jenner himself.

Of that branch of the expedition which was destined for Peru, it is ascertained that it was shipwrecked in one of the mouths of the River de la Magdalena; but having derived immediate succour from the natives, from the magistrates adjacent, and from the governor of Carthagena, the subdirector, the three members of the faculty who accompanied him, and the children, were saved, with the fluid in good preservation, which they extended in that port and its province with activity and success. Thence it was carried to the isthmus of Panama, and persons properly provided with all necessaries undertook the long and painful navigation of the River de la Magdalena; separating, when they reached the interior, to discharge their commission in the towns of Teneriffe, Mompox, Ocana, Socorro, San Gil y Medellin, in the valley of Cucuta, and in the cities of Pamplona, Giron, Tunja, Velez, and other places in the neighbourhood, until they met at Santa Fé; leaving every where suitable instructions for the members of the faculty, and, in the more considerable towns, regulations conformable to those rules which the director had prescribed for the preservation of the virus, which the viceroy affirms to have been communicated to *fifty thousand* persons without one unfavourable result. Towards the close of March 1805, they prepared to continue their journey in separate tracks, for the purpose of extending themselves with greater facility and promptitude over the remaining districts of the viceroyalty, situated in the road of Popayan, Cuença, and Quito, as far as Lima. In the August following they reached Guayaquil.

The result of this expedition has been, not merely to spread the vaccine among all people, whether friends or enemies, among Moors, among Visayans, and among Chinese, but also to secure to posterity, in the dominions of his majesty, the perpetuity of so great a benefit, partly by means of the central committees that have been established, as well as by the discovery which Balmis made of an indigenous matter in the cows of the valley of Atlixco, near the city of Puebla de los Angeles, in the neighbourhood of that of Valladolid de Mechoacan, where the adjutant Antonio Gutierrez found it, and in the district of Calabozo, in the province of Caracas, where don Carlos de Pozo, physician of the residence, found it.

A multitude of observations, which will be published without delay, respecting the development of the vaccine in various climes, and respecting its efficacy, not merely in preventing the natural small-pox, but in curing simultaneously other morbid affections of the human frame, will manifest how important to humanity will prove the consequences of an expedition which has no parallel in history.

Though the object of this undertaking was limited to the communication of the vaccine in every quarter, to the instruction of professors, and to the establishment of regulations which might serve to render it perpetual; nevertheless the director has omitted no means of rendering his services beneficial, at the same time, to agriculture and the sciences. He brings with him a considerable collection of exotic plants. He has caused to be drawn the most valuable subjects in natural history. He has amassed much important information; and among other claims to the gratitude of his country, not the least consists in having imported a valuable assemblage of trees and vegetables in a state to admit of propagation, and which, being cultivated in those parts of the peninsula that are most congenial to their growth, will render this expedition as memorable in the annals of agriculture as in in those of medicine and humanity. It is hoped that the sub-director and his coadjutors, appointed to carry these blessings to Peru, will shortly return by way of Buenos-Ayres, after having accomplished their journey through that viceroyalty, the viceroyalty of Lima, and the districts of Chili and Characas; and that they will bring with them such collections and observations as they have been able to acquire, according to the instructions given by the director, without losing sight of the philanthropic commission which they received from his majesty in the plenitude of his zeal for the welfare of the human race.

EXTRAORDINARY PHÆNOMENON.

The most singularly formed individual in the world, pehaps, exists at this moment at Void, a town in the second division of the department of the Meuse. The writer of this article vouches for the truth of it: the facts are supported by the testimony of respectable medical gentlemen, the mayor of the town, and personal examination of this extraordinary man.

This unfortunate being enjoys good health, although deprived of the ordinary means of voiding his excrements. He has lived more than half a century, notwithstanding his mouth performs by turns the labours of mastication and dejection. Being thus deprived in a manner of all the parts of the body from the chest downwards, he constantly sits in a small cart, which is drawn by children through the streets, and he subsists by begging.

His name is Claud Rouget, a native and inhabitant of Void, and he is 59 years of age. In his youth he experienced a gradual and long continued compression. This compression was felt from the xiphoid cartilage and over the whole extent of the lower belly, so that the pylorus, all the viscera of that region, such as the intestines, the liver, the spleen, the kidneys, the bladder, the glands of the pancreas and of the mesentery, and all the secretory organs, experienced such an alteration, that they are as if totally annihilated. The abdomen is glued to the spine of the back; all the

the lower extremities are atrophous; the anus is obliterated and quite close.

This unfortunate individual only prolongs his existence by means of the glands of the stomach, which pump up a slight portion of chyle, diluted by the salivary and gastric juices. In half a quarter of an hour after having taken food, he voids it by the mouth, in the state of a thick emulsion, with as much and even more ease than by the ordinary method. The bile, this animal soap and the glands of the lower belly, not concurring to the extraction of the nutritive parts of his food, the voiding of it is thus facilitated, and obliges him to eat frequently.

This simple account of it may excite surprise and curiosity; but we presume that such of our readers as are devoted to the study of nature will make some observations on the subject.—*Magazin Encyclopedique,* 1806, tome i. p. 418.

ART OF MEMORY.

Germany.—A new branch of science is begun to be studied in Germany. It is the science called by the antients *mnemonica,* or the art of memory. We find in Herodotus, that it was carefully taught and practised in Egypt, whence it was transplanted into Greece. This historian attributes the invention of it to Simonides; but this opinion is refuted in a dissertation published by M. Morgenstern, of Dorpat, upon *mnemonica.* He there asserts, that this science is more intimately connected with the Egyptian hieroglyphics than is generally thought, and that this connection may help to explain them. However the case may be, this singular art, so long neglected, has reappeared in Germany with some eclat. M. Aretin, who may be accounted the restorer of it, has recently had M. Kæstner, a clergyman, as his pupil, whom he has permitted to teach his new doctrine at Leipsic; at the same time exacting a promise from him not to suffer his pupils to write down his lectures. M. Kæstner travels about like Dr. Gall.

According to a book written, it is said, by a child of twelve years of age, and mentioned in the Leipzic catalogue

logue for the last September fair, *mnemonica* is a true science, and may be taught by means of seventeen different rules, and which will give a memory to individuals of every age.

NEW COMET.

A small comet has been discovered by M. Pons, at Marseilles, on the 10th of November. According to Lalande's enumeration this is the 97th.

LECTURES.

Mr. Brookes's Spring Course of Lectures on Anatomy, Physiology and Surgery, will commence on Wednesday the 21st of January, at Two o'clock in the Afternoon, at the Theatre of Anatomy, Blenheim-street, Great Marlborough-street.

In these Lectures the Structure of the Human Body will be demonstrated on recent Subjects, and further illustrated by Preparations, and the Functions of the different Organs will be explained.

The Surgical Operations are performed, and every Part of Surgery so elucidated as may best tend to complete the Operating Surgeon.

The Art of Injecting, and of making Anatomical Preparations, will be taught practically.

Gentlemen zealous in the pursuit of Zoology, will meet with uncommon opportunities of prosecuting their researches in Comparative Anatomy.

Surgeons in the Army and Navy may be asssisted in renewing their Anatomical Knowledge, and every possible Attention will be paid to their Accommodation as well as Instruction.

Anatomical Converzationes will be held Weekly, when the different Subjects treated of will be discussed familiarly, and the Students' views forwarded——To these none but Pupils can be admitted.

Spacious Apartments, thoroughly ventilated, and replete with every convenience, will be open at Eight o'clock in the Morning, for the purposes of Dissecting and Injecting, where

where Mr. Brookes attends to direct the Students, and demonstrate the various parts as they appear on Dissection.

An extensive Museum, containing Preparations illustrative of every part of the Human Body, and its Diseases, appertains to this Theatre, to which Students will have occasional admittance.—Gentlemen inclined to support this School by contributing preternatural or morbid Parts, Subjects in Natural History, &c. (individually of little value to the possessors,) may have the pleasure of seeing them preserved, arranged, and registered, with the names of the Donors.

The inconveniencies usually attending Anatomical Investigations, are counteracted by an Antiseptic Process, the result of Experiments made by Mr. Brookes on Human Subjects at Paris in the year 1782; the account of which was delivered to the Royal Society, and read on the 17th of June, 1784. This method has since been so far improved, that the florid colour of the Muscles is preserved, and even heightened. Pupils may be accommodated in the House.—Gentlemen established in Practice, desirous of renewing their Anatomical Knowledge, may be accommodated with an Apartment to Dissect in privately.

Mr. Taunton will resume his Winter Course of Lectures and Demonstrations, on Anatomy, Physiology, Pathology, and Surgery, on Saturday the 31st of January, 1807, at eight o'Clock in the Evening, precisely; at No. 21, Greville-street. The Lectures will be continued at the same hour every Tuesday, Thursday, and Saturday.

Particulars may be known by application to Mr. Taunton, Greville-street, Hatton-garden.

Mr. Blair's Lectures on Anthropology, or the Natural History of Man; illustrated by anatomical preparations, &c. (for the information of scientific and professional gentlemen, amateurs of natural history, students in the liberal and fine arts, &c.) will recommence on Tuesday evening, the 27th of January, at the Bloomsbury Dispensary, No. 62, Great Russel-

Russel-street; to be continued every succeeding Tuesday and Friday evening, at eight o'clock precisely, until the termination of the course, which consists of about twenty lectures, delivered in the following order:

I. Preliminary Observations, on the Structure of Animals and Vegetables in general. (This introductory lecture is open to visitors, without tickets.)

II. Classification of the Organs and Functions of the Human Body.

III. Description of the Bones, Cartilages, Ligaments, and Fasciæ.

IV. Enumeration and Action of the Muscles; illustrated by various Drawings, Casts, and a living Muscular Figure, &c.

V. Remarks on the Application of Anatomy to the Arts of Painting and Sculpture.

VI. On the common Integuments, Membranes, and Cavities.

VII. The Organs and Theory of Digestion, Nutrition, and Absorption.

VIII. The Organs and Phænomena of Circulation; including an Account of the Properties and Uses of the Blood, and the Modes of applying a Tourniquet in Cases of violent Hæmorrhage.

IX. On the Glands, Secretions, and Excretions.

X. The Organs and Theory of Respiration and Speech; with Remarks on the Production of Animal Heat.

XI. The Doctrine of Procreation, Gestation, and Parturition; accompanied by Suggestions on infantile Life, Growth, Maturity, and Decay.

XII. The Structure and Functions of the Brain and Nerves.

XIII. On the Organs and Theory of Hearing, Smelling, and Tasting.

XIV. Description of the Mechanism and Uses of the Eye; with Hints on several of the Causes and Remedies of depraved Vision.

XV. The Sense of Touch or Feeling, and the supposed physical Causes of Bodily Perception.

XVI. On the Agency and corporeal Effects of the Human Passions.

XVII. Of Physiognomy and Craniognomy; with concluding Observations on the Study of animated Nature, as a Branch of Liberal Education.

Twenty transferable tickets are given for two guineas, which admit gentlemen to all the lectures delivered in one season; and, after having attended a single course, the complete set of tickets may be renewed, for their own use, by an advance of one guinea.

Further particulars, with a printed Syllabus of the whole Course (price 5s. in boards), may be had at Mr. Blair's house, No. 69, Great Russel-street, Bloomsbury-square.

LIST OF PATENTS FOR NEW INVENTIONS.

To James Frederick Matthey, of Suffolk-street, Charing-cross, in the city of Westminster, lieutenant in De Meuron's regiment; for various improvements upon fire-arms and guns of all descriptions. December 4.

To Samuel Williamson, of Knutsford, in the county of Chester, weaver; for an improvement in weaving cotton, silk woollen, worsted, and mohair, and each of them, and every two or more of them, by looms. December 4.

To William Hyde Wollaston, of the parish of St. Mary-la-Bonne, in the county of Middlesex, gentleman; for an instrument whereby any person may draw in perspective, or may copy or reduce any print or drawing. December 4.

To William Speer, of the city of Dublin, esq. now residing in Crown-street, in the city of Westminster; for his new art, method, or process, of purifying, refining, and otherwise improving fish oils and other oils, and converting and applying to use the unrefined parts thereof. December 13.

To Thomas Scott, of Clerkenwell-close, in the county of Middlesex, musical instrument-maker; for an improved musical instrument called a flageolette English flute, or an instrument on the flageolette principle, so constructed as a single instrument that two parts of a musical composition can

can be played thereon at the same time by one person. December 13.

To Ambrose Bowden Johns, of Plymouth, in the county of Devon, bookseller; for certain compositions, and a mode of manufacturing the same for covering and facing houses and various other useful purposes. December 22.

To William Bell, of the town of Derby, engineer; for an improvement upon and an addition to smoothing irons, planing irons, and various edge-tools applicable to many useful purposes. December 22.

To Anthony George Eckhardt, of Berwick-street, Golden-square, in the county of Middlesex, gentleman, F.R.S. and member of the Society of Haerlem; for certain improvements in the mode of covering or inclosing books, whereby their contents will be secured from the observation of any person but the owner, and will also be preserved from injury. December 22.

To Anthony George Eckhardt, of Berwick-street, Golden-square, in the county of Middlesex, gentleman, F.R.S. and of the Society of Haerlem, and Joseph Lyon, of Millbank-street, Westminster, in the said county of Middlesex, cooper; for their new method of manufacturing pipes for the conveyance of water under ground different to the present pipes. December 22.

Charles Schmalcalder, of Little Newport-street, in the parish of St. Ann, Soho, in the county of Middlesex, mathematical and philosophical instrument-maker; for a delineator, copier, or proportionometer, for taking, tracing, and cutting out profiles; as also copying and tracing reversely upon copper, brass, hard wood, card paper, paper, asses' skin, ivory, and glass, to different proportions directly from nature, landscapes, prospects, or any other objects standing or previously placed perpendicularly; as also pictures, drawings, prints, plans, caricatures, and public characters. December 22.

METEOROLOGICAL TABLE,
BY MR. CAREY, OF THE STRAND,
For December 1806.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Nov. 27	46°	48°	46°	30·02	0	Rain
28	46	57	51	29·90	10	Fair
29	52	59	42	·70	7	Showery
30	41	46	46	·78	10	Showery
Dec. 1	50	59	51	·02	7	Fair; a violent storm in the night
2	40	42	41	28·80	21	Fair
3	40	44	40	29·67	10	Cloud
4	38	47	44	·72	0	Rain
5	50	54	50	·48	16	Fair
6	50	50	44	·35	5	Cloudy
7	40	47	42	·34	0	Rain
8	40	45	42	·12	0	Rain
9	43	45	40	·50	0	Rain
10	38	44	40	·40	15	Fair
11	41	48	40	·40	5	Fair
12	41	48	54	·42	10	Fair
13	55	54	46	·09	0	Stormy
14	45	45	44	·49	0	Rain
15	44	45	50	·70	11	Fair
16	52	55	55	·69	5	Stormy
17	54	55	53	·72	0	Rain
18	53	54	54	·74	0	Rain
19	47	48	43	·65	6	Fair
20	44	51	44	·32	12	Fair
21	42	46	34	·23	0	Showery
22	49	54	50	·56	4	Cloudy
23	54	55	55	·98	0	Rain
24	55	47	44	30·28	15	Fair
25	55	54	43	·12	6	Cloudy
26	40	46	42	·26	10	Fair

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

XLV. *Discovery of a new Vegetable Principle in Asparagus* (Asparagus sativus *of Linnæus*). *By M.* Vauquelin*.

On examining the products of the vegetable kingdom more attentively than was formerly the custom, modern chemists have distinguished a great number of products unknown to the antients; but for a long time no immediate principle has been found in any vegetable so singular and interesting as that which we are about to mention.

Last summer M. Robiquet, a young chemist who joins to a solidity of reasoning the greatest accuracy in making experiments, by desire of M. Parmentier submitted the juice of asparagus to the chemical analysis, of which he has given the results in the *Annales de Chimie* †.

During a journey I lately took to the country, having left in my laboratory a certain quantity of the juice of asparagus concentrated by evaporation, I observed in it a great variety of crystals, two of which varieties appeared to belong to new substances: as they had a different form, transparency, and taste, it was easy to separate them.

The one of these species, perfectly white and transparent when it had crystallized several times, has a fresh taste, a little nauseous, exciting the secretion of the saliva: it is hard, brittle, and has a regular form.

The other species, although equally white, is not so transparent nor so hard, nor is it crystallized in the same form; on the contrary, it is without consistency, crystallized in fine needles, having a perceptible saccharine taste, and analogous to that of manna.

M. Robiquet, on making the experiments above mentioned, had perceived the former of these substances; but he thought it was an ammoniacal salt; because at that time, having only obtained a very small quantity and imperfectly purified, it retained, to all appearance, among its flakes some traces of a salt with a base of ammonia, with which the juice of asparagus abounds; and this circumstance deceived him.

* From *Annales de Chimie*, tom. lvii. p. 18.

† See the present volume, pages 33 and 115.

 Since

Since that period M. Robiquet and myself jointly submitted this substance to new experiments, the principal of which are subjoined. The form which it assumes in its crystallization, according to M. Haüy, to whom we sent a certain quantity, springs from a straight rhomboidal prism, the grand angle of the base of which is about 130 degrees.

This substance is soluble in water in a middling degree, and its solution gives no sign of acidity or alkalinity: the infusion of galls, the acetate of lead, the oxalate of ammonia, the muriate of barytes, and the hydrosulphuret of potash, cause no change in the solution of this substance; and it is not soluble in alcohol.

These experiments indicating that the substance in question is not a salt with an earthy base, we triturated a certain quantity of it with caustic potash and a little water, to see if ammonia was disengaged from it; but we discovered no sensible traces: the potash, as we thought, rendered it more soluble in water.

Seeing, therefore, that it contained neither an earth nor any ammonia, we endeavoured to ascertain the existence of the alkalis in it, and for this purpose we burned a great quantity of it in a platina crucible; it swelled considerably, at first exhaling pungent vapours, which affected the eyes and nostrils like the smoke of wood; it furnished plenty of charcoal which had no taste, and which upon incineration left but a very imperceptible trace of earth, which is certainly foreign to it.

Towards the end of the decomposition of this substance, the smell which is liberated from it is a little analogous to that of animal matters, and it is likewise a little ammoniacal.

The nitric acid decomposes this substance; nitrous gas is disengaged; the liquor assumes a yellow colour and a bitter taste like that of animal substances; when the action of the nitric acid is finished, lime is abundantly disengaged from the ammonia of the liquor.

This alkali, therefore, is formed during the operation we have related, since the substance of asparagus affords no perceptible traces of it before.

This substance is not an acid, since it does not redden

the

the tincture of turnsole, and since it has not the taste common to all these bodies in a more or less remarkable degree.

It is not a neutral salt, since it contains neither an earth nor an alkali; but as it furnishes, by means of fire, the same products as the vegetables, we are obliged to regard it as an immediate principle of asparagus.

It is probable that it is composed, like the vegetable products, of hydrogen, oxygen, and carbon, in particular proportions: it is not less probable that there is also a small quantity of azote in it; this seems at least to be indicated by the smell which is disengaged from it by heat, and by the ammonia which it forms with the nitric acid.

Although we obtained a sufficiently large quantity of this substance, we could not submit it to a great number of experiments, because the greatest part of it was wasted in the laboratory; and the small quantity we gave M. Haüy to determine the form of it only remained: we purpose continuing our researches as soon as the asparagus season returns, but we thought it our duty to give an account of our progress hitherto.

We shall also ascertain whether or no this singular substance exists in other vegetables.

As to the saccharine matter which we also found in the juice of asparagus, we had not enough of it to recognise what kind of sugar it was; in the mean time we are of opinion that it is manna.

It may be concluded from the above experiments, that, besides the principles discovered in asparagus by M. Robiquet, there exists a principle crystallizable like the salts, which is, however, neither an acid nor a neutral salt, and the solution of which is not affected by any of the re-agents generally employed to ascertain the presence and the nature of the salts dissolved in water; and that there also exists another saccharine principle, which seems to have an analogy with manna.

XLVI. *Upon the Affinities of Bodies for Light; and particularly upon the refractive Powers of different Gases* *.

[Concluded from p. 158.]

THE authors † of the memoir now before us commence the second part of their work by a very ingenious and just observation. "The action of bodies upon light," they say, "is not exercised in a sensible manner except at very short distances: the intensity of this action is necessarily connected with the nature of the particles of the bodies, and with their arrangement; that is to say, with their most intimate properties: so that the philosopher who observes the refractive powers of substances in order to compare them with each other, acts exactly like the chemist who presents one and the same base successively to all the acids, or one and the same acid to all the alkalis, in order to determine their respective powers and their degrees of saturation. In our experiments the substance we present to all the bodies is light, and we compute the action which they exercise upon it by their refractive power; that is to say, by the increase of effect which the action of their particles tends to impress on them.

"There is here a particular advantage, which is not to be met with in the same degree in any chemical experiment; it is the almost inconceivable intensity of the action of bodies upon light, an intensity which sometimes goes the length of impressing upon it, in an infinitely small instant of time, a velocity double of that which it has in space; and which at least always modifies it in a sensible manner, even in bodies of the weakest refractive power."

The diversity of the velocities impressed upon light by the various refracting bodies presents a very extensive scale, upon intermediate points of which all these may find a place at great intervals, and which thus presents a method of distinguishing them by this particular character, and even of pursuing them, in some measure, in various combinations.

* From *Biblioth. Brit.* vol. xxxii.

† Messrs. Biot and Arrago.

The

The authors of the memoir, having ascertained by their experiments the powerful action of hydrogen upon light, naturally infer from this, that it is the presence of this principle in water, in gums, oils, and other inflammable substances, which gives them that great refractive power which Newton has so well observed. This refracting influence of hydrogen is eminently conspicuous in ammonia, which is composed of hydrogen and azote; the refractive power of this gas being double that of the air, and surpassing that of water.

The great velocity, and the extreme tenuity of the molecules of light, give it a particular advantage in this mode of research, where it is employed as a re-agent, the greater or less degree of condensation of the constituent principles of a body having little influence upon its refractive power in comparison with that produced by the affinity of these same principles with light, excepting certain extraordinary cases in which the condensations are very considerable. In every case, by multiplying the refractive power of each principle by the ponderous quantity which enters into the combination, the sum of these products gives the refractive power of the compound.

In a simple mixture, without intimate combination among the component parts, the refractive power observed in the compound is exactly equal to that which the calculation gives according to the proportion of the constituent principles; thus the refraction of the common air is exactly equal to what ought to be produced by a mixture of 0·21 of oxygen in volume with 0·787 of azote and 0·003 of carbonic acid. By calculating the refraction according to the relative quantity of these principles, we obtain it as exactly as by direct observation.

This law holds in combinations in which condensation is not very strong: for instance, in ammoniacal gas, in which the constituent principles, the azote and the hydrogen, are reduced to one-half of their total volume by the effect of condensation, the refraction observed is exactly what agrees with a mixture of 0·805 of azote (in weight), and of 0·195

 of

of hydrogen. Thus supposing that the composition of ammonia was unknown, but that barely the nature of its principles was known, their relative proportion might be discovered by the proof of their refractive power as well as by chemical analysis.

And in water composed of hydrogen and oxygen, in which there is a strong, intimate, and very dense combination relatively to its component parts, the same law still holds, with some slight difference only, which may arise either from the uncertainty even of the experiment which Newton himself made to determine the refractive power of water at a time when the necessary instruments were much less perfect than they are at present, or rather from the influence of the state of condensation of the molecules of the liquid compared with the state of gaseous dilatation. Thus the refractive power of water, calculated according to the proportions given by Humboldt and Gay-Lussac in their excellent memoir upon eudiometry, is = 1·50, that of the atmospheric air being 1. But according to Newton, and by employing for the mean refraction that of yellow light, it would be = 1·73; stronger than the preceding by about one-eighth of the total value. Far from being astonished at this difference, we ought to be surprised that it is not more considerable, when we reflect upon the enormous condensation experienced by oxygen and hydrogen thus combined.

After having proved the exactitude of this new process of analysis by a comparison of its results with those already obtained by the ordinary chemical analysis, it became interesting to endeavour to apply it to the solution of the grand ænigma of the composition of the muriatic acid. The authors therefore proved, not without great difficulties in the manipulation, its refractive power in the state of gas. They found it a little more considerable than that of azote; which proves that the above acid is not a compound of azote and oxygen, since the presence of the latter principle would diminish instead of augmenting the refraction of the former. Nor is it an oxide of hydrogen less oxygenated than in water, as has been supposed from some late Galvanic experi-

 ments;

ments; for its refraction is much weaker than that of this liquid.

The proof of the refracting power of the carbonic acid gas was doubly interesting, as a confirmation of the analysis of Lavoisier, as well as because carbon enters into the composition of a great number of transparent bodies, and constitutes, according to modern experiments, almost the whole body of the diamond, the refractive power of which has been remarked by Newton; a result which might be compared with that obtained from experiments upon carbonic acid gas.

Here an anomaly presents itself. The refractive power of the carbonic acid gas is a little less than that of the atmospheric air, and a little greater than that of oxygen. If we admit as the most probable chemical constitution of this gas that which results from the analysis of Lavoisier, we shall ascertain that it is composed of 0·76 in weight of oxygen and of 0·24 of carbon. It results from this that the refractive power of carbon is = 1·44; that is to say, less than that of water. Every other proportion, into which less oxygen entered, would give to carbon a refractive power still weaker.

If this result gave rise to doubts upon the proportion admitted above, between the two components of carbonic acid, we might verify it by this simple method—by examining if the refractive power of certain liquid or solid substances into which carbon enters in a known proportion, with other component parts, of which the relative proportion and the individual refractive power have been determined; by examining, we say, whether the refractive power of these really agrees with the results of the calculations founded upon the analysis of Lavoisier, which are presumed to be exact.

We have as yet, unfortunately, but a very small number of accurate analyses of those substances into which carbon enters in a remarkable quantity. These analyses have been made by Lavoisier, Berthollet, Fourcroy, and Vauquelin. We have also the observations of Newton upon the refractive powers of compounds.

 Thus,

Thus, according to Lavoisier, oil of olives is composed of 0·21 of hydrogen (in weight), and 0·79 of carbon. But on combining according to these relations the refractive force of hydrogen as it results from the observations of the authors of the memoir, and that of carbon, as concluded from the trial made on carbonic acid, we find the refractive force of the oil of olives to be = 250, that of the air being 1. But Newton's observations give 2·73 for the refractive power of oil of olives. The difference between calculation and observation is, therefore, scarcely one-ninth of the total value; and it takes place in circumstances which condensation tends to explain. The following is a verification sufficiently satisfactory:

The analysis of alcohol, also made by Lavoisier, presents another mean for making a similar proof. This liquid is composed, according to him, of 0·544 of oxygen, 0·166 of hydrogen, and 0·29 of carbon. According to these proportions, the refractive power of alcohol is found upon calculation to be = 1·94, that of the air being 1. But the experiments of Newton give = 2·23 for alcohol. The difference is one-eighth of the total amount; but the difference of the results may be explained by a condensation of the change from the gaseous to the liquid state. In the last place, the chemical analysis of gum given by Messrs. Fourcroy and Vauquelin admits of trying the refractive analysis of the same solid as a verification of this new process. According to these chemists, gum is composed of 0·6538 of oxygen, 0·1154 of hydrogen, and 0·2308 of carbon. From this the refractive power of gum deduced from calculation would be = 1·63; according to Newton it is = 1·89. The difference of about one-seventh is again, in some measure, favourable to the explanation given.

The want of an exact analysis of other substances into which carbon enters, does not allow us to push the parallel of the two methods any further. But we may say that in these very substances the refractive powers observed place them in the order indicated by the combined influence of their elements. Thus the refraction is strongest where hydrogen

drogen predominates, weakest where carbon prevails, and still weaker where oxygen abounds. Thus ether refracts more than alcohol; and the essence of turpentine more than the fixed oils: carburetted hydrogen refracts less than pure hydrogen, and so much the less as the proportion of carbon is increased in it. These views, which by themselves furnish but a very slight induction, assume some importance when they agree with the results obtained by a rigorous calculation.

But the diamond here presents a very great exception. The experiments of Newton give as its refractive power = 3·2119, that of the air being 1; this amount is almost double of that just now assigned to carbon. Condensation to the solid state will not explain so great a difference; for in gum, for instance, which is also a solid, we do not see that this circumstance has an influence equally disproportioned: wax, which is equally solid, ought to refract much more than oil of turpentine, for it contains also more carbon; nevertheless it refracts much less, and far less, for a stronger reason, than the diamond.

"What ought we to conclude," say the authors, "from the detail we have given? Is not the diamond a pure carbonate? and does not its great refractive power display the presence of hydrogen, the most energetic of all other bodies in the power of refraction?"

It would be necessary, according to the experiments of Newton, that the diamond should contain more than a third of its weight of hydrogen, in order to explain, by the presence of this ingredient, its great refractive power. If we want to reduce this number proportionally to the small differences remarked between the calculation and the theory, we should carry it to one-fourth, but we cannot descend lower without being in opposition to all the other results.

The authors, on this occasion, request the first class of the Institute to repeat, by a direct experiment, the analysis of the diamond with all the precaution which should attend their suspicions of the presence of hydrogen. We know that

that their request has been complied with, and we may expect some excellent results from their labours.

In the mean time they invite chemists to continue their scrupulous researches into the composition of bodies. "On our part," they add, "we shall neglect nothing in order to multiply our observations upon solid bodies, liquids, and aëriform fluids; and perhaps we may still owe some useful result to the happy analogy unfolded to us by Newton. These researches seem already sufficiently advanced to present a method of verifying to a certain point the chemical analyses of transparent bodies; and it is, perhaps, a result singular enough in itself that we may penetrate so far into the composition of bodies, and ascertain, in a sufficiently connected manner, the nature and proportions of their principles by the geometrical instrument called a repeating circle."

The authors remark that the results they have obtained are very favourable to the system of the emission of light, and appear to be contrary to that of the undulations of light. "In fact," they say, "in the first system one should conceive that the refracting powers of compounds ought to depend on those of their principles. The combination of the attractive powers should take place proportionally to the masses; and the little influence of condensation only proves the prodigious relative distance of the particles of light, as well as their extreme tenuity relative to the molecules of bodies and to the distances which separate them; circumstances which are already indicated by many other phænomena. But if we supposed with Huyghens and the partisans of his doctrine, that light is produced by the vibrations of a very elastic substance, without transmission of substance, we could no longer conceive this relation so simple of compounds with their component parts; the condensation or the dilatation of the mediums ought necessarily to have a very complicated influence upon the progress, the direction, and the velocity of the numerous waves which are propagated: and what would not this influence be in the passage from the gaseous to the liquid state, when the constituent principles are collected under a volume two or three thousand

sand

sand times less than their primitive volume? in the same manner as takes place, for instance, in the composition of water."

The well-known phænomena of the prismatic dispersion of light, a dispersion which varies in general with the refractive powers of the mediums, are also connected with the chemical composition of bodies, and seem to indicate that their molecules have an action a little unequal over that of light: this same inequality may arise from a method of analysis analogous to that now developed, but still more subtle; and by observing the dispersion which takes place in liquids, from it we might infer what would take place in the component gases if their smallness of density permitted its effect to be appreciable. Thus we should have, for instance, the dispersive power of the atmospheric air according to that of the oxygen and the azote observed in the liquid composed of these two gases.

The labours of which we have now given an account are as applicable to astronomy as they are to chemistry. The refractive force of the atmospheric air, which is given by these experiments, is one of the most delicate elements of the theory of refractions. It is capable of being determined in two ways; by a great number of celestial observations, and by the direct and terrestrial method employed by our authors. M. Delambre, to whom astronomy is so much indebted, has just published on this subject new researches for his solar tables according to the formulæ of M. de Laplace; and the coefficient he has deduced from the comparison of more than 500 observations is that which has been employed in the *Mécanique Céleste.* But it happens, by a very remarkable coincidence, and well calculated to merit confidence, that one single terrestrial observation of our authors is sufficient to give the coefficient of M. Delambre: not one of them differs from it in any sensible measure; and the mean result deduced from their aggregate would not produce a difference of one-eighth of a second upon the position of the stars observed at 45° of altitude *.

The

* In order to give an idea of the progress of these experiments, and the precision

The result obtained by the authors of the memoir upon the exact proportion maintained to the last degrees of rarefaction between the refractive power of the air and its density, is not one of the least interesting consequences of their labours. We may, however, admit this principle without

precision we may hope for in their results, we here subjoin one of them accurately transcribed, with the note of the authors which accompanies it.

" *Observations made upon the Refraction of the Atmospheric Air at different Densities.*

Exterior barometer.	Thermometer attached to the exterior barometer, centigrade division.	Interior barometer of the prism.	Thermometer of the interior barometer, centigrade division.	Deviation observed caused by the want of parallelism in the faces.	Deviation calculated by the mean refractive power of the air, or L = 0·000294958	Difference or deviation owing to the non-parallelism of the faces of the prism.	Deviation owing to the defect of the parallelism of the faces, such as was observed directly.
0m·7654	+ 5°	0m·526	+ 6°	1′ 37·1″	1′ 52·3″	15·2′	16·6″ } March 7, 1806.
0·7654	+ 5	0·283	+ 6	3 29·2	3 45·8	16·6	
0·7658	+ 5	0·120	+ 6	4 45·8	5 2·4	16·6	
0·7660	+ 5	0·020	+ 6	5 31·8	5 48·9	17·1	
0·7662	+ 4·5	0·005	+ 5	5 43·5	5 58	14·5	} Mar. 9, 1806.
0·7548	+ 4·8	0·4055	+ 5·4	2 29·2	2 44·1	14·9	
0·7551	+ 4·8	0m·2425	+ 4·8	3 45·0	4 0·9	15·9	
0·7549	+ 4·8	0m·6130	+ 4·8	0 48·6	1 6·3	17·7	

"There is none of these experiments in which the difference of the calculation and of direct observation exceeds 2″, although the densities of the interior air had been taken in very different proportions. These results equally confirm the exactitude of the coefficient which expresses the refractive power of the air, for the freezing point and pressure 0m, 76. This coefficient, such as we give it here, results from the experiments we made with the prism devoid of air. It differs very little from that given by M. Laplace, after Delambre in the *Mécanique Céleste*, tom. iv. p. 246, which is L = 0·000294047; the difference would scarcely give one-eighth of a second upon the height of the pole at Paris; and it is not possible to answer for so small a quantity in astronomical observations."

fear,

fear, as well in the theory of astronomical refractions as in the reduction of observations. Thus the tables of refraction published by the Board of Longitude in France, and which are founded upon this law, upon the coefficient of M. Delambre, and upon the beautiful analysis of M. de Laplace, may be regarded as being as perfect as astronomers can ever require.

Lastly, the coefficient of the barometer, as well as the exact relation of the specific gravities of the air and mercury, is a determination of some utility to astronomers, since upon that depends the height of the atmosphere (being supposed homogeneous), a height which is one of the elements of the theory of refractions.

"We are of opinion," the authors conclude, "that we may still deduce from our researches one other important truth; but it is requisite that we should recall the general results found by naturalists and chemists upon the nature and constitution of the atmosphere.

"Mr. Cavendish is the first who endeavoured to establish that the proportions of the two elements of the atmospheric air were constant, in spite of the distance of places and the difference of temperatures. The observations since made by M. de Mairy in Spain, M. Berthollet in Egypt and in France, Mr. Davy in England, and by Beddoes on the air brought from the coast of Guinea, have confirmed this grand result. But one of the finest experiments made on this subject is that of Gay-Lussac, who, having been elevated alone in a balloon to the height of 6900 metres, the greatest ever attained by any person, brought some atmospheric air from these regions. This air, being analysed on his return, comparatively with that on the surface of the earth, gave the same principles in the same proportions: this proves that the chemical constitution of the atmosphere at these great heights is the same as at the surface of the earth. This result has been since confirmed by the experiments made by Messrs. Humboldt and Gay-Lussac on eudiometry. The air of the surface of the earth, analysed at different days, at various hours and temperatures, presented no change in its composition: it always contained 0·21 of oxygen in volume,

0·787

0·787 of azote, and 0·03 of carbonic acid. We ourselves had occasion to verify this grand law of nature in a journey we made to the Alps last year. The atmospheric air, analysed in places the most distant from each other, in deep valleys and on high mountains, on the banks of the lake of Geneva and at Neufchatel, in the glacieres of Chamouny, at Col de Baume, in the Valais, upon the great St. Bernard, at Turin, and at Grenoble, always presented to us the same composition. But since we have found that the refractive power of the air corresponds to that of the constituent principles which compose it, and that it may be deduced from them exactly, it also results from this fact, that this refractive power is the same over all the world at equal densities; and thus the tables of refraction, calculated by the geometricians and the astronomers of Europe, may extend, without modification, to all the countries in the world, provided that the refractive power of the air is not changed by the effects of heat: this is what the experiments we purpose making this summer will enable us to decide.

" In the present memoir we have endeavoured to present to natural philosophers and to chemists some useful results founded upon scrupulous calculations and precise observations. We have endeavoured to determine, by direct experiments, all the physical facts which serve as the foundation of the theory of the atmospheric refractions, and which hitherto had been concluded from observations. In this respect we have it particularly in view to answer the questions proposed by the author of the *Mécanique Céleste* in his tenth book, and to fix the points to which he had called the attention of philosophers."

XLVII. *Memoir upon living and fossil Elephants.* By *M.* Cuvier.

[Concluded from p. 211.]

The British islands, which from their local situation do not seem to have ever had any living elephants, present us with a great number of fossil ones.

Sloane

Sloane possessed a tusk, dug up from a bed of gravel in Gray's-inn-lane, London, 12 feet below ground. He had another, also, from the county of Northampton, found in blueish clay, below layers composed of 14 inches of vegetable earth, and 30 of flint mixed with earth *.

A grinder from the same, and of 14 laminæ, was found further down, being under 16 feet of vegetable earth, 5 feet of sandy earth mixed with flint, 1 foot of black sand mixed with small stones, 1 foot of small gravel, and 2 feet of large gravel, where the tooth was, and below it alone was found blue clay †.

In the year 1630 a portion of a cranium was found at Gloucester with some teeth; and a lower grinder has been dug up at Trentham, in the county of Stafford ‡.

In 1700 several very large bones, one of them a humerus, were dug up at Wrebness, near Harwich, upon the river Stowre, 15 or 16 feet below the surface, in a bed of gravel §.

At Norwich, in the county of Norfolk, in the year 1745, there were found a grinder weighing 11 English pounds, and several large bones ||.

I have myself at this moment before my eyes, owing to the kindness of M. G. A. de Luc, the metacarpal bone of a little toe of the right fore foot, found at Kew, 18 feet below ground, one foot and a half of which was composed of mould, 5 feet of reddish sandy clay, very fit for making bricks of; 8 feet of siliceous gravel, and 3 feet of reddish sand, which rests upon clay. This sand contained many other ossifications: among others, the nucleus of a horn of the ox kind; and in another pit, in the same field, there was found a tusk, which broke upon being taken out. The clay contained shells, and among others some nautili ¶.

The small island of Sheppy, at the mouths of the Thames and Medway, furnished a vertebra, a femur, a tusk, &c. in a place washed by the tide **.

* Natural History of Northamptonshire, by Morton, p. 252. † Ibid.

‡ Plot's History of Staffordshire. § Phil. Trans. vol. xxii. no: 274.

|| Phil. Trans. vol. xlv. art. xxi..

¶ These details are extracted from a letter with which I was favoured by M. de Luc, dated Geneva, December 6, 1805.

** Phil. Trans. vol. xlviii. p. 626, 627.

Mr.

Mr. Peale mentions, still more recently, some bones found in Salisbury Plain, near Bristol, and in the Isle of Dogs *. Dom Calmet, in his Dictionary of the Bible, speaks of a giant found in the neighbourhood of Salisbury, near the famous Stonehenge.

Pennant procured two grinders and a tusk from Flintshire. They were extracted, by some miners, from under a lead mine 118 feet deep, in a bed of gravel; and among the upper layers there was one of calcareous stone 10 or 12 feet thick: a stag's horn was found along with them. I suspect much that this position has not been well described; it is, perhaps, the only one of its kind.

Ireland has furnished elephant bones in its southern parts. There were four fine jaw-bones dug up in 1713 at Magherry, eight miles from Beltarbet, in digging the foundations of a mill †.

Scandinavia, although extremely unfit to breed living elephants, contains plenty of fossil ones.

M. Quensel, superintendant of the cabinet of natural history of the Academy of Sciences at Stockholm, has had the goodness to send me the drawing of a large lower jaw in the above cabinet: it was found in a hillock of sand, near the river Jic, in Ostrobothnia.

J. J. Dœbeln has already described some gigantic bones dug up, in 1733, at Falkenberg, in the province of Halland. To judge of them from the drawings, they must be a first rib, a metacarpal bone, and a nondescript bone of an elephant.

The giants' bones dug up in Norway, spoken of by Pontoppidan in his Natural History of Norway, must be nothing else than elephants' bones.

Thomas Bartholin speaks of an elephant's jaw-bone which was sent from Iceland to Resenius, and given by the latter to the cabinet of the university of Copenhagen. It was petrified into silex.

Sloane had some in his cabinet altered in the same manner; but he has not informed us of the cause of it.

* Historical Disquisition on the Mammoth, p. 7. Note.

† Phil. Trans. vol. xxix. no. 349.

Pontoppidan

Pontoppidan also mentions after Torfæus a cranium and a tooth found in Iceland of a prodigious size.

Of all the countries in the world, the vast empire of Russia contains the greatest quantities of fossil bones, particularly in those provinces where we might least expect to find them, the frozen regions of Siberia.

Russia in Europe contains great quantities in several places; an immense quantity was found in 1775 at Swijatowski, 17 wersts from Petersburg.

There is in the Petersburg cabinet a tusk from the neighbourhood of Archangel, in the valley of the Dwina. Corneille le Brun mentions some tusks found near the surface of the ground at Vorones upon the Tanais. There is an immense collection of them, as well as of the bones of various other animals, upon the banks of the Tanais, near the town of Kostyusk.

M. Pallas, in his recent travels through the southern provinces of Russia, mentions several places between the Tanais and the Wolga; particularly the environs of Pensa, and two other places nearer the Wolga.

As to Asiatic Russia properly so called, the testimony of travellers and naturalists agrees in representing that region as swarming with fossil elephants.

This phænomenon is so general there, that the inhabitants have invented a fable to explain it; and they tell us that these bones and tusks belong to a subterraneous animal living in the manner of the mole, but never being permitted to see the light of day. They have named this animal *mammont*, or *mammouth* according to some, from the word *mamma*, which signifies *earth* in some Tartar idioms; according to others, from the Arabian word *behemoth*, employed in the book of Job for a large unknown animal, or *mehemoth*, an epithet which the Arabs are accustomed to add to the name of elephant (*fihl*) when it is very large.

They describe the tusks found in Russia by the appellation of mammont horns (*mammonto vakost*): these tusks are so numerous and so well preserved, particularly in the southern parts, that they are employed in the same manner as fresh ivory; and they form an article of commerce so important, that the czars formerly reserved the monopoly of it to themselves.

It was the profit they produced which perhaps excited the searching for them, and which occasioned the discovery of so many of these bones in that vast country; adding to these circumstances, that the immense rivers which run into the Frozen Sea, and which are prodigiously swelled at the time of a thaw, and carry away large portions of their banks; and thus every year immense quantities of bones are discovered, besides those which are found in digging wells, &c.

We ought not to believe that these animals have been simply led from India by the rivers of the neighbouring mountains, because this would still take place at the present day, as lately observed by a respectable author *.

M. Pallas informs us that there is not in all Asiatic Russia, from the Don or the Tanais to the extremity of the promontory of the Tchutchis, any brook or river upon the banks or beds of which elephants' bones, and those of other animals foreign to the climate, have not been found.

But the higher regions, and the primitive and schistous chains of mountains, want them, as well as marine petrifications, while the lower declivities and the vast sandy plains furnish them wherever they are intersected by brooks or rivers, which proves that they would be found in abundance in the rest of their extent also if dug for.

There is but very few elephants' bones in such places as are low and marshy: thus the river Ob, which flows sometimes through low and marshy forests, only contains them in such places "ubi adjacentes colles arenosi præruptam ripam efficiunt." Strahlenberg made the same observation several years ago upon the manner in which these bones are brought to light in consequence of inundations.

They are found in every latitude, and it is from the north that the best ivory comes, because it has been less exposed to the action of the elements.

A circumstance which, independently of this prodigious abundance,

* Patrin Hist. Nat. des Mineraux, tom. v. p. 391, et sequent.: also Nouveau Dict. des Sc. Nat., art. *Fossiles.*

abundance, excludes every idea of expeditions conducted by men is, that in some places these bones are mixed with an innumerable quantity of bones of other savage animals large and small.

What is still more remarkable is, that they are often found in beds filled with marine bodies, such as shells, &c. The above is an extract of the details of M. Pallas.

One particularity not less striking than any other related to us by this great naturalist is, that in some places elephants' bones have been found having still some fragments of flesh attached to them. The general opinion of the people of Siberia is, that mammonts have been dug up covered with skin and flesh, and still bleeding: this is an exaggeration; but it arises from the circumstance of the flesh being sometimes found preserved by the frost.

Isbrand-Ides speaks of a head the flesh of which was putrid, and of a frozen foot as large as a man of middling stature: and Jean-Bernhard Muller speaks of a tusk the cavity of which was filled with a matter resembling clotted blood.

We might, perhaps, doubt these facts if they were not confirmed by one of the same kind extremely well authenticated, that of the entire and complete rhinoceros dug up near Vilhorci, in 1771, with all the flesh, skin, and hair belonging to it. We are indebted to M. Pallas for a circumstantial description of this phænomenon; and the head and feet of it are still preserved at Petersburgh. These facts prove that it must have been a sudden revolution which had buried these astonishing monuments of antiquity.

To these general remarks we shall subjoin a cursory view of the principal places in Russia where fossil bones have been discovered.

We have already mentioned those found in the beds of the Wolga; we may add those between the Wolga and the Swiaga, and those along the banks of the Kama, where they are mixed with marine shells; those of the river Irguis, and the bones given by M. Macquart to the Council of Mines, which are mixed with rhinoceros' ones.

It was also from the Wolga, without doubt, that the femur,

mur, brought from Casan by the astronomer Delille, and described by Daubenton *.

M. Pallas gives a long list of bones, tusks, and elephants' and rhinoceros' teeth sent from the government of Casan to Petersburgh in 1776 and 1779, and which also come from the banks of the Swiaga.

The French journals recently contained an account of a complete skeleton found in the territory of Struchow, in the government of Casan.

The beds of the Ob are full of them. The people of Samoieda bring great quantities of them continually for sale at Beresova: they collect them in the immense naked plains which extend to the Frozen Sea, and which are also filled with shells. There is an immense heap of bones at Kutschewaskoi, upon the Ob.

The Irtisch, one of the principal branches of the Ob, is perhaps the river which has afforded the most; as well as its tributary rivers, the Tobol, the Toura, and the Isette. The two latter, which descend from the eastern range of the Oural mountains, often afford these bones mixed with marine productions.

Strahlenberg speaks of an entire head, of four feet and a half long, brought from Tumen upon the Toura. The Tom, another tributary of the Ob, has furnished plenty of them, as well as the Keta.

An entire skeleton has been found upon the banks of the former, between Tomsk and Kafnetsko, by Messerschmidt.

Fossil bones have been found upon the Alei, and even at the foot of these mountains so rich in mines, from which several branches of the Ob derive their source. M. Pallas asserts that there was a grinder taken from a mine in the famous mountain of the Serpents, and found along with some antient marine productions.

The beds of the Jenissea has furnished them at all times near Krasnojark, whence M. Pallas received a grinder. They are to be found even so far north as the 70th degree of north latitude, below Selakino, that is to say, very near the Frozen

* Mem. de l'Acad. for 1762.

Sea.

Sea. The above naturalist also names the Angara, otherwise called the Great Tombuska, among the rivers where they have been dug up. Messerschmidt and Pallas mention the Chatanga also, a river which falls into the Frozen Sea between the Jenissea and the Lena. Isbrand-Ides and Jean Bernhard Muller mention Jakutsk, upon the Lena; and the Academy of Petersburgh is in possession of a rhinoceros' cranium found not far from the mouth of this river, with almost the whole skeleton.

The Vilhoui, which falls into the Lena, and upon the banks of which this whole rhinoceros was found, is certainly not devoid of elephants' bones.

Upon adding to all these places the shores of the Kolyma and the Anadir, spoken of by Pallas, we shall find that there is not a spot in Siberia which does not contain elephants' bones. But what will appear, without doubt, still more extraordinary than all we have related, is, that of all places in the world, certain islands in the Frozen Sea, to the north of Siberia, contain the greatest quantity of elephants' and other bones.

The island which is nearest to the continent is 36 leagues long. "The whole island," says the editor of Billing's Voyage, "with the exception of three or four small rocky mountains, is a mixture of sand and ice; so that when a thaw melts a part of the shore, abundance of teeth and bones of the mammont are found.

"All the island," he adds, "according to the expression of the engineer, is formed of the bones of this extraordinary animal, of horns and craniums of the buffalo, or of an animal resembling it, and some rhinoceros' horns." This is certainly a very exaggerated description, but it proves how very abundant these bones are.

Another island, five leagues further off than the former, and 12 leagues long, has also plenty of teeth and bones; but a third, at 25 leagues to the northward, showed none of them.

It may be that the south of Asia furnishes these bones in as great abundance as the north.

The most southern parts of Asia hitherto found to contain

tain fossil elephants' bones are, the sea of Aral and the shores of the Jaxartes. Daubenton mentions a petrified fragment of a grinder from the shores of this lake; and Pallas asserts that the people of the country bring to market ivory from the neighbourhood of this river.

In general it may be remarked, what is very singular, that none of these bones are dug up in such climates as we know to be inhabited by elephants at the present time, while they are so common in latitudes where they could not exist at present.

Have none of them been ever buried in the latter places? Has heat decomposed them? or, when they have been discovered, have they been neglected, from being ascribed to the animals of the country, and nothing extraordinary observed about them? Naturalists who intend visiting the torrid zone, have a very important subject of inquiry before them.

It would seem, at least, that fossil bones have been seen in Barbary, where there are no elephants of any kind at the present day.

Without mentioning the giant's tooth seen by saint Anthony upon the shores of Utica, and which was as large as a hundred of the human teeth of the present day, the skeleton of the giant dug up by some Spanish slaves near Tunis, in 1559, must have been that of an elephant, more particularly as a second skeleton was dug up at the same place in 1630, which was certainly that of an elephant, as was ascertained by Peyresc *.

In order to complete the history of fossil elephants, it is necessary to inquire if they are to be found in America, a country where no living ones have been discovered since it was known to the Europeans, and where these animals surely could not have been destroyed by the weak and small population which occupied that continent previous to its discovery by the Europeans.

Buffon has already advanced the doctrine of the existence of these bones in South America, and, as he asserts, in that

* Gassendi's Life of Peyresc.

part

part of it alone. We know, also, that he imagined as the cause of their extinction in that continent, the impossibility, from the place where they lived, of their passing the isthmus of Panama when the gradual increase of cold weather forced them towards the south, as if the whole of Mexico was not still warm enough for them to live in.

To conclude; the facts upon which Buffon rested his hypothesis are not entirely accurate. The bones which were discovered in America in his time were not those of the elephant; they belonged to another animal which we have now distinguished by the name of *mastodonta*, and which is also known by the name of the animal of the Ohio.

But there are certainly proofs of the existence of real elephants' bones in America at this moment: several recent authors testify this. Mr. Rembrandt Peale, in his Historical Disquisition on the Mammoth, says that jaw-bones have been found in Kentucky completely similar to those of Siberia, but in small number, in a state of decomposition, and unaccompanied with other bones; whence he concludes that the extirpation of the elephant in this continent was long previous to that of the *mastodonta* or animal of the Ohio, or that its carcase was brought there by some catastrophe.

I find a true elephant's jaw, very well represented, in a plate of the work of J. Drayton upon Carolina.

Catesby speaks of some real fossil elephant teeth in this country. "In a part of Carolina called Stono were dug up three or four teeth of a large animal, which all the negroes, who were natives of Africa, declared to be elephants' grinders; and I thought so myself also, having seen some similar ones which had been brought from Africa *."

Mr. Barton, who pointed out this passage to me, remarks, with truth, that it ought not to be inferred that these teeth were precisely similar to those from Africa, but merely elephants' teeth in general: I should here say, teeth composed of laminæ. In fact, we cannot suppose that Catesby and his

* Catesby's History of Carolina, vol. ii. App. p. 7.

negroes were fit to distinguish the species of this genus at a period when no naturalist had as yet distinguished them.

Mr. Barton adds, that he has himself seen some teeth of the European fossil elephant found, in 1795, at some distance to the north of the place spoken of by Catesby, at a place called Biggin-swamps, near the source of the west branch of Cooper river. They were eight feet deep, mixed promiscuously with bones of the great mastodonta.

The same gentleman saw a grinder of this description procured from a branch of the river Susquehanna, with a portion of a tusk six feet long and 31 inches round, which would have been at least ten feet long if it had been entire; and, what is remarkable, the Delaware savages call this branch *Chemung*, or Horn river.

It was according to these facts that Mr. Barton wrote to M. Lacepede. " The skeletons or bones of some large animals, more or less allied to the family of elephants, have also been discovered in different parts of North America. Among these I recognise the grinders of a species which, if not the same as the elephant of Asia, must have been (as to the form of its grinders at least) more nearly allied to that species than is the mammoth *." Mr. Barton here means the mastodonta.

I have some pieces of fossil bones from America in my own possession. I am indebted for them to the friendship with which I am honoured by M. Humboldt. During all his travels, he neglected no opportunity of collecting fossil carcases, with the view of furthering my researches; and he sent me, upon his return, two pieces of real elephants' bones, the one from North and the other from South America.

The first consists of separate laminæ of grinders, and is consequently unequivocal. They are very large, and entirely similar to those of Siberia by the straightness and the small degree of festooning in the lamina of the enamel, as well as by the small dilatation of their middle. This specimen came from Mexico.

* See Philosophical Magazine, vol. xxii. p. 98.

The

The other piece is a point of a tusk of calcined ivory, but completely recognisable: it comes from the province of Quito, in Peru. This tusk was less compressed than the tusk of the mastodonta is at present, and I have every reason to believe that it belonged to an elephant.

I shall carefully deposit in the museum these two precious pieces, which prove that the true elephants of antiquity, with grinder teeth composed of thin laminæ, have also left their carcases to the north and south of the isthmus of Panama.

That we may neglect no information on the subject, we have to mention the stories told us by the Spaniards of the giants' bones found in Mexico and Peru. We may find extracts of these fables, accompanied with new and detailed accounts, in the "Gigantologie Espagnole," by Torrubia the Franciscan.

What hinders us from applying all these details to the elephant is, that they may owe their origin to the bones of two mastodontes, which are much more common in America than those of the elephant; and no person who has transmitted these details has taken the trouble of giving drawings, or has said any thing that might lead us to distinguish the species. It is true, however, that their pretended giants are now completely extirpated.

This enumeration of the places where the fossil bones of elephants have been found, is the result of an investigation which our anatomical labours, properly so called, have not permitted us to render so complete as we could have desired. It is probable that our enumeration would have been much more voluminous if we could have spared time to go over more carefully the works of naturalists and travellers, or the philosophical journals. It is, however, already sufficient to give an idea of the prodigious quantity of these bones which the earth contains, and of what may be yet discovered if such researches are continued, and if they were oftener directed by men of science.

XLVIII. *De-*

XLVIII. *Description of an Autocratic Cock, useful in Breweries, Distilleries, &c.* By Joseph Steevens, *Esq.*

To Mr. Tilloch.

SIR,

Having frequently observed the great inconvenience attending the application of ball-cocks to the supply-pipes of large reservoirs, liquor-backs, &c. (arising from their beginning to close the aperture of the cock long before the backs, &c. are full); and having had a case of this kind immediately under my own direction, I was induced to turn my attention to the construction of an apparatus to remove this objection.

The reservoir, or back, being about 60 feet above the level of the Thames, from whence it was supplied, it was therefore necessary to have a large conduit-pipe in order to fill it during the short period which the water continues on, at such heights, at neap tides; and it was also indispensable that the cock should remain fully open during the whole time, and shut suddenly when the back was full; which could not be accomplished by the modes now in use: for in cases where large cocks are to be opened and shut by means of a ball or other float, the lever must be much longer or the float much larger than in common, as some of those cocks require a force to turn them from 50 to 100 lbs., with a lever 16 or 18 inches long: hence it is in broad and shallow backs (the area of some of which are from 4000 to 5000 feet) that the ball begins to contract what is termed the water-way of the cock before the back or reservoir is half full, in consequence of which the whole quantity of water would seldom be admitted in one tide. I have therefore inclosed you a drawing and description of an apparatus* to obviate this objection, and which I feel confident in saying will be found far more useful in similar situations than the ball-cock.—I am, sir, your obedient servant,

26, Garlick-hill,
Dec. 5, 1806.

Joseph Steevens.

* The original has been three years in use at the brewery of Cade and Co , an acting model of which is in the Mathematical Society's repository.

Description

Description of the Autocratic Cock.

Let ABC (Plate VIII.) represent the section of a portion of a reservoir or liquor-back. DE, a large pipe supplying the same. F, a cock on which is fixed the large wheel T, about two feet diameter, and the small wheel *u*, about five inches diameter: to the former is attached on one side the weight G, and on the other the vessel H*s*, which is provided with the tube *so*. *ik* is a tube to supply the vessel H*s*, and is moveable on the joint *c*, to allow the orifice *i* to be occasionally raised above and depressed below the surface of the water, by means of the line or copper chain *w*, which is fastened to the wheel *u*. *lm* is a slender rod of iron or wood moveable on the centre *l*: to this, by means of the bar *zr*, is fixed the cork or hollow metal float *r*, which, for the sake of rendering visible, is represented a little below its proper situation. To the end of the rod *lm* is fixed the wire *mn*, provided with a valve at *n* and another at *s*, for the purpose of closing alternately the ends of the tube *so*. The valve *n*, when the apparatus is in the present position, must be as much below *o* as is equal to an arc of 90° of the wheel T: the tube *so* must also be equal in length at least to *on*, that the orifice *o* may fall within the waste-pipe *op*, which passes through and is supported by the bracket *qq*, which bracket serves also to receive the vessel H*s* when it descends. The vessel H*s* is capable of containing a sufficient quantity of water, when about four-fifths full, to overcome the weight G and the friction of the cock F: this cock being open when the weight rests on the bottom of the back, it is evident if the vessel H be filled it will descend until it meets the bracket *qq*, by which descent the weight G will be raised and the cock shut: the tube *i* will also, by means of the wheel *u*, be lifted above the surface of the water *vi*, which will prevent any more from running into the vessel H; and the application of the orifice *o* to the valve *n* will prevent that which is already in from escaping until it is again opened by the descent of the float *r*, which cannot happen until a portion of the water is drawn off: the valve near *s* prevents the water from escaping while H is filling; but if the

the tube *so*, or the orifice *o*, be small, there will be no occasion for the valve *s*. Now it is plain from the figure, if the back be filled until the surface of the water rises to *xr*, the float *r* will be lifted, and the valve *n* brought within the reach of the tube *so* when it descends, which will presently be the case; for when the surface arrives at *v* it will run through *ik* into H, and cause it to descend and shut off the cock F. The wheel T should be a little flatted near *y*, that after being set in motion it may not be stopped by friction until the cock is effectually turned off.

XLIX. *Description of Mr.* ARTHUR WOOLF'S *improved Piston for Steam-Engines.*

THE common method of packing the piston of a steam-engine is so well known, that a very particular description of it in this place is not necessary. Suffice it to say, that the hollow part round the piston (A, *a*, Plate VII.) is filled with rounds of hemp or cotton, loosely spun or twisted, which is pressed into a pretty compact form by a ring B, *b*, which is worked down by screws distributed round the ring and working into the body of the piston; by which means the packing is made to fill the diameter of the cylinder pretty closely, and to prevent, while the packing remains sound, any steam from passing between the piston and the cylinder. In the usual method, whenever the piston, by continued working, becomes too easy, and so occasions a waste of steam, it is necessary to take off the top of the cylinder, even when fresh hemp or cotton is not wanted, merely to get at the screws, which serve to force the upper ring nearer to the bottom of the piston, by which means the packing is forced outwards against the side of the cylinder. This is heavy laborious work, and is therefore generally shunned by the man that attends the engine, as long as the engine can possibly be made to work without taking this trouble; and in consequence of this neglect a great and unnecessary waste of steam is occasioned, and a waste of fuel in proportion.

Mr.

Mr. Woolf's improvement on the piston is such as to enable the engine-man to tighten the piston without the necessity of taking off the cover of the cylinder, except when new packing becomes necessary. He accomplishes this by either of the two following methods:

He fastens each of the screws into a small wheel (*c*, fig. 1, and *c*, *c*, *c*, *c*, *c*, fig. 2. Plate VI.), which are all connected with each other by means of a central wheel (*d*, *d*,), which works loose upon the piston-rod in such a manner, that if one of the small wheels be turned, it turns the central wheel, and the latter turns the other four. The one that is to be first turned is furnished with a projecting square head, which rises up into a recess in the cover of the cylinder. This recess is surmounted by a cap or bonnet *e* (Plate VI. and VII.), which being easily taken off, and as easily put again in its place, there is little difficulty in screwing down the packing at any time. The parts are so clearly expressed in the plates that no further description is necessary to make any person comprehend it.

The other method is similar in principle, but a little different in construction. (See Plate VII.) Instead of having several screws all worked down by one motion, there is in this but one screw, and that one is a part of the piston-rod: on this is placed a wheel of a convenient diameter, the centre of which is furnished with a female screw. This wheel is turned round, *i. e.* screwed down by means of the pinion *o*, *o*, which is furnished with a square projecting head rising into a recess of the kind already described. The ring is prevented from turning with the wheel by means of two steady pins.

L. *On the Food of Plants. By the Rev.* Joseph Townshend, *Rector of Pewsey, Wilts* *.

What is the food of plants?—Before we can give a satisfactory answer to this question we must collect facts, we must multiply experiments. For this purpose, in the years

* From *Letters and Papers of the Bath Agricultural Society*, vol. x.

1792

1792 and 1793 I put various seeds to vegetate in different airs; in atmospheric air, in vital air, and in azote. The general result was, that neither wheat, oats, nor barley, vegetated in azote; but in vital air vegetation was uniformly rapid.

July 12, 1796, I placed eleven cabbage plants in pots, all healthy plants, and weighing each a quarter of an ounce apothecaries' weight. The pots stood in pans with water, and remained in them till June 12, 1797, when the plants were taken out of the pots and weighed again.

Of these pots four had quartz sand, washed clean, and rendered perfectly free from mixture of either argil or calcareous earth.

No. 1. had nothing but this sand: the plant lived, but did not increase in bulk; when examined, the radical fibres were found numerous and extended, but very small; and when the plant was weighed in January 1797, it had not increased in weight.

No. 2. had the same kind of sand and woollen rags: the roots shot vigorously, the plant cabbaged, and in January 1797 weighed two ounces.

No. 3. had the same kind of sand, with about 1-4th part charcoal in powder: the roots were less vigorous than the former, and in January 1797 the plant weighed 3-4ths of an ounce.

No. 4. had this sand with about 1-20th lime. The plant did not increase, yet lived, and in January 1797 weighed only 3 dwts., having lost 2-5ths of its original weight.

No. 5. had brickmaker's clay alone: the plant lived, looked fresh, but in January 1797 weighed only half an ounce.

No. 6. had brickmaker's clay, with an equal proportion of the quartz sand. This plant, like the former, lived, looked fresh, and in January 1797 weighed half an ounce.

No. 7. had brickmaker's clay, with about 1-4th part charcoal in powder. In January 1797 the plant weighed half an ounce.

No. 8. had brickmaker's clay and woollen rags. This plant cabbaged well, and in January 1797 weighed four ounces.

No.

No. 9. had brickmaker's clay, with about 1-20th lime. The plant lived till December, but never grew.

No. 10. had clean dung from the bowels of a horse, with quartz sand well washed. This plant dropped some of its largest leaves during the frost, and yet in January 1797 it weighed 4½ ounces.

No. 11. had peat earth alone: the plant continued healthy to appearance, and in January 1797 weighed half an ounce, but the root was rotted off.

No. 12. was planted at the same time in the garden, near the pots, in rich mould: this did not drop any leaves, and in January 1797 weighed four ounces.

Such was the result of these experiments on cabbage plants.

In January 1797, having removed the cabbage plants, I sowed wheat in the same pots; and 25th September of the same year I made the subsequent report:

No. 1, with quartz sand alone, had two stems 23 inches long, and the ears 1½ inch.

No. 2, the sand and rags, had four stems 28 inches long, and the ears 2½ inches.

No. 3, the sand and charcoal, had one stem 18 inches long, and the ear 1¾ inch.

No. 4, the sand and lime, had two stems 21 inches long, and the ear 2 inches.

No. 5, the clay alone, had three stems 27 inches long, and the ears 1¾ inch.

No. 6, the clay and sand, had four stems 25 inches long, and the ears 2½ inches.

No. 7, the clay and charcoal, had four stems 24 inches, and the ears two inches.

No. 8, the clay and rags, had twelve stems 33 inches long, and the ears 2½ inches.

No. 9, the clay and lime, had one stem, very slender, 15 inches, and the ear 1¾ inch.

No. 10, the dung and sand, had sixteen stems 37 inches long, and the ears 2¾ inches, very strong.

No. 11, the peat earth, had six stems 35 inches long, and the ears 2¼ inches.

Thus,

Thus, it appears, that in both sets of experiments the results were similar.

From these facts, compared with other facts with which we are conversant, such as the flowering of bulbous roots in water, and more especially the vast increase of the withy-tree, recorded by Mr. Boyle, our attention is naturally turned in the first place to water, as the supposed nutriment of plants.

In the experiments before us, both the cabbage and the wheat of No. 1. were well supplied with water; but in the space of six months the former had not increased in either weight or bulk; and the latter, in eight months, produced only two miserable stems.

In Catalonia, more especially in the vicinity of Barcelona, the soil is principally quartz, from decomposed granite; yet being well watered, and plentifully supplied with light and heat, the crops of every kind are most abundant.

M. de Saussure remarks, that "we deceive ourselves exceedingly when we imagine that the fertility of any district depends wholly on the nature of its soil, because abundance and scarcity in crops arise principally from the degree of heat and humidity in the air with the quantity and quality of the exhalations with which it is charged." He adds, "I have seen, in Sicily and Calabria, rocks and gravel arid and uncultivated, such as in Switzerland would have been altogether barren, which there produced more vigorous plants than are to be seen on the richest and best cultivated lands amongst the Helvetic mountains *."

It is astonishing to see, in a warm climate, the rapid growth of vegetables when they are well supplied with water. The smallest cutting of a vine will, in the space of fifteen or sixteen months, cover the front of an extensive edifice, or form a spacious harbour from which the assembled family may gather in abundance of the most luxuriant grapes. In such a situation the seeds of limes, oranges, and lemons, will, in four or five years, produce a shady grove; and mulberry trees, when wholly stripped of their leaves for the nu-

* Voyage dans les Alpes, 1319.

triment,

triment of silkworms, will again, in a few days, be covered thick with foliage.

Adanson, in his account of Senegal, informs us, that "when every thing green has been devoured by locusts, not a vestige of their destructive progress, after a few days, can be discovered."

From the consideration of these and other facts similar to them, many distinguished chemists have delivered it as their opinion that water is decomposed by vegetables. M. Chaptal says, "La décomposition de l'eau est prouvée non seulement dans le vegétal mais dans l'animal." And for this last he quotes the authority of Rondelet.

That water, as such, enters largely into the composition of vegetables, is evident; but whether or not, and to what extent, it is decomposed, has not, as I apprehend, been yet demonstrated. In water meadows, with a plentiful supply of running water, vegetation proceeds even in the depth of winter, and during the severest frosts; but stagnant water is at all times unfriendly to our meadows. Any given quantity may remain upon the surface for weeks or months subject to decomposition; but instead of being in this state beneficial, it is injurious to our crops. In our water meadows we universally observe that it is not humidity which does good, but a thick sheet of water flowing incessantly, night and day, (for a certain period) over the surface.

Hence it seems probable that water is essential to the growth of plants, not merely as such, but as it proves a vehicle of other substances which are their proper food.

If we may form a judgment from their analysis, carbon may be regarded as the chief pabulum of plants; and this we know can, in a given proportion, be conveyed to them by water. M. Chaptal is not only of opinion that carbonic acid is essential to their growth, but he affirms that the base of this acid contributes to the formation of the vegetable fibre. In support of this opinion he observes, that in fungi, which live in subterraneous places, this acid abounds; but by bringing them from almost perfect darkness gradually to the light, this acid disappears, and the fibres proportionably increase. This opinion is confirmed by some experiments of of M. Senebier, in which he observed, that "plants abundantly supplied with water which had been impregnated with carbonic acid, transpired much more oxygen than when they were supplied with common water."

Some plants take more carbon than others into their composition; as for instance, the *Agaricus quercinus*, *Agaricus antiquus*, *Boletus versicolor*, *Boletus igniarius*, *Boletus striatus*, *Boletus perennis*, *Clavaria hypoxylon*, *Clavaria pistillaris*, and many others. All these contain, from the result of analysis, a quantity of carbon nearly equal to all their other component parts. But the *Lichen crispus*, *Pinaster granulatus*, and *Lycoperdon tessellatum*, contain a very small portion of carbon.

Plants do not, however, retain all the carbonaceous matter they receive; they obtain more in the day, when exposed to light, than they naturally require; but by the absence of light they part with this surplus, and therefore yield respirable gas only in the day-time.

The separation of oxygen from plants by radiant light, seems to arise from the chemical affinity between oxygen and light. For this fact we are indebted to Dr. Ingenhouz; but Humboldt was the first who ascertained that hydrogen gas applied to plants, even when excluded from the light, occasions a separation of their accumulated oxygen.

Some plants, as for instance *tremella nostoc*, the *filices*, *musci*, and *algæ*, retain their oxygen weakly, and part with it readily. And it is remarked by Van Uslar, to whom I am indebted for many of these observations, that such plants as contain much oxygen, and retain it obstinately, are white; as for instance, our endive and celery when excluded from the light; while such as contain much oxygen, and part with it easily, are generally green.

If the analysis of plants leads us to consider carbon as one of the most essential articles in their composition and support, no less does the experience of ages prove to us that the principal source from which they derive their nutriment, whatever it may be, is to be sought for in vegetable earth, the produce of animal and vegetable substances decayed. Many plants, indeed, require little or no earth for their

their vegetation, such as the numerous *lichens* and *tragacanths*, of which genera the former were discovered by Saussure on the highest of the Alpine granite rocks. In lower situations these form a soil for the genista, for the cistuses, and more especially for rosemary and lavender, which abound on the most elevated mountains of the Pyrenuces. These again, by their decay, form vegetable earth, in which the luxuriant pine-trees and the ilex grow.

This vegetable matter, being washed down into the valleys, helps to form and to increase their soil to a considerable depth, and to give them that fertility which is not readily exhausted.

When we analyse a soil, we never fail to find it composed of substances derived from a superior level. If the hills are quartzose, calcareous, argillaceous, or magnesian, so is the soil in all the valleys which communicate with them. But with these earths in a rich soil we find a great proportion of vegetable matter, or of animal exuviæ; and as these are deficient or abound, vegetation languishes, or is exceedingly luxuriant.

Good mould abounding with vegetable matters is commonly of a dark colour, pulverises easily, and has therefore what is called a mellow look; but when exhausted or impoverished by frequent crops, the richest soil, such as I have here described, becomes arid, of a lighter colour, compact, and comparatively barren. In a maiden soil, or where every shower of rain brings down from more elevated regions a quantity of vegetable matter, a succession of luxuriant crops may be taken incessantly without any diminution of fertility. Thus it is in the country newly occupied by the Americans, in Kentucky, on the Ohio, and in the whole extent of territory watered by the Mississippi or by its tributary streams. Thus also in some parts of Spain, where an extensive plain happens to receive the spoils of rich circumjacent hills, as in the well-watered vale of Orihuela, near Murcia, of which they say, "Let it rain or not rain, corn never fails in Orihuela." Indeed, so productive is wheat in that highly-favoured district, that the farmers commonly receive 100 for 1 upon their seed.

In my experiments, No. 10, we see, by the luxuriant growth of the cabbage and the wheat, what vegetable matter can produce: for in neither of these could any kind of nutriment be derived from the quartz sand in which they spread their roots.

The same kind of sand in the vicinity of Barcelona is, by the assistance of a bright sun and copious irrigation, rendered exceedingly productive; but then they spread upon the land all the dung they can procure, and not only station children and old women on the highways with little baskets to collect this manure as it falls from horses or from mules, but, like the farmers in the south of France, they pick the leaves from the trees in autumn, and this at a considerable expense. Of such importance do they consider vegetable matter as the food of plants.

It must be confessed that we have frequently occasion to observe plants dependant on the nature of the earth in which they are found, and affecting each its peculiar earth, in which they grow spontaneously and thrive.

Thus on chalky and calcareous soils we find *Thesium linophyllum*, *Anthyllis vulneraria*, *Asperula cynanchia*, *Lotus corniculatus*, *Hippocrepis comosa*, *Poa cristata*; and three of the *Sedums*, the *S. acre*, *S. album*, and *S. reflexum*; as on the Wiltshire downs, and on the hills round Bath.

On sand we see *Arenaria*, *Rumex acetosella*, and all the sorrels; the *Plantago maritima*, the *Plantago Coronopus*, the *Onopordum Acanthium*, the *Sedum Anglicum*, and most remarkably the *Spartium scoparium*.

On clay, if wet, the *Carices*, the *Junci*, *Schœnus*, *Aira cespitosa*, and *Aira cærulea*, *Orchis latifolia*, and *Orchis conopsea*; if dry, the *Primula veris*, *Orchis mas*, *Orchis maculata*, and *Poa pratensis*.

On bogs, the *Equiseta*, *Vaccinium uliginosum*, *Anagallis tenella*, *Scirpus palustris*, *Menyanthes trifoliata*, and *Drosera*, delight to dwell.

On the sea shore, and wherever the muriatic salt abounds, as near Alicant in Spain, we find *Salicornia Europæa*, four species of *Salsola*, *Chenopodium maritimum*, and two species of *Mesembryanthemum*.

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These maritime plants appear to decompose a part of the soil in which they grow; the alkali produced by burning them, or the sal soda used in glass and soap, is evidently derived by them from the muriatic salt.

But when we see the *Lichen parellus* fixing itself on the siliceous rock, or the *Lichen immersus* affecting as it does the calcareous rock in preference to the siliceous; whatever may influence this choice, we cannot suspect that either of these rocks contributes by its decomposition to the nutrition of these plants; nor, as I apprehend, have we reason to imagine that either chalk, sand, or clay, are in any form the aliment of the plants.

Woollen rags have been found of great utility as a manure, more especially for wheat. And in the experiments before us we may observe, that sand with rags produced a cabbage of two ounces, and four strong ears of wheat. In clay with rags our cabbage weighed four ounces, and we had twelve strong ears of wheat. But in what manner these rags produced effect it is difficult to say, for in January 1797 they were not visibly decayed; and in the month of September in that year they still retained their texture. The quantity we usually spread upon one acre is not more than four or five cwt.; and yet in the experience of every farmer it is found that in the first year they nearly double the crop of wheat, and in the two succeeding years they yielded a visible increase. At present, therefore, we can merely record it as a fact, that woollen rags are highly beneficial to the land: but we cannot pretend to say by what process they contribute to the nutriment of plants.

Lime in our experiments was clearly detrimental with sand; the cabbage lived, but weighed less in January than when planted in July: the wheat had two slender stems. In clay with lime our cabbage lived till December, but never grew. The wheat had one stem, which was extremely slender, and the ear was diminutive.

These facts appear discordant with the experience of farmers in every quarter of the globe; for lime is found to be an excellent manure. In some parts of Wales they have

 scarcely

scarcely any other dressing for their wheat. I well remember, that in the parish of Lansamlet, in Glamorganshire, my father, who was very attentive to agriculture, put most of his stable dung on meadow land, and used only lime for wheat. He had two lime-kilns constantly burning for his own use, and with this manure he obtained the most abundant crops; but then his land was principally a dark vegetable mould, and much of it was peat, which before it was drained had been a bog. On this land I have counted sixty grains to an ear, not picked and culled out of many others as being longer than the rest, but taken by handsful at random.

In his land, lime as a dressing was particularly apt, because, as we know, it hastens the putrefactive process, and promotes the dissolution of vegetable substances, converting them quickly into vegetable mould.

Now in my experiments there was no vegetable matter to be dissolved, and therefore no benefit, according to chemical principles, was to be expected from the lime. The trial was however made, and the received opinion as to the effect of lime is thus far confirmed.

But in my experiments the lime appears to have been deleterious. This was not from its causticity, for the plants lived; but from its action as a cement in forming a crust on the surface of the pots impervious to air. For in these pots I remarked, that after rain the water stagnated, and did not readily penetrate as in the other pots.

Free access of air to the roots of plants seems to be of vast importance, and almost essential to their growth. With regard to seeds, access of air is absolutely needful to their vegetation. Hence it is that charlock (*Sinapis arvensis*) will remain in the earth for centuries, if deposited below the vegetating distance, as we have occasion to observe on Salisbury plain, where no charlock is ever seen, unless when the downs are broken up. The land is then covered with it; but till then the seeds remain as *in vacuo*, and are therefore not liable to change.

This deposit of seed must have happened in most remote antiquity,

antiquity, either when the hill country, like the low lands, formed part of an extensive forest; or more probably when these extensive downs were subject to the plough.

Being solicitous to know whether these seeds were antediluvian, I took earth from different depths, and soon got below the stratum in which these seeds are found.

The necessity of air for the vegetation of seeds will account for effects which in agriculture are too frequently observed.

If soon after wheat or barley has been sown on what is called a running sand there falls a dashing rain, the sand runs together, that is, it forms a crust, which in a great measure is impervious to air, and scarcely a grain of corn will grow; or if on clay land, during a time of drought, a garden plot is watered, and left exposed to the scorching beams of a meridian sun, the ground will bake, that is, the surface will be hardened, and being thus rendered impervious to air, vegetation ceases. But if the surface has been previously covered with fern leaves, as practised by skilful and attentive gardeners, no such effect will be produced. The plot may be watered, and vegetation will be rapid.

The admission of air, and its vast importance to the growth of plants, will account for the good effect produced by harrowing our wheat crops in spring, as lately introduced, and now universally adopted by our best farmers. The good effect produced is made apparent by the luxuriant growth of pease, beans, turnips, and cabbages, after they have been hoed; and is at present so well understood, that many agriculturists hoe their turnips twice, and their beans four times, not merely with a view to the destruction of weeds, but because they observe the benefit arising to their crops by the free admission of air into the earth. The palpable advantage of this practice has led many farmers to consider the principles on which the practice has been founded, and to try by experiments how far it can be pushed.

In this pursuit, and satisfied of the benefits to be derived from loosening the surface of the ground contiguous to his crops, the Rev. Mr. Close has given up the broad-cast hus-

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bandry, keeps the hoe constantly in motion, and now finds that he has never occasion for a fallow.

But the most astonishing effect produced by giving free admission of air to the roots of wheat was last year exhibited by Mr. Bartley, secretary to the Society of Arts at Bath. In August 1800 he sowed his wheat in rows with three-feet intervals, and six inches distance from grain to grain. The proportion of seed was two quarts to an acre. The soil was a deep sandy loam, but out of condition, and filled with couch. This wheat was hoed in autumn, hoed again, and earthed up both at Christmas and spring. When it was in bloom the intervals were dug up, and it was once more earthed up. At harvest this crop yielded sixty-six bushels per acre. Such was its luxuriancy, that many of the plants produced 98 perfect ears, many of which, nine inches long, contained each 100 grains.

In the broad-cast husbandry of the hill counties of Wilts and Hants, the produce was formerly three or at most four for one, as it was in the greatest part of France. By the drill, without hoeing, the return would not be near so much; but in Mr. Bartley's crop we see more than 1000 for 1; and some grains yielded nearly ten times as much*.

I shall make but one observation more upon this subject, which is, that an orchard planted on the green sward requires double the time for its maturity as one on cultivated land, that has a more plentiful supply of air admitted to its roots.

Thus we see that all the great agents in nature are concerned in the process of vegetation, and may be considered as the food of plants. But to determine in what manner each contributes to nutrition, must be left to the investigation of succeeding generations.

LI. *Letter*

* It must ever be with reluctance that an exception can be taken against any argument of so able a writer as the present, especially in a matter of alleged fact. But in this instance it seems proper to remark, that the argument drawn from the reported success of Mr. Bartley should be received with caution, on account of the peculiarity of the soil;—that soil being remarkably deep, fat, and productive, and within the limits of a nursery-man's garden, near a city abounding with manure, are circumstances not common to other situations.

LI. *Letter from* Thomas Keith, *Esq. Secretary to the Master of His Majesty's Household, &c., respecting Mr.* Bonnycastle's *Treatise on Plane and Spherical Trigonometry.*

To Mr. Tilloch.

Sir,

I beg leave to correct some erroneous statements in your valuable Magazine for November 1806, article xxx. p. 176, &c., respecting Mr. Bonnycastle's Treatise on Plane and Spherical Trigonometry, lately published; not doubting, from that attention to impartiality which I have had frequent opportunities of observing in your work, that you will do me the justice to insert my remarks.

It is not my intention, sir, to follow the writer of this article step by step in his observations on Mr. Bonnycastle's book, nor in the smallest degree to doubt the rectitude of his intentions when he says, "we are conscious that in recommending this treatise to general favour, we are equally discharging an act of justice to the author, and of service to the public." My remarks will therefore be as short as possible.

The plane trigonometry, we are told, contains "a great variety of practical examples;" and the spherical is applied "to the solution of astronomical problems," and contains "tables of right ascension, &c. useful in the preceding solutions;" to which he adds, "This part of the work cannot fail to be of the highest utility." In another place, (speaking of a part of the work, the merit of which I am not inclined to dispute, as it concerns not myself,) "This part of the work alone is sufficient to stamp its value, *did not every other part bear evident traces of the same hand.*" This is an assertion, sir, which could not possibly have been made had the writer seen my treatise on the same subject, published

situations. Consequently, the result of any experiments made in such a spot, is not to be considered as applicable to the general practice of agriculture and planting on a large and common scale of cultivation. With the necessary allowances which the local advantage above mentioned suggests, the consequences drawn by this gentleman may still be of importance for the consideration of our practical readers.—*Editor of the Letters and Papers.*

lished by Longman and Co. and Vernor and Hood, in 1801. I do not, from this observation, by any means wish to ascribe to myself any of the merit attributed to Mr. Bonnycastle by the writer of the article in question, nor to put my work in competition with Mr. Bonnycastle's; but I cannot suffer such a remark as the above to pass unnoticed, when I can clearly prove that a very considerable portion of Mr. Bonnycastle's work is *a direct copy of mine*. Exclusive of detached matter, there are upwards of *seventy pages* in which there are scarcely ten lines in any one page which are not directly copied from my work; and in many pages not a single line nor figure but what is copied. For instance, pages 232 and 233 are both entirely copied from different parts of my work: thus, example 2d, is my second example, p. 267; example 3d, is my fourth example, p. 268. The note at the bottom of the page is taken from my article marked D, p. 268; and the xixth problem is a copy of my first example, p. 255. In p. 255 of my Treatise is the following erroneous remark: "The sun's parallax is about 9″, therefore his upper limb will appear in the horizon when he is 32′ 51″ below it; and as his semi-diameter is 15′ 47″, *his centre will appear in the horizon when it is* 48′ 38″ *below*." Mr. Bonnycastle, in his note, p. 233, says the same. At p. 303 of my work, 10° 46′ are stated to be equal to 43′ 40″ of time; and this error is carried through my calculation: this Mr. Bonnycastle has copied at p. 250 of his treatise; and brings out my conclusion, though the calculation occupies two pages. All the astronomical problems in my work, the framing and calculation of which were attended with a considerable degree of labour, are adapted to the year 1796, at which time I expected my work to be published; and the "tables of right ascension, &c. useful in the solutions," and already noticed, were collected from the Nautical Almanac, for the purpose of illustrating the examples. Mr. Bonnycastle has adapted all his examples to the year 1796, and copied my tables altogether; the intention of which is too obvious to need any comment! Lastly, the geometrical figures in various parts of the book are likewise the same as those in my work, as if pricked from my plates by schoolboys. Thus the figure at p. 111 is the same

same size as my figure xiii. Plate VI; the figure in p. 128 is a copy of my figure xvii. Plate VI; and the examples at both places are mine, as well as the constructions; but as my examples and constructions are placed at different pages, and my figures are given in plates, the coincidence, at first sight, is not so obvious as it would have been had my work been printed in the manner of Mr. Bonnycastle's.

From the specimens already given, the truth of my assertions cannot be doubted; for it would baffle all the mathematical laws of chance to produce, from fortuitous circumstances, such a coincidence as I have pointed out: but as this subject will be more amply discussed elsewhere, I shall desist from troubling you with any more remarks; and hope, as it is a mere act of justice to myself, that your readers will excuse me for having occupied any part of your valuable work with matter which has no tendency either to instruction or entertainment.

I am, sir,
Your most obedient servant,
THOMAS KEITH.

No. 18, Norfolk-street,
Fitzroy square,
January 12, 1807.

LII. *On the Cultivation of the Poppy.* *By* T. COGAN, *M. D.**

ALTHOUGH the ardour with which the British nation pursues whatever promises to be of public utility, is perhaps unequalled by any other, and certainly exceeded by none; yet there is one subject which has hitherto been permitted to escape our attention, and in which several nations upon the continent can not only boast of their superior policy, but are already enjoying considerable advantages from it; I mean the cultivation of the poppy to a great extent for the benefit of its oil, as an article of food, and for other useful purposes.

It will doubtless be remarked, that we ought not to ascribe the neglect of it as an article of food to inattention altogether, but to a superior caution, as the narcotic quality of

* From *Letters and Papers of the Bath Agricultural Society*, vol. x.

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the poppy renders it totally unfit to be taken inwardly? This, it is allowed, is, in appearance, a very formidable objection; and, as it respects the lives of multitudes, it ought not to be treated with levity: the objection itself, and the argument from analogy on which it is founded, ought to be completely confuted before the article can be recommended to the community in this novel point of view.

We might observe that the objection is solely founded upon very slight and imperfect analogy. It assumes, that, because some parts of a plant are noxious, the whole must be equally noxious. But this assumption may be confuted in numberless instances. Daily experience testifies that different parts of plants possess not only different, but opposite qualities. Oranges and lemons, which are used in profusion, possess juices that are both palatable and refrigerating; but these are inclosed in a rind, the essential oil of which is extremely acrid and stimulating: and it is well known that the bland and nutritive tapioca is the produce of a tree whose roots are highly poisonous. In this case, therefore, the argument from analogy may be considered as a very proper motive for caution; but if it advances further, it degenerates into a pernicious prejudice.

There have been, however, many incidental circumstances which have had a partial influence in removing these prejudices. It is well known that compounders of medicine have made a very liberal use of the seeds of poppies, as substitutes for the oil of sweet almonds, without the least detriment to the patient. They have sometimes imputed to it additional virtues, from its being supposed to possess narcotic properties. But that they have erred in their hypothesis is plain, from the practice of many individuals who have made the seeds of poppies a common article of food *.

But it will be the principal object of the following paper to inform the inhabitants of this country, through the medium of your publication, that the above objection has been repeatedly advanced and repeatedly confuted; that experiments, first made with a degree of caution, have finally re-

* See Prosper Alpinus, lib. iv. cap. i. Geofrey Mat. Med. tom. ii. p. 715. Lewis's Materia Medica, article *Papaver album*.

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moved prejudices long and inveterate; and that the white poppy (*Papaver hortense semine albo*) is cultivated to a very great extent in France, Brabant, and Germany, and, more recently in Holland, chiefly to extract the oil from its seeds; which is found not only to be salubrious, but to be peculiarly delicate in its flavour. It is now become a considerable article of commerce; the oil of a superior quality for the use of the table, and the inferior for manufactories and various other purposes. It is produced not only with considerable profit to the cultivator, but also to the merchant and consumer.

As it is natural to imagine that the prejudices against the common use of poppy oil for culinary purposes will be very general, since they are apparently sanctioned by prudent caution, it is not expected that the most positive assertions, founded upon the experience of strangers on the continent, would be sufficient to remove them. But a circumstantial narrative of a contest which has already taken place, and of the final triumph of experience over the opposition founded on analogous reasoning, and a particular statement of the advantages which have accrued to the cultivator, merchant, and consumer, may perhaps attract the attention of some agriculturists in our own country, who may thus be encouraged to make similar experiments; and as the issue must be the same, they will be able to produce absolute demonstration that the oil is totally destitute of the noxious qualities that have been ascribed to it; and finally convince the public that it may become a cheap and useful substitute for the olive oil, and a very beneficial article of commerce.

For this purpose I shall state to the agriculturist a succinct account of the rise and progress of the cultivation of the poppy, in order to express the oil from the seed; the manner of cultivating it, and the emoluments which have been received by the cultivator, from authentic documents in the Dutch and German languages which are in my possession.

In the year 1798, the society established at Amsterdam for the encouragement of agriculture, being informed that the

the oil of poppies was cultivated in several parts of France, Flanders, and Brabant, thought it an object of sufficient importance to make more particular inquiry; and they learned, from indubitable authority, not only that it was generally used in the place of olive oil, but that several thousand casks of it were exported annually, a large quantity of which was imported into Holland and sold under the name of olive oil, or mixed with it in considerable abundance; and they appealed to several merchants, who were members of the society, for the truth of this assertion, without being contradicted.

These facts induced the society to propose three premiums, consisting of a silver medal and ten ducats each, which were divided into the three following classes:

The first to the husbandman who should sow not less than half an acre of a clayey soil with poppy seed; the second on a sandy ground; and the third on turf or peat land.

They also offered to the person who shall have cultivated the largest quantity of ground, on the two first species of soil, in the most masterly and advantageous manner, a gold medal, value fifty ducats, or that sum in money, in lieu of the above premiums.

The candidates were to give an accurate statement of the quantity of seed sown per acre; the time of sowing and of gathering the poppies; the quality of the soil; the manner of procedure in every part of the process; the quantity of oil produced, and the total of the expenses.

In consequence of the above proposals, in the year following (1799) Mr. P. Haak became a claimant; sent in satisfactory specimens of the oil produced, accompanied with testimonies from two respectable physicians, that upon experiments made, it fully appeared that the use of the oil was not in the least prejudicial to the human constitution; and that the oil cakes were very wholesome and nutritive food for cattle.

The committee appointed to receive this report, not only expressed their entire satisfaction at the attestations of the physicians, but they laid before the society at large an account of the proceedings which had taken place in France upon the interesting question concerning the noxious or salubrious

lubrious qualities of the poppy oil, in the following narrative:

So early as the beginning of the 17th century the oil of poppies was produced in such large quantities that it gave rise to great and lasting contentions, which rose to such a height that the government was desired to interfere and appease the contending parties, either by authorising the use of this oil, or totally to prohibit the consumption, according as experiments should decide whether it contained the noxious qualities ascribed to it, or not.

The opposers urged the objections already stated: they asserted, that as the capsulum or poppy-head contained juices highly narcotic, this must also be the case with its seeds; that the frequent use of the oil extracted from them exposed the consumer to all the dangerous consequences arising from the too liberal use of opiates; and that they would finally obtund the faculties of the soul; that the oil was of a drying quality, for that it was upon this account it became peculiarly useful to painters: they therefore implored government to confine its uses to this object.

The advocates maintained that no proofs existed of these pernicious effects; on the contrary, experience testified that the seeds were peculiarly nutritive both to men and cattle; they asserted that the antient Romans, concerning whose mental powers there could be no doubt, were accustomed to mix the oil and meal of the poppy seed with honey, and have it served up as a second course at their tables; and that it was on account of its nutritrious qualities, so well known to the Romans, that Virgil gives it the title of *vescum*, food, by way of pre-eminence; and that the peculiar qualities of this oil rendered it a desirable object of cultivation; that its taste was delicate and pleasant, somewhat resembling that of the hazel nut; that it continued in a fluid state, exposed to a much greater degree of cold than was required to congeal the olive oil; that it contained a larger quantity of fixt air, which preserved it a longer time from being rancid; that in these particulars it not only approached to the finest oil of Provence, but it mitigated the disagreeable taste which that oil acquired by length of time; and that the poppy oil decidedly

decidedly deserved a preference to every other oil expressed from seeds, whether nut, almond, or beech; which, though they yielded large quantities, soon became rancid: and as there was no appearance of its being pernicious in the more extensive use of it, so valuable a product ought not to be confined within the narrow bounds of the painter's use.

Things were in this state, without any prospect of accommodation between the parties, when the severe winter of 1709 overtook the combatants. This damaged the olive, nut, and almond trees to such a degree, that there was a great scarcity of their oils; and they were obliged to have recourse to the substitutes, beech and rape, &c. But it was soon perceived that these were far inferior to the oils extracted from the red, white, or brown poppy, which had a much nearer resemblance to the small portion of the olive oil which the winter had spared. This was consequently mixed with the olive oil in the proportions of one-fourth, one-third, one-half, without the least opposition. But when it was attempted to sell the poppy oil in its pure and unmixed state, the opposition became so violent, that the lieutenant-general of the police of Paris resolved, in the year 1717, to order the medical faculty of that city to make the strictest examination concerning this subject, and deliver in their report.

The faculty appointed forty of the most celebrated practitioners in medicine as a committee of inquiry, who were witnesses to various experiments accurately made, and whose report was expressed in the following terms: "*cum sensuissent doctores, nihil narcotici, aut sanitati inimici in se continere, ipsius usum tolerandum esse existimarunt*;" that is, they were of opinion, that as there is nothing narcotic or prejudicial to health contained in the oil, the use of it might be permitted.

But this decision was unsatisfactory; and popular clamours determined the court of justice to pass a decree in the year 1718, whereby the sale of poppy oil, whether mixed or unmixed, was prohibited, under a fine of three thousand livres for the first offence. Notwithstanding this prohibition, the sale of the article was clandestinely encouraged and gradually

gradually increased until the year 1735, when the court issued a severer decree, enjoining it, upon superintendants appointed, to mix a certain quantity of the extract of turpentine to every cask containing 1100 lbs. of this oil; of which not less than two thousand casks were consumed in Paris alone. This attempt to render the use of it impracticable, had no other influence than to annihilate the public sale of the article, but the secret demand for it increased; till at length, in the year 1773, a society of agriculture undertook to examine, with the closest attention, all that had been alleged, either by writing or otherwise, for or against the general use of this oil. Experiments were repeated, in the presence of the most distinguished chemists, with the same result, and the society presented a petition to the minister of police, setting forth the great advantages that would accrue both to commerce and agriculture by reversing the prohibition.

This petition was put into the hands of persons who vended various kinds of drugs, and who had, as a body, opposed the subject of it, with orders to state all their objections to the medical faculty; by these means the faculty became masters of every thing that was urged in the debate. They again made several experiments in the year 1776, and finally confirmed the decree of the faculty issued in 1717, declaring that the oil of poppies was not injurious to health, that it did not contain a narcotic power, and that it might be recommended to general use with the utmost safety. The medical faculty at Lisle had also made a similar declaration in the year 1773. From that time to the present the cultivation of the poppy has not met with any formidable opposition; and has increased to such a degree both in France and Brabant, that they have been able to export a considerable surplus, to the great advantage of the husbandman as well as the merchant; and in seasons of scarcity it has been found of the most essential service in all cases where the use of oils was required. In the northern parts of France it was used by soap-boilers as a substitute for other oils, which were extremely dear; and in Brabant the oil-cakes are constantly used as food for cattle with obvious benefit."

These facts being established, the committee of agricul-

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ture in Amsterdam proposed the premiums above mentioned, in order to ascertain whether the experiments made would authorise the cultivation of the article upon a large scale; whether the soil and climate of Holland were beneficial to its growth; whether the quantity or quality of the oil would be similar to the product of France and Brabant; whether the profits would indemnify the husbandman from giving it the preference to other crops; whether the oils could be afforded cheaper than those in common use; and to what purposes either in the arts or manufactories it might be applied.

Deeming it possible that the narrative of a contest which subsisted the greater part of a century, and in which the advocates for the internal use of the poppy oil were uniformly triumphant, may have some influence in destroying our own prejudices and apprehensions respecting the pernicious quality of this oil, I shall now proceed to state, in as concise a manner as perspicuity will permit, the most interesting particulars respecting its culture, selected from various foreign publications upon the subject.

Soil.—The poppy may be cultivated with success on various kinds of soil. It has been tried on a rich black soil, peat ground, and sandy heaths, and been productive. Those lands in which the wild poppy abounds the most, are obviously most congenial to its nature. The richer the soil, and the clearer from weeds, the larger will be the crop. It is not so adviseable, however, to manure for the poppy, as for the crop preceding it, as it is more exposed to injury by weeds. Hence it succeeds the best after carrots, cabbage, potatoes, &c. The land was generally prepared by the spade, as in planting potatoes; and the finer it is worked, the greater the advantage. But when it is cultivated to a great extent, they use the plough. The seed has generally been sown broad-cast, the plants thinned, and weeded afterwards, as in the culture of turnips; but in drills it is sown about six or eight inches distant in the rows, which has been strongly recommended, experiments upon a small scale having manifested a superiority in this mode.

The Kind and Quantity of Seed.—Although the white poppy has been chiefly used in France and Brabant, under the

the supposition that it produced the finest oil, yet it has been found that various other kinds will answer the purpose as well. It is even asserted that the blue poppy, while it yields the largest quantity of seed, is in no respect inferior in the quality of the oil. Admiral Kingsbergen, whose private virtues render him no less a favourite with his countrymen than his skill and courage as a naval officer, instituted an experiment with different kinds of seeds in the same soil, and he could not perceive any difference in the quality of the oil, while the seeds of the blue poppy yielded considerably more.

The quantity of seed generally used in the broad-cast has been after the rate of 2 lbs. to an English acre. In drills a lesser proportion has been used.

Time of sowing.—This is from the middle of March to the middle of April. If it be sown much earlier, it is more likely to be choked by weeds; if later, the harvest will be thrown deep into the autumn; and unless the weather be unusually favourable, the seeds will not ripen kindly.

Weeding.—As soon as the plants appear about two inches above the ground, they must be carefully weeded and thinned, till they stand about seven or eight inches from each other. The weeding to be repeated as often as it shall appear necessary.

Harvest.—In the beginning, middle, or end of August, according as the time of sowing has been earlier or later, and the season propitious, the seeds are ripe for gathering the poppy heads. Several methods have been recommended to harvest the crop. At first, the heads or balls were broken off from their stems, gathered together in large quantities, and deposited in a barn, or any other convenient place, in large heaps, in order to dry them. This method was not only tedious but injurious; some of the balls becoming musty, communicated a disagreeable taste to the seeds, and consequently to the oil. Mr. Poske, of Zell, in the electorate of Hanover, prefers the following method:—He draws the entire plants out of the ground; binds a sufficient number of them at each extremity, and places them against each other in the manner of wheat-sheaves; and lets the whole remain in the field for

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eight or ten days, until they are perfectly dry. It was customary to cut open the capsulum with a knife: he prefers hacking it in two or three places with a bill-hook, and asserts that one person may in this manner do more work than ten times the number of hands in the former manner; and that the seeds are more easily evacuated from their cells. But the most convenient and expeditious method is to cut off the poppy heads as they stand in the field: the reapers having an apron before them, tied up at the corners. In this they collect as large a number as is convenient, and empty them into bushel baskets placed upon a cloth; by which a considerable quantity of seed is saved. The heads are afterwards put into corn sacks, in a competent number to be trodden by men or children in *sabots*, or to be bruised by a mallet or flail; by these means the heads are confined from flying from the stroke, and the seeds preserved from being scattered, and afterwards passed through a sieve of a proper size.

In extracting the oil, it is of the utmost importance that mill, press, and bags, be perfectly clean and pure. New bags are necessary, as those used for linseed, rape, or any other seed, will communicate an unpleasant taste to the oil. It is adviseable to extract the oil as soon after the harvest as possible, as the seeds will yield a larger quantity than if deferred till the spring.

The first oil is destined for the use of families. This is cold-drawn, as any degree of warmth injures the flavour. After as much is extracted in this manner as possible, a considerable quantity of an inferior quality is obtained by heating the cakes, and pressing them a second time.

The oil expressed must remain for the space of five or six weeks before it is used, that it may deposit in a sediment a kind of milky substance that is mixed with it. It must then be poured into another vessel; and this should not be perfectly closed at first, but the opening be covered with a linen cloth, or a pricked bladder, that certain exhalations may pass. Nor should the oil be immediately used after the process is finished, as it continues to improve for a considerable length of time.

That which is first expressed is of a pale colour; is peculiarly bland and soft, has a flavour approaching to that of the almond oil. It is used for salads and other domestic purposes, either alone or mixed with olive oil. Should the latter be stale or rancid, it will be considerably improved by a mixture of recent poppy oil. It is not asserted that this oil may be placed in competition with Provence or Italian oils of prime quality; but that it is superior to the olive oils sold in shops, being often used to improve their quality. May I not add, that the inhabitants of this country are somewhat prepared for the culinary use of this oil, by being already accustomed to its taste, though without their knowledge. For since it has long been imported into Holland, and used without suspicion, we cannot suppose that the merchants of this commercial nation are totally strangers to the commodity *.

The second-drawn oils are of a deeper colour, and are applicable to all the purposes of the more common oils. This may even be used as lamp oil; and it is alleged that it does not give off so large a quantity of smoke, and emits a brighter flame.

The oil-cakes are peculiarly serviceable for feeding and fattening of cattle; being deemed equal to linseed cakes. All cattle are very fond of it, and eat it with eagerness. This is the constant use of it in Brabant. The stems are sometimes used for fodder, containing a considerable quantity of nutritive oils; or mixed with stable-dung and other manures, they enrich their quality.

* We are told by Mr. C. A. Fisher, in his *Letters written during a Journey to Montpellier, in the Year* 1804, "that the oil of Provence, which, on account of its purity, mildness, and fine flavour, is famous all over Europe, is exported to Italy in large quantities, and was formerly exported to many distant countries. But since the hard winters of 1789, and the following years, so many olive trees have been frozen, and during the revolution so few planted, that Aix (which was the principal seat of its traffic) has now entirely lost its first and most lucrative branch of commerce."

Two inferences may be drawn from the above information: our best oils, though imported from Italy, are probably of the growth of Provence; and it is still more probable that the inferior sorts could not be afforded, even at the present price, without a large mixture of the poppy oil.

Expenses, Produce, and Profits.—Concerning these articles it will be necessary to be particular, though it is somewhat difficult, from a difference in the current coins, measures, &c. I shall state the result of experiments made on 300 roeden *, about one acre of a sandy soil, and 300 roeden of a heavy peat, made by a claimant named S. N. Van Eys. The peat land being low and humid, he was obliged to make deep trenches between the beds. The harvest on this soil was later, the poppy heads were not so dry when gathered, and they shrunk considerably in drying. There was so small a difference in the quantity of seed from these different soils, that no important preference could be given. The sand ground yielded in this instance rather less than the peat land. As the quality of the seeds appeared perfectly similar, he mixed the whole produce together, when he sent them to the oil mills.

The produce of the sand ground rather exceeded 13 sacks, that of the *veen* or peat land was about 12 sacks; together, they made 25 sacks 1 bushel of seed. These yielded of oil in the following proportions:

	Mingles†.	Cakes.
23 sacks which were pressed cold gave	271	834
2 sacks warmed - - - -	29	56
834 cakes, warmed and pressed, gave	73	
Total oil	373	890
Cakes diminished in a second pressure to 726 - - - - minus		108
Total of cakes		782

Mr. Van Eys remarks, that poppy oil, of a very inferior quality, is sold retail at one guilder, or 1 *s.* 10 *d.* per mingle, or quart, and that mixed with olive oil at a much higher price. However, he estimates the cold-drawn at 16 *d.* only, and the

* The English statute acre is 160 square perches; and the Dutch morge, consisting of 600 roeden, is equal to 300 square perches: so that the difference between a Dutch morge and two acres, is as 300 to 320, the former being only twenty perches less than two acres.

† A mingle is about two pints.

the second sort at 14*d*. per mingle. The cakes are valued at ten guilders, or 19*s*. per 100. His receipts stand thus:

271 mingles (cold-drawn), at 16*d*.	F. 216	16
102 ditto (warm), at 14*d*. -	71	8
782 cakes, at 10 f. per 100 -	78	4
Total	F. 366	8—£. 33 0 8

Statement of Expenses.

To digging, &c. 600 roeden, at 1½*d*. per roeden	F. 52	10
Seed, sowing, weeding, &c.	42	19
Harvesting, beating out seed, &c.	48	3
Pressing out the oil, bags, &c.	63	8
Total	F. 207	0—£. 18 14 0

Receipts - -	F. 366 8 0	—£. 33 0 8
Expenses - -	207 0 0	18 14 0
Total of profit -	F. 159 8 0	£. 14 6 8

This degree of profit upon nearly two acres does not at first appear to be encouraging; particularly if we take into consideration rent of land, taxes, &c. which are not mentioned in the statement. Mr. Van Eys has remarked, that the expenses attendant upon pressing out the oil, in this first essay, were considerably greater than would be experienced in the usual course of business. We may also notice that the preparation of the ground by manual labour created a difference in expense that would prove an equivalent at least to the value of land and contingent charges. But what is of much greater moment is the very low price of the oil, as stated in the above account: that of an inferior quality being valued at somewhat less than 5*s* per gallon, and the superior at less than 5*s*. 6*d*., whereas common lamp oil is with us sold for 6*s*. per gallon, and salad oil of no extraordinary quality at 2*s*. 6*d*. or 3*s*. per pint, or 1 *l*. or 1 *l*. 4*s*. per gallon.

It clearly appears from these facts, that 1*s*. 6*d*. per pint, or 12*s*. per gallon for the prime article wholesale, and at least 4*s*. per gallon for the inferior sort, would be an advantageous price for the purchaser, who would be able to retail it considerably under the current prices of these articles.

According to this estimate, the receipts upon 271 mingelen, or quarts, of the cold-drawn would amount to about 40*l.*; upon 102 quarts of the inferior, to 5*l.*; and upon 782 cakes, at 1*l.* per 100, to 7*l.* 10*s.*: total 52*l.* 10*s.* for one *morge*, which would be after the ratio of 26*l.* 5*s.* per acre. The expenses, not exceeding 10*l.* per acre, would yield a clear profit of 16*l.*

Should the oil of superior quality answer the description given of it, and be more palatable than the olive oil in common use, 12*s.* per gallon would, perhaps, be too low an estimate for our national character. For observation authorises me to assert it as a serious fact, that nothing has a greater tendency with us to depreciate articles of nutrition, especially if they approach to luxuries, than to render them too cheap: and although we complain universally, that such articles are extravagantly dear, we almost as universally suspect or despise whatever may be purchased at a very reasonable price. But as retailers are both able and willing to obviate this objection, the above statement for the vender in wholesale may be permitted to remain.

But there is another important point of view in which this subject may be considered. Successful attempts have lately been made to procure opium from the poppy, in no respect inferior to that imported from the East*; and it is asserted, that although it may be afforded at a very inferior price, the product would afford ample profits to the cultivator. As the opium issues from the rind, and the seeds have been proved not to partake of its narcotic properties, an important inquiry presents itself, Whether the poppy may not be cultivated with a view to both articles? This can only be determined by solving another question: Will the incisions made in the green and unripe capsulum, and the exudation of its juices, prove injurious to the seeds in this advanced state of its growth? The argument from ana-

* See Transactions of the Society instituted at London for the Encouragement of Arts, &c. on the mode and advantages attending the cultivation of opium. Vols. xiv, xv, xvi, xviii.

logy,

logy, which is the only mode until we can obtain facts, appears to favour the negative of the question; not only as there is no immediate correspondence in the qualities of these two parts of the same vegetable, but as many experiments have proved that by checking the growth, or weakening the vegetative powers of one part of a plant, they are increased and improved in another.

Desirous of obtaining some information concerning this interesting subject, I sowed, in the year 1804, about half a lug of garden ground with the white poppy seed; and when the heads were advanced to a sufficient state of maturity, I scarified the external surface of one portion of them with a penknife, suffering the others to remain entire; and though the exudations were very considerable, there was no perceptible difference in the colour, taste, or size of the seeds; excepting where the incisions passed through the whole integument, which frequently happened from the imperfection of the instrument, and my inexpertness. The seeds which lay nearest to the openings were discoloured by the admission of external air, but the taste of the seed was not injured.

This little experiment served to convince me that the seeds of the poppy are peculiarly grateful to birds, rats, and mice. The first dexterously made large holes in the lower surface of the ball, through which the seed fell to the ground; and they thus materially injured a considerable portion of my crop while it was standing; nor were the latter less destructive, when the poppy heads were spread upon the floor of the summer-house in order to dry them. I was, however, indemnified for this loss, by observing, that not a single instance of mortality presented itself to evince the noxious quality of the seed.

If future experiments should prove that both objects be pursued by the same culture, scarcely any plan can be devised which would prove equally profitable to the cultivator, and more beneficial to the community.

I am not so sanguine, gentlemen, as to expect that any person, upon reading the above account, will immediately resolve to cultivate the poppy to a great extent, as an article

article of profit. There is often a long repose between the acquisition of knowledge, and the application of it to practical purposes; and in this case I allow that many difficulties are to be surmounted before the open and avowed consumption of this oil would be sufficiently extensive to make the production of it an object of sufficient magnitude. But the increasing demands for oils of all sorts in our extensive manufactories, and by the daily improvements in our provincial towns, the immense sums expended in the importation of foreign oils, and most probably of this very oil under a false name, and the daily increase of their price, render a power in reserve most desirable. The time may arrive when the scarcity of oils for domestic use may increase to an alarming degree; in this case, the general reluctance to the use of those which are now deemed of an inferior quality may in a great measure subside, and we may perhaps rejoice at being supplied at a cheaper rate with that very oil which passes smoothly among us under the fictitious character of genuine oil of olives. I shall at least enjoy the satisfaction of putting it in the power of the public to assist themselves at some future period; and take encouragement respecting the success of my endeavours from the nature of this very plant, which is frequently known to lie for years in the soil in a state perfectly inert, until some favourable circumstances may have promoted a vigorous vegetation, to the surprise and alarm of the farmer, who has uniformly mistaken it for a weed.

N. B. It may be objected that in the above estimate of the profits mention is not made of the duties which may hereafter be imposed by government, and become considerable deductions. But this objection has no reference to our first essays. The duties will not become an object until the product of poppy oils shall sensibly diminish the importation of foreign oils; and in that case the wisdom of government will doubtless prevent their rising so high as to operate as a discouragement to a culture which would turn the balance of the oil trade in our favour; and, should we be able to extend this culture so far as to export the article, a very moderate duty upon both home consumption and exportation may prove

prove more than equivalent to the duties at present collected.

Since writing the above, I am informed by a person who deals largely in foreign oils, that letters from Leghorn announce an alarming deficiency in the last year's product; that the quantity is very small, and of a very inferior quality. This information should operate as an additional motive to the attempt recommended. The injury induced upon olive trees by inclement weather is frequently to such an extent, that it can only be repaired by the slow growth of new plantations. This circumstance gives an astonishing advantage to a substitute, of which, by its being an annual product, the deficiency of the most unfavourable year cannot be equally extensive, and would probably be supplied by the increased abundance of the year ensuing.

LIII. *History of Astronomy for the Year* 1805. *By* Jerome de Lalande.

[Concluded from p. 253.]

The meteorology of 1805 has been remarkable for the variations of temperature. It froze in the months of March, June, and September: on the 17th and 18th of December a degree of cold of from 6° to 7° froze the Seine at Paris; and on the 31st we had the same temperature as in spring.

On the 7th of December, at eight o'clock in the evening, at Basle, in Switzerland, the inhabitants thought themselves at the mouth of a furnace. This heat lasted three hours.

There was a hurricane on the 13th of December, in which several vessels were lost.

Perhaps the auroræ boreales, which have such an intimate connection with electricity, and which are constant in the north, may occasion storms which determine the winds, and contribute to these unaccountable variations of the seasons in our country.

There happened a phænomenon this year which furnished me with an opportunity of explaining the origin of hurricanes. At Belfort, on the 4th of July, there was one of those hurricanes, so rare and extraordinary in Europe, which tore

tore up trees, and unroofed houses. Hitherto I had not been able to ascertain the cause; but my journey to Lyons furnished me with an idea which might be realized. M. Molet, an intelligent medical professor, found, from his notes, that it thundered that day at Lyons. In passing through Sens I saw M. Soulas, who informed me that the wind had changed from north to south. The journals informed me that there was a dreadful storm at London on the same day.

There is, in my opinion, a mass of electrical clouds of 100 myriametres in extent, the detonation of which had made an immense vacuum, and that the air rushed with great violence to fill up this vacuum. I had a confirmation of this on the 11th of January 1806. Some dreadful claps of thunder at Brest, Rouen, Chartres, and Ypres, produced tempests and hurricanes which overturned the chimneys of the houses at Bourdeaux, Besançon, Nancy, and Dijon. Loud claps of thunder are very rare at this time of the year, but the south wind made it very hot; the atmosphere was rainy, the lower clouds were near enough to draw sparks from the earth to an extent of 60 myriametres: there were also earthquakes at the same time.

The hurricanes of the Isle of France and America, which are much more violent, require us to suppose the stormy masses much larger; but to these must be added water-spouts and submarine eruptions.

M. Fiot, inspector-general of health, has sent me the results of the heights of the river, observed every day during the year 13. The mean state of the river this year was 1·35 metres on the scale of the bridge of La Tournelle, instead of 1·24, which I had found by a mean of eighteen years, 1777—1794. The year 13 has been considered as a rainy year, nevretheless there have been years of 1·73, as 1787; whilst in 1803 we had only 0·59. This height is relative to the low water of 1719; but the river has sometimes been lower by some centimetres.

The Turin Academy has published its memoirs for 1804 and 1805, in which there is a new barometer of M. Vassalli-Eandi, with heights measured in Piedmont.

M. Beraud, who for these thirty years past has made an immense

mense quantity of meteorological observations in Piedmont, and who still continues them, notwithstanding his great age, has sent us those of 1805.

Meteorology and navigation may equally claim a memoir by M. Biot, which explains, by means of an interior magnet, all the declinations and inclinations of the needle observed by M. Humboldt in his travels.

There is an unpublished memoir of Tobias Mayer, of which his son had the goodness to send me an extract. It contains an hypothesis by which he explains the observed inclinations and declinations. He supposes a very small magnet in the interior of the earth having two poles, the centre of which magnet is removed from the earth by one-seventh of the radius, and recedes from it each year one thousandth part.

The line drawn from the centre of the earth through that of the magnet was in 1750 at 201° of longitude and 17° of north latitude: the longitude increases 8′ a year, and the latitude 14′.

The axis perpendicular to the line joining the centres, drawn in such a manner that the plane which passes by this axis and this line, is inclined to the meridian of this line 11½ degrees towards the east on the north side; and this angle increases 8½ minutes a year.

M. Azuni has published a dissertation upon the origin of the compass, in order to prove that the French were the first who used it: it was known in France in the 12th century, by the name of *mariniere*: it was used in the reign of *Saint Louis*. *Gioia d'Amflai*, to whom it has been attributed, did not live till about the year 1300. The figure of the *fleur de lys* has been used in the compasses of all nations. I have already remarked, in my Abridgment of Navigation, that father Ximenes, the celebrated Italian astronomer, has proved the priority of the French in his work *Del Gnomone Fiorentino*, p. 59.

Mr. Earnshaw and Mr. Arnold, two English watch-makers, on the 7th of June 1804 presented to the Board of Longitude of London their escapements for time-keepers

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or chronometers; and the Board of Longitude has published them. That invented by M. Breguet at Paris, is described in the volume of the History of Mathematics of Montucla, where I have given the history of machines.

Nautical astronomy has been enriched with an important book entitled "A complete Collection of Tables for Navigation," by M. Mendoza, a Spanish officer, who has resided some time in England; comprised in 727 pages, in large quarto. It contains all the tables necessary for correcting the altitudes and distances by the simplest method hitherto found out; for it is reduced to the addition of three numbers, which we take from these tables. We have also tables of logarithms, semi-diurnal arcs, amplitudes, the most extensive table of the longitudes and latitudes of places on the earth, and, generally speaking, every thing that is requisite at sea. If we add to the above the horary tables which I published at great length in my Abridgment of Navigation, in 1793, sailors will require nothing more to know when they are in any part of the world; and I hope that, in spite of all the efforts of the English, the French will not be the last to profit by these advantages, under an emperor who is so well acquainted with the importance of a navy.

These tables render the calculations so easy, that navigators would do wrong not to use this method of finding the longitude. M. Mendoza is at present occupied with a more complete treatise on nautical astronomy.

M. Luyando has published at Madrid twenty-three charts, upon which we may find, by a pair of compasses, the sides or angles of spherical triangles within a few minutes, and the correction of the distances observed at sea within a few seconds. These charts, as well as those of M. Margett's, at London, may be useful to navigators who are not fond of calculations. Those of M. Luyando will cost less, but their use is rather more difficult.

M. Duval le Roi has published at Brest, Elements of Navigation, which are worthy of that able professor.

M. Depaquit has published a new Theory of the Tides.

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I did all I could to dissuade him from it: my efforts have been in vain; and I only mention this to prevent the public from being deceived.

The observations of the tides have been continued in several harbours: at St. Maloes, by M. le Cerf; at Ostend, by M. Porquet; at Sables d'Olonne, by M. Depoge.

The tide at Brest, at the spring equinox, having taken place with an east wind, I requested some observations of it, and I found that it had not surpassed the mean tide; which completely confirms the system I have laid down in my treatise upon the flux and reflux of the sea.—that when the equinoxial tides are highest, it is occasioned by the wind.

Geography has been enriched by various important voyages. That of Hearn has been published, made between 1769 and 1772, to the north-west of Hudson's bay. It extended from Churchill's river, which flows into the bay, to the 72° of latitude, an extent of 100 myriametres, in the country of the Esquimaux who inhabit the neighbourhood of the Copper river. The maps of North America will be much altered by this voyage. The communication from sea to sea, so often spoken of, seems to be more and more illusory.

Captain Krusenstern, the commander of two Russian vessels, with which he sailed round the world, arrived at Kamschatka on the 8th of August, 1804; after having doubled Cape Horn and landed upon the Marquesas and Sandwich islands, he proposes visiting China and Japan. The account of this voyage will be very interesting, and will do honour to the Petersburgh Academy, who obtained permission from the emperor of Russia for it to be undertaken.

In the 28th number of the Annals of the National Museum of Natural History, February 1805, we find that captain Lewis is gone to reconnoitre the Missouri as far as its source. He will afterwards search for the nearest river to the westward, and will descend by it to the Pacific Ocean. This expedition, composed of twelve persons, will probably return in a few months. The president, Mr. Jefferson, intends

tends sending persons to explore some other rivers which as yet are absolutely unknown.

On the 6th of February 1805, Mungo Park sailed from Portsmouth, in order to return into the interior of Africa, where he before made an interesting expedition.

Lieutenant Ohlsen is occupied in drawing a map of Iceland. He speaks of a boiling spring at Stort, which first made its appearance in 1784. It throws up the water to the height of 300 feet. This proves that there is a great deal of water in the interior of the earth, and supports the hypothesis by which I have explained the sinking of the waters which covered our mountains, and which I think must have entered into the interior cavities.

M. Schubert, the able astronomer of Petersburgh, has set out for China with the Russian embassy. We may therefore expect from him some good observations on the geography of Asia. We understand that the embassy had arrived upon the frontiers after a journey of 600 myriametres, and that they were within 130 myriametres of Pekin; but the Chinese are not fond of much company, and M. Schubert returns by the north: his journey will be very useful in many respects.

M. Portalis, minister of religion, wishing to send some missionaries to China, concerted for that purpose with M. Brunet, the superior of St. Lazarus. They hope to be able to set out this year, accompanied by an astronomer, who is already preparing his instruments. The manuscripts collected by M. Bertin on the subject of China, are in the hands of a secretary, who has offered to sell them to government. M. Billien and M. Abaric, attached to the foreign missions, have been in China, and know the Chinese well. Thus we have not lost all hope of seeing this branch of knowledge come again into favour in France.

The geography of Europe has also received some recent advantages. M. Benzenberg writes from Dusseldorf, that the king of Bavaria has ordered some accurate maps of the country of Berg to be drawn.

M. Henry has returned from Alsace; on account of the

war,

war, he could not venture into Spain to continue the measurement of the meridian. He will resume his triangles for his degrees of longitudes, or rather he will continue his triangles of Helvetia.

M. Hennet, imperial commissary of land-surveying, has published the laws, *arrêts*, and instructions, the circular orders and decisions on that subject, in two volumes octavo. He will give a third in 1806. They are busily occupied in drawing plans of every part of France.

The chief geometer of the department of Aveyron, and the learned professor at Rhodes, M. Tedenat, are occupied in rectifying the principal points of the plan with a circular instrument of eight inches radius, made by Messrs. Becker and Michel, which gives the angles certain to two seconds.

While we have been waiting for this complete and detailed description of France, there has been published a geographical, statistical, historical, and political dictionary of France, containing a description of the cities, villages, the history, population, mineralogy, hydrography, commerce, natural and artificial productions, the old and new government, the civil, military, and ecclesiastical institutions, and a dictionary of the colonies, with a general map, &c. in five large volumes in quarto. Fifteen years' labour has been bestowed on this great work, and much pains and expense: several men of learning have assisted in it; they are not named, but they certainly deserve to be known. The dictionary of Expilly, in six volumes folio, was never finished, although I had repeatedly requested it might: this perhaps will supply the place of it.

As it is necessary we should conclude our History of Astronomy with an account of the losses it has sustained, I shall begin with M. Ratte, who, as an astronomer, has for a long period been an honour to the academy of Montpelier.

Etienne-Hyacinte de Ratte, son of Jean-Pierre de Ratte, and of Gillette de Flaugergues, was born at Montpelier the 1st of September, 1722. His taste for the sciences, and principally for mathematics, displayed itself very early: he had masters of every kind; he studied all the sciences with ardour, and the extent and the variety of his acquirements

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at so early an age, astonished the literary characters who were then very numerous at Montpelier. The Royal Society of Sciences established in that city in 1706, were anxious to receive among them so promising an associate, and in spite of the regulation, requiring the members to be twenty years of age, that requisite was dispensed with in favour of young de Ratte, who in 1741 was admitted a member. In the year following he was elected perpetual secretary; the duties of which function he continued to discharge with the greatest credit until the Royal Society of Montpelier was suppressed. He published some volumes in 1766 and 1778, under the title of *Memoires*, afterwards under the title of *Assemblées et Bulletins*, and in every volume there are some articles, eulogies, and memoirs of his composition. His eulogies on Plantade, Clapies, Lapeyronie, Venel, Lafosse, Pitot, Sauvage, Linné, Leroy, Lamure, &c. have proved both the extent of his knowledge and his ability as a writer.

He also composed several physical and mathematical memoirs upon vortices, fluids and aloes, which are printed in the Collections of the Royal Society; and he furnished for the French Encyclopedie the articles *cold, ice, hail*, &c.

The famous prediction of Halley, respecting the return of the comet of 1682, which he fixed about 1757 or 1758, occupied the attention not only of astronomers, but also of all the scientific men of the day. M. de Ratte was anxious to participate in the discovery of this comet; and this was the occasion that determined him to pursue the study of astronomy. He was among the first who discovered it at its issuing from the sun's rays. These observations gave him pleasure, and he never afterwards omitted observing every comet any way remarkable. He also observed the transit of Venus in 1761: the observation which he made of it at Montpelier was one of the most complete; and it became the basis of much calculation upon the sun's parallax, with which M. Ratte was occupied. He afterwards made several observations upon the transits of Mercury over the sun, upon the eclipses of the sun and moon, the satellites of Jupiter, and occultations of the stars; the greatest part of which have never been published. He directed

rected M. Poitevin's studies towards astronomy, who is still occupied with it, and whose observations have been frequently printed. He often regretted very much, that no astronomer was established to make use of the observatory of Montpelier.

M. de Ratte, the father, died in 1770: he was deacon of the counsellors of the court of aids at Montpelier. The wishes of his family and of the public engaged M. Ratte, jun. to take that office upon him; and he discharged the duties of it, until the suppression of the court of aids, in the most distinguished manner. He was often the principal organ of that assembly on important occasions and in times of difficulty: it was totally set aside, however, in 1793.

At the conclusion of the reign of terror, the members of the old Royal Society who had the good fortune to escape proscription, conceived the idea of re-establishing it under the name of the Free Society of Sciences and Belles Lettres. This idea succeeded; the society was formed, and M. de Ratte was at first appointed secretary, and then president. This society has already published two volumes of its memoirs under the title of *Bulletins*, which contain interesting researches and observations. There is a discourse by M. Ratte in the bulletin of the 3d of May, 1801. We there see that his zeal set his age at defiance.

M. de Ratte was chosen a non-resident associate of the National Institute, and afterwards named a member of the legion of honour.

He had enjoyed the most perfect state of health during the whole course of his life; but he was latterly attacked with a retention of urine, the attacks of which becoming very frequent, were exceedingly painful; but the habit he had taught himself of suffering without complaining, and his natural gaiety, made his friends forget that he was unwell. He was present at the academy on the 24th of June, 1805; and it was not until the night of his death that he was thought in danger. He expired on the 15th of August, aged 83.

M. de Ratte was of a low stature; he had a happy and intelligent physiognomy; he conversed with pleasantness and good humour; he never contradicted any one; and he always descended to the level of every person with whom he conversed: his modesty and simplicity were extreme; and it was surprising to find in a man so consummately learned, the candour and simplicity of a child: his memory was surprising: he lived a life of celibacy, and by his death, the house of Ratte, established in Languedoc since 1433, became extinct. This family was originally from Bologna in Italy, and was known so early as the year 1125 by the talents and virtues of Hubert de Ratte, cardinal and archbishop of Pisa, and by the military exploits of Jean de Ratte, count of Caserta in the kingdom of Naples.

The astronomical observations of M. de Ratte have been collected by his nephew M. de Flaugergues, at Viviers. M. Poitevin, secretary of the academy, who is also an astronomer, has published his eulogy at great length at Montpelier.

We have also lost M. Romme, an able professor of navigation at Rochefort. He had laboured in astronomy along with me in his youth; I procured a place for him at Rochefort, and he made several observations.

In 1771 he published a method for determining the longitudes at sea; in 1800 he gave a model of a calculation for finding the latitude and longitude at sea, wherein he seems to think the method of Borda inconvenient in certain cases. M. Delambre, in the *Connoissance du Temps*, An XII. p. 263, has shown that several authors are actually mistaken in thinking that the sum of the two altitudes and the distance surpasses 180°; but this cannot happen.

Romme gave in 1778 the art of mast-making; in 1781, that of sail-making; in 1787, *L'Art de la Marine*, or principles and general precepts on the art of building, manœuvring, and steering vessels; a work very much esteemed among navigators.

He had composed several other works, which Barois the elder was upon the point of printing in 1798; but I was particularly desirous of seeing published his tables of the winds, the tides, and the currents in every sea; which appeared in two volumes in octavo. In 1796 he sent me

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some curious observations upon the tides of the Charante; in which there are some particular circumstances which I intend to publish in a second edition of my Treatise on the Flux and Reflux of the Sea.

In 1787 he made some experiments upon the resistance of water, which ship-builders are much in want of. I have given the result of them in Montucla's History of Mathematics, tome iv. p. 454, according to the account given by the commissaries of the academy. He gave a marine vocabulary in French and English; and perhaps no person was ever more usefully or more constantly employed in this grand art, which is the principal source of the prosperity and grandeur of states.

He was brother to the deputy, who obliged me to make the republican calendar in 1793, and who perished in the troubles of the revolution on the 17th of June, 1795; the latter had been tutor to the children of count Stroganoff, a Russsian nobleman, who resided a long time at Paris.

We have also lost M. de Chabert, a celebrated navigator, of whom I shall speak more at length.

Joseph Bernard de Chabert, *ci-devant* marquis, *chef d'escadre* in the navy, commander of the orders of Saint Louis and Saint Lazarus, inspector of the marine depôt, free associate of the Academy of Sciences, and lately member of the Board of Longitudes, and of the Royal Societies of London, of Berlin, Stockholm, Bologna, and Brest, was born at Toulon, 28th February, 1724. He was the son of an officer of the royal navy, and he entered into the service in 1741; his father, when dying, caused himself to be carried on board the ship commanded by his son, and said, "I shall now die without regret."

In 1746 he went to Nova Scotia in a French squadron. He then saw how very defective the maps of America were; he was a witness to the dangers our vessels experienced, and he gave an account of them upon his return. Lemonnier undertook to request permission of the minister to allow him to remain at Paris for the purpose of studying astronomy, that he might go and remedy the inconveniences he had met with, and encourage the officers of the navy to

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pursue the study of a science which equally contributes to their glory and their security. At the age of thirty he received the cross of St. Louis, which he preferred to a pension. At the return of peace in August 1748, he presented a prospectus of the voyage and the observations. M. Rouillé and M. de la Gallissoniere furnished him with instruments; he set sail in 1750, in a frigate commanded by the marquis de Choiseul-Praslin, and he executed a chart of the coasts of Nova Scotia and Newfoundland, and of the banks and islands in the Gulf of St. Lawrence. I had the saisfaction of furnishing him with an object of comparison by my first observation of an eclipse of one of the satellites on the 2d of October, 1750. His voyage was printed in 1753, in 288 pages in quarto. It contains observations on the magnet and upon currents; details upon the calculations which navigators require; all of which evince that he had already become a very good astronomer, and his example excited emulation in the navy, where it was much wanted. The first volume of the *Savans Etrangères*, published by the academy in 1750, contains a memoir of M. de Chabert upon the longitude of Buenos Ayres. In the history of the year 1756, his observations made in 1753 along the coast of Spain and at Port Mahon are mentioned. In 1756 he gave several other memoirs upon the transit of Venus, upon hurricanes, and the eclipse of 1760.

In 1758 he was received a member of the academy; and on the 25th of April, 1759, he read at his public entry into it his idea of constructing charts of the Mediterranean, for the purpose of making a second volume of the *Neptune Français*, published in 1693, and thereby following the labours of Chazelle and Feuillée, who died in 1710.

He set out on the first of May, 1761: after having laid down the eastern coast of Spain, he passed into Sardinia, and crossed over to Fez, Algiers, and Tunis, where he succeeded in determining several important longitudes, by means of a transit instrument which he fixed on shore, and which he succeeded in placing in a few hours exactly in the meridian.

In 1767, after having sailed round the coasts of Sicily, he he steered for Tunis: he then went to Tripoli, and returned into the Adriatic gulf. In 1768 he commanded the Hirondelle, in 1775 the Mignonne, and in 1776 the Atalanta, on board of which was M. Choiseul. He was in possession of a time-keeper, without which he could not have drawn the chart of the Cyclades and that part of Greece adjoining: he had reserved this business to the present opportunity. The inscription he caused to be engraved in the grotto of Antiparos proves his great learning, and will be a monument of it to future travellers. At his return our able astronomer Mechain, being attached to the depôt of which M. de Chabert was director, passed several years of his life in reducing and calculating this immense quantity of observations, which had cost so many years of labour and travelling, and from which resulted the best charts of that part of the Mediterranean. Several times he was in imminent danger of his life. I hope his journal will be published, as it cannot fail to excite considerable interest from the anecdotes I have heard him relate.

The American war obliged him to exchange his scientific labours for the military marine service in the naval armaments of generals d'Estaing and De Grasse. In 1778 he commanded the Valiant in d'Estaing's fleet: in 1780 he had the *Saint Esprit*: he fought a battle, near the Chesapeak, with five English vessels, on the 5th of September 1781: he relieved the Diadem, which the English were very near having taken. He brought safely into a French port a convoy of 130 sail; he was then named *chef d'escadre*, and received the red ribbon; this was the reward of twenty-two naval campaigns, in fifteen of which he commanded corvettes, frigates, or ships of the line: he sailed during the whole of the war, which ended in 1783, first as commander of a vessel and afterwards as chief of a division, without ever neglecting his astronomical observations and the use of the time-keepers, as may be seen in the Memoirs for 1783.

The misfortunes of the revolution obliging him to remove from France, he went to England, where Dr. Maskelyne paid him every attention that could be expected from one great astronomer to another, giving him an unlimited credit upon his banker *, which M. de Chabert, however, would not make use of. He lost his sight suddenly in 1800; and this misfortune can only be attributed to his excessive labour in calculating observations. Having returned to Paris in 1802, he was received in the most distinguished manner by those heroes of France who were best acquainted with his talents and bravery. He received a pension in the month of December 1804: he was elected a member of the Board of Longitude, and would have been included in the reorganization of the Institute in 1803, had there been a vacant place. On the 4th of January 1805 he presented to the Board of Longitude a chart of Greece, with the particulars of the coast: and I trust we shall find many useful things among his immense collection of materials. Although blind, he never ceased his labours: he generally wrote or dictated until nine o'clock every evening; and when M. Nevev, his secretary, left him, he always felt regret. We have many times found his memoir useful in the geographical discussions of the assembly. Some days previous to his death he asked me for some observations which I had received from Spain, in order to compare them with his own; and two days before his death he desired some passages of his Memoirs upon Pensacola, in Spain, where he laboured in 1768, to be read to him. Whenever his journals are published, the world will be astonished at his ardour, his accuracy, his labours, his dangers, and the presence of mind with which he remedied all the inconvenient circumstances that opposed his pursuits.

An inflammation of the lungs carried him off in nine days; but he died without pain, consoled by religion and adored by a beloved family.

He married, in 1771, mademoiselle Tascher, the daughter of a gentleman at Coire, in the Grisons, and sister of a late president, who was intendant of the *Isles-du-Vent*, and

* It is presumed Lalande has mistaken Dr. Maskelyne for sir Joseph Banks in this account; for he lived almost the whole time either with sir Joseph or at his expense. Dr. Maskelyne was always very civil to him, and the marquis usually spent about a month at the royal observatory every summer during his stay in England.

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who was considered the best informed man in Paris, and frequented as such by men of letters. M. de Chabert left one daughter, madame Roland, who, in concert with her mother and her husband, afforded the greatest comfort to this excellent man. M. Roland, who has been already distinguished by his travels in Egypt, is at present attached to the army of Naples, and his extensive knowledge will enable him to profit by his interesting travels.

No person is better qualified to speak of M. de Chabert than myself, who have known him since the year 1750, and have lived with him: his benevolence and his mildness were universally admired by his inferiors. He once found a new-born infant upon a desert, nearly dying, whom he reared and educated. She became an interesting girl, but died too young to enjoy the full extent of his bounty. M. de Chabert was one of the first among the officers of the navy to show an example of zeal and learning: he was also one of the first who made use of his dignity to promote the good of the service itself; and his memory will be preserved among those who have rendered service to all nations, since the navy is one of the sources of their prosperity, as it is one of the means of improving the human species. M. de Missiessi, known by his campaign in America in 1804, and by his work on the stowage of vessels in 1789, was the son of a first cousin of his; and the name of this worthy successor of his fame was in the expiring lips of his illustrious parent.

On the 9th of September we lost M. Dulague, born at Dieppe on the 26th of December 1729, an able professor of navigation at Rouen, to whom we are indebted for several works and observations. The Academy of Rouen will publish his eulogium.

M. Lesage, who died at Geneva, was occupied with celestial philosophy. His Newtonian Lucretius, upon the cause of universal gravitation, is a curious work, which I have quoted in my Astronomy, art. 3530.

Victor Comeiras, who died in the month of October, published Bailly's History of Antient and Modern Astronomy, in two vols. 8vo, and thereby brought that work within

within the reach of a greater number of readers by reducing the price.

M. Arago, secretary to the observatory, devotes himself entirely to astronomy, and we have reason to hope he will repair our losses.

Isaac Lalande, the third of the name, has begun to turn his attention to astronomy: he both calculates and observes. The first eclipse which he calculated made us acquainted with an error of a quarter of an hour in the calculation of the next eclipse. I baptized him *Isaac* in honour of sir Isaac Newton, whom I wish him to take for his pattern, and that he might always have him in his memory.

M. Contè, who died on the 6th of December, at the age of fifty years, was not decidedly an astronomer, but his labours in aërostatics authorise me to speak of his loss here; which is, indeed, truly lamentable both for the arts and sciences, or, in more extensive terms, for the whole human species.

LIV. *Method of ascertaining whether Wines are adulterated with Litharge.* By M. Nauche, *Physician* *.

The property possessed by litharge, or the demi-nitrous white oxide of lead, in depriving wines of a bad quality of their bitterness or pungency, and thereby rendering them mild and pleasant to the taste, has induced some avaricious speculators to raise the price of their wines by adding a certain quantity of litharge to them.

This reprehensible practice, occasioning colics and various other diseases, renders it dangerous to use wine. It is necessary, therefore, to be in possession of the means of ascertaining this adulteration, or of ascertaining the innocence of such persons as may be unjustly accused of it.

The agents employed for this purpose are the sulphuric acid, and water charged with sulphuretted hydrogen gas.

When pure sulphuric acid is poured upon wine contain-

* From *Biblioth. Phys. Econ.* July 1806.

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ing litharge, it causes a white precipitate in the liquor which soon falls to the bottom of the vessel.

The same acid poured upon unadulterated wine merely brightens its colour a little, without producing any precipitate.

This method, although a good one, is not so accurate as the employment of water charged with sulphuretted hydrogen.

In order to prepare this water, it is only necessary to put into a phial a paste made of iron filings and sulphur; pouring afterwards into it some drops of sulphuric acid, and liberating the gas which is produced from the mixture into a flask filled with water, by means of a bent tube with which the phial is furnished.

Poured upon unadulterated wine, this water does not occasion the least change in it; while, on the other hand, it renders the wine adulterated with litharge black and flaky, and produces an abundant precipitate, which soon falls to the bottom of the vessel.

Some people make use of sulphur or alkaline sulphurets; but these re-agents produce in pure as well as in adulterated wine a change of colour, and precipitates so little distinct from each other, that it is difficult to establish any thing decisive from their differences. It will be much better to adhere to the processes here pointed out.

LV. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

JANUARY 8, 1807. The Right Hon. Sir Joseph Banks, bart. president, being recovered from his indisposition, took the chair, when an interesting mathematical paper was read on the power of friction and the resistance of bodies, applied to the cogs of wheels, &c., by Davis Giddy, esq. M.P. The principles, which were here slightly introduced in the form of a letter at the request of the president, Mr. Giddy observed had been discovered nearly at the same time by the mathe-

mathematical professor at Cambridge, who he hoped would be induced to lay them before the public in a more detailed manner. Not being able, at present, to give the whole of this very ingenious paper, we shall not mutilate it by any imperfect abridgment.

January 15. The President in the chair.—E. Home, esq. furnished the history of two cases of cataract in which the operation of couching was successfully adopted. The observations on light and colours, and on the figures of bodies, made by the patients, the one a boy of seven, the other of thirteen years of age, were similar to those recorded in the writings of Pott and Ware, whose opinions these two cases only served to confirm. One of these patients discriminated correctly the difference between colours, but called a card that was held before him round, until that he was suffered to touch it with his fingers, when he observed that it was square. In the dimensions and distance of external objects they both evinced extreme ignorance, and seemed to acquire that knowledge gradually, as children learn to read.

January 22. On this evening the Society proceeded to the election of a secretary and member of the council, in consequence of the death of the late E. W. Gray, M.D. The junior secretary, Dr. Wollaston, naturally succeeded the senior, Dr. Gray; and H. Davy, esq. was unanimously elected junior secretary and member of the council. The right hon. C. Greville then took the chair, and Mr. Davy commenced the reading of a paper, by Dr. Herschel, on sir Isaac Newton's explanation of the circular rays of light seen between two lenses placed on each other, or brought in contact in various manners. Dr. Herschel rejected the supposition of sir Isaac in ascribing fits of transmission to light, and proceeded to detail the various minute experiments he performed with plano-concave and plano-convex lenses in order to produce these circular rays. The reading of this curious paper was not concluded.

SOCIETY OF ANTIQUARIES.

Jan. 8, 1807. The earl of Leicester, president, in the chair. Dr. Latham communicated some verbal corrections

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to a paper in the 14th volume of the Archaiologia, relative to the names of some birds formerly found in this country.

A very antient and original charter granted by an earl of Buchan to the family of Gartshore of Gartshore, ancestors of the present Dr. Garthshore, was read. This charter, like most of those given in former times, had no date; but from the name of the earl of Buchan, and other collateral circumstances, it appeared not to be of a later date than 1250, and was probably granted during the reign of Alexander the First or Second. In sir John Sinclair's Statistical Account of Scotland it is observed, that the lairdship or manor of Gartshore has continued in the same family more than six centuries, and this charter confirms the fact. It is a curious, and certainly among the most antient documents of former times.

Jan. 15. The Rev. Dr. Hamilton in the chair. Mr. Jackson presented the society with some specimens of antique marble belonging to the churches of Florence. He was unable to give any account in what provinces these marbles were found.

Jan. 22. The earl of Leicester, president, in the chair. The indefatigable Mr. Lysons communicated some more of the records in the Tower.

ORIGINAL VACCINE INSTITUTION,

Broad Street, Golden Square.

A meeting of the members of the Vaccine Club was held on the 4th of December, at the British Coffee-house, W. Devaynes, Esq. M. P. vice-president, in the chair. After the minutes of the proceedings of the institution since the former meeting were read, it was determined that the most important part of the investigation of the cow-pock for the public at present is, the ascertaining the proportional number of cases of small-pox amongst those who have been inoculated for the cow-pock. It appeared, that in the practice of this institution the proportion was a little more than one case out of each thousand inoculated since the commencement of this establishment. These cases it was requested might be well considered, to discover whether or no they could

could have been prevented by any known circumstances of inoculation. On examination it was admitted that no such circumstances were known, and of the reality of the subsequent occurrence of the small-pox there was the most convincing evidence.

The next question for consideration was, the proportion of persons who might be still susceptible of the small-pox after vaccination, but who had not been yet infected, either because they had not been exposed, or, if exposed, because their constitutions were not at that time susceptible.

It was proposed to defer the attempt at an estimate till subsequent occurrences should furnish data, except that it might be admitted that at least not more than one half had been infected with the small-pox, who were susceptible; so that at the fewest number, according to the practice of this institution, was one out of five hundred, who might be considered as susceptible of the small-pox after vaccination. These results, it was remarked, had induced the institution not to warrant any person as secure without the test of a second inoculation.

In the fourth place, inquiry was made, What might have been the probable proportion in the same region of practice in other hands than those of this establishment? This could not be accurately stated for want of documents; but there was no reason to believe that it was less than above mentioned, in all probability it was greater, if confidential communications were to be depended upon.

In the fifth place, the question was proposed, What might be the proportion of failures elsewhere than in London, in our own country?

It was observed, that in all probability the proportion of failures of persons duly vaccinated was much smaller in other parts of the imperial kingdom than in London, for which two reasons might be assigned.

1st. Because there was a less chance of exposure to infection.

2d. Because there had probably not been any epidemic prevalence of small-pox as there had been in London. Hence, if even one person out of a hundred was left susceptible

ceptible of the small-pox, it was very conceivable that a failure might not yet have occurred in many places; and it was remarked that on this account, without supposing any imposition, a delusion might subsist for some time that persons were secure who in reality were not so.

With regard to the accounts from abroad, the evidence was almost entirely negative with regard to failures. This evidence, however, was stated to be of little weight comparatively to that in London, for various reasons.

1st. For the reasons assigned for the exemption from failures in this country out of London.

2d. Because the mass of people had not the liberty of acting or speaking as they profess under our government: however, lately accounts had arrived, and more might be expected, of failures in India.

In the last place was discussed the proportion of failures in the small-pox inoculation; and here it appeared astonishing, that any sensible practitioners should ever have considered the small-pox inoculation as upon the same footing as vaccination upon this point. It would be unjust to place the two modes of inoculation upon the same footing; for the most extensive inoculators in this country had declared they had never seen the small-pox twice in the same person. Such was the evidence of the late Dr. Archer, baron Dimsdale, sir William Watson, Dr. Woodville, and the whole family of the Suttons. Thus, while in the course of sixty years, or from 1746 up to 1806, at the Small-pox Hospital, amongst 60,000 persons who had undergone the small-pox, not one had been known to have taken the small-pox a second time; yet in the course of seven years vaccine practice at the same place, it was well known that a certain number had taken the small-pox subsequently to the cow-pock.

It was not to be concluded, however, from these statements, that vaccination was not greatly preferable to variolation; but it was to show the necessity of a second inoculation until the circumstances should be known in which security could be given by one inoculation.

It was the conduct of the too sanguine and prejudiced advocates that made these statements necessary; but it was allowed that, with the precaution just mentioned, there was good reason to believe vaccination would be highly beneficial to society, and merited the further investigation required for secure practice.

FRENCH NATIONAL INSTITUTE.

[Continued from p. 275.]

M. Berthollet has taken up the subject of affinities with a perseverance worthy of its importance; and he has this year communicated to us a third course of his researches.

He has shown that we may by means of compression combine with the three alkalis quantities of carbonic acid much greater than usual, and thereby form salts perfectly neuter, like all the other acids.

It is to these complete combinations that he reserves the name of *carbonate:* to the ordinary combinations he gives the name of *sub-carbonate*; and he shows that there are between the one and the other several intermediate combinations.

It is the same with the earthy carbonates and several sorts of salts: the phosphate of soda, for instance, can crystallize both with excess of acid and excess of base.

Indeed the partisans of the old doctrine suppose, that in these cases of variable proportions there is no combination, but the superabundant principle is simply interposed in the free state between the molecules of the two principles combined in the ordinary proportion.

M. Berthollet says in answer, that if this was the case, sulphuric acid poured upon a sub-carbonate would at first take up some free alkaline molecules, before attacking those which are combined with carbonic acid. But this is not the case; for the least drop of the first acid immediately produces the liberation of the second, that is to say, effervescence. The acidulated sulphate of soda effloresces in the air, *i. e.* it there loses its water of crystallization; which would not take place if the superabundant sulphuric acid was present in the free state, because there is no substance which more strongly attracts humidity than this acid.

M. Ber-

M. Berthollet had given a method of estimating the degree of acidity of the different acids, and the degree of alkalinity of the different bases, by the quantity required by each of these kinds of substances in order to saturate or neutralize the other completely, so as that the combination may evince no sign of acidity or alkalinity.

He confirms this method by showing, that the proportions of these quantities are constant, and that if it requires, for instance, twice as much of any kind of acid to saturate one base than another, there must be twice as much of every other kind of acid to the first than to the second.

But the degree of resistance of heat does not correspond to this force, and it is easier, for example, to decompose by fire the carbonate of magnesia than that of lime, although the affinity of these two earths for the acid is nearly the same. It is that the former carbonate has much more water, and, as other experiments show, that water favours the liberation of carbonic acid.

The consequences of these facts to all the branches of chemistry, and particularly for the theory of analyses, are incalculable.

The tables of the affinities and a great part of the analyses made to this day are invalidated by the above; and in short, experience proves that almost all these products have need of being revised. For example, M. Klaproth, and after him M. Vauquelin, have found a fifth of fluoric acid in the topaz, where its existence had never been suspected. This stone therefore passes into the class of acidulated substances.

Another mineral, hitherto regarded as a stone, passes into the class of the metals: it is that which was formerly called *oisanite*, or octaëdral schorl of Dauphiny, and which M. Haüy has recently named *anathasis*. M. Vauquelin found nothing in it but the oxide of titanium, as in the other mineral called red schorl.

This fact is important, because it presents two minerals among which chemists cannot yet find any essential difference of composition, although their physical qualities, and particularly their crystallization, are all different.

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In mineralogy there has also been a similar case in that of the *arragonite*, where chemists have only found a carbonate of lime, although its weight, hardness, fracture, and crystallization differ much from that of calcareous spar or common carbonated lime.

A different example, but which also establishes a sort of opposition among the physical and chemical characters of the minerals, has also presented itself this year.

It is the ore of iron known by the name of spathic iron *. It constantly has the same crystalline form as carbonated lime, and indeed it often contains a very considerable quantity of the latter substance. M. Haüy had ranked it among the varieties of this species, considering the oxide of iron in it as merely accidentally arising out of the crystallization of the lime, nearly the same as it is with the sand in the singular crystals of stone found in the forest of Fontainbleau: in fact, we have known for a long time past that the quantity of iron is very variable in it.

But two young chemists, Messrs. Drappier and Descostils, have discovered that the quantity of lime varies still more considerably in spathic iron, that sometimes there is none at all in it; and that magnesia and the oxide of manganese are often in as variable quantities, according to the specimens.

Here, therefore, there are very different combinations which present themselves under a form always the same.

These kinds of difficulties, these apparent oppositions between two branches of one and the same science, or between two methods of regarding objects, must be referred to some imperfection in the principles of the one or other of the two methods, and merit all the attention of the friends of truth. Researches on the subject would probably terminate by the discovery of some new general fact, which conciliates every thing.

The labours upon native platina, of which we have spoken in our two last reports, have been continued this year by different chemists, and have led to some clear and satisfactory results.

* See Philosophical Magazine, vol. xxv. pages 31, 245, 317.

M. Fourcroy

M. Fourcroy has given an account of these labours in a memoir, wherein he has done justice to those who have taken part in them.

The following is an abridged account of these labours:

It will be recollected that M. Descostils, in endeavouring to account for the various colours of the triple salts of platina, perceived that the red colour of some of them was owing to some unknown metal.

Messrs. Fourcroy and Vauquelin on their part, on examining a black powder which remains after platina has been dissolved, and finding that in some experiments a very strong smelling metallic vapour arises, that in others the substance is manifested in a manner more fixed, they also regard this powder as a new metallic substance, the different properties of which they attribute to different degrees of oxygenation.

But at the same time Mr. Tennant of London had examined this same black powder, and succeeded in decomposing it once more into two different metals, the one fixed and the other very volatile: and Mr. Wollaston, another English chemist, operating upon the solution which was formerly thought to contain only platina, found in it two other metals, different both from platina and from those which compose the black powder.

Thus, after the long and painful researches of which this singular mineral has been the object for more than forty years, chemistry has succeeded in producing from it eleven metallic substances, viz. platina, gold, silver, iron, copper, chrome and titanium, found by Messrs. Fourcroy and Vauquelin in the sands, more or less coloured, which are always mixed with platina.

The two new metals discovered by Mr. Wollaston were palladium and rhodium; and those discovered by Mr. Tennant were iridium and osmium *.

The metal discovered in platina some years ago by M. Vauquelin and called chrome, has been discovered in the meteoric stones by M. Laugier.

* Mr. Wollaston's and Mr. Tennant's papers have already been given in the Philosophical Magazine.

It has been since discovered by M. Thenard in the meteoric stones which fell near Alet, department of Gard, and which the Academy collected and sent to the Institute.

These stones, the fall of which has been established by testimony no less respectable than that of preceding ones, differ from them, however, in colour and consistence: they are blacker and more friable; but their analysis yielded M. Thenard nearly the same principles: the metals in them are only more oxidated, and there is a little more coal in them. This result has been confirmed by a committee of the Institute.

The last year announced the opinion of M. Pacchiani upon the composition of the muriatic acid, which he thought he had produced by depriving water of a part of its oxygen by means of the Galvanic pile.

This discovery would have been one of the most important that chemistry had ever produced; but it is not verified when care is taken to remove from the apparatus every thing which could furnish sea-salt. This has been established by Messrs. Biot and Thenard by direct experiments.

In a work upon refraction, at first undertaken for astronomical purposes, M. Biot was led to employ the action of bodies upon light in a very advantageous manner, for the analysis of transparent substances *. The substances produced by organized beings have never yet been sufficiently rigorously examined. Although we know generally of what elements they are composed, and that these primitive elements are not very numerous, their combinations are so varied, they change their nature so easily in the operations which they undergo; that we must study for a much longer time these combinations themselves as if they were simple, and abstraction made of their true elementary principles. These matters thus considered are what are called the *immediate principles* of organized bodies. This year has made

* An extract of Messrs. Biot's and Arrago's memoir upon this subject is given in our present volume.

known to our chemists several of these immediate principles.

Messrs. Vauquelin and Robiquet have found in the juice of asparagus a crystalline substance soluble in water, which nevertheless is neither an acid nor a neutral salt; and it is not affected by the ordinary re-agents*. They purpose following up the examination of its nature.

[To be continued.]

LVI. *Intelligence and Miscellaneous Articles.*

NEW COMET.

Letter of Dr. Olbers to the Editor of the Hamburgh Correspondent.

Bremen, Dec. 23, 1806.

The comet discovered by M. Pons at Marseilles on the 10th of November has not been visible to us these few days past, on account of its too great increase of southern declination. According to observations made here and at Lilienthal, M. Bessel, superintendant of the observatory of the celebrated senator of justice Schroter, at Lilienthal, has calculated the path of this comet. From the calculation it follows that the new comet, after appearing in superior brilliancy in the southern parts of the globe, and after passing very close to the south pole of the ecliptic on the 31st of December, will be again visible towards the middle of January, above the horizon of the observatories in the south of Europe, and about the 20th of the same month will be also visible in this neighbourhood. It will then be seen in the milky way, in the sign of the Whale, included by the new astronomers in the sign of the Electrical Machine. With us the comet will rise but a very little above the clearest part of the south and south-west horizons, and on that account we can only observe it if we are favoured with warmer weather; but in the south of Germany, France, Italy, &c. it may be very distinctly observed, and followed with the telescope, until very near the end of February.

* See the present volume, pages 33, 115, 239.

In order to facilitate the finding again of this comet, M. Bessel has calculated the following places of the same for the midnight of Paris:

	Degrees of Ascension.	South Declination.
January 15,	- 25° 14′ - - -	39° 18′
January 25,	- 19 40 - - -	29 34
February 4,	- 17 33 - - -	23 58

Until the 16th of February this comet will become clearer and more brilliant than it was on the 10th of November, the day of its first discovery.

W. Olbers, Dr.

LIST OF PATENTS FOR NEW INVENTIONS.

To William Henry Wyatt, of Hatton Garden, gentleman, in consequence of a communication made to him by a foreigner; for a new discovery of the means of facilitating the chemical action between copper and several saline substances, so as to produce important improvements in the art of separating gold and silver from copper plated or united with either of those metals, and in the manufacturing of sulphate of copper, and in the making of many kinds of colours for painting. January 15.

To Chester Gould, of Birmingham, in the county of Warwick; for his invented improvement or machine to ascertain the weight of any thing to the amount of ten tons and upwards, to be made use of instead of the common steel-yard, or beams and weight. January 24.

LECTURES.

On Monday, February 3, a Course of Lectures on Physic and Chemistry will recommence at the Laboratory in George-street, Hanover-square, at the usual morning hours, viz. the Medical at Eight, and the Chemical at Nine in the morning, by George Pearson, M.D. F.R.S., of the College of Physicians, senior Physician of St. George's Hospital, &c.

A Clinical Lecture is given every Saturday Morning at Nine o'clock, on the Patients in St. George's Hospital.

Proposals may be had in George-street, and at the Hospital.

METEOROLOGICAL TABLE,
BY MR. CAREY, OF THE STRAND,
For January 1807.

Days of the 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	44°	51°	47°	30·11	5	Cloudy
28	50	53	47	29·90	12	Fair
29	48	50	40	·50	0	Rain
30	47	54	45	·56	0	Rain
31	41	41	35	30·19	10	Cloudy
Jan. 1	34	39	30	·56	11	Fair
2	28	30	27	·55	7	Foggy
3	30	38	35	·26	5	Fair
4	41	41	35	·30	8	Fair
5	35	39	27	·50	10	Fair
6	26	35	32	·42	4	Fair
7	33	39	30	·33	3	Fair
8	31	38	35	·04	7	Fair
9	40	46	46	29·76	4	Cloudy
10	40	44	32	·98	8	Fair
11	33	38	36	30·24	6	Cloudy
12	37	41	40	·05	0	Cloudy
13	40	41	32	·75	10	Fair
14	28	35	30	30·00	6	Cloudy
15	21	31	44	30·08	11	Fair
16	46	51	48	29·88	14	Fair
17	47	51	42	·85	0	Rain
18	38	44	35	·65	10	Fair
19	32	42	45	·50	8	Fair
20	32	41	35	·06	5	Fair
21	35	42	37	28·85	0	Rain
22	39	38	35	29·18	0	Rain
23	36	42	35	·66	12	Fair
24	35	43	34	30·20	12	Fair
25	29	34	28	·42	10	Fair
26	27	39	40	·48	7	Fair

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

INDEX TO VOL. XXVI.

Acetic ether. On, 85, 86
Acid, acetic. On, 85, 86, 111
Acid fumigations. On, 71, 81
Acids. Action of, on fat, 77
Agricultural patent, 95
Agriculture. Smithfield Club, 271
Air-pump. Improved, 38
Alburnous vessels of trees. On, 120
Alcohol. Action of, on fat, 78
Alembics. On the form of, 8
Amsterdam. Agricultural Society of, 90
Analysis of ipecacuanha, 14; of coffee, 17; of asparagus, 33, 115, 289; of native cinnabar, 146; of seeds of lycopodium, 188; of turquoise, 220
Animal fat. On, 72, 105
Antifriction rollers. Patent, 191
Antiquaries. Society of, 181, 269, 364
Ants destroy weevils, 91
Arrago on affinities of bodies for light, 151, 292
Asparagus. On, 33, 115, 289, 373
Astronomy. A prize question, 89; history of, 237, 347; precession of equinoxes, 269; comet, 373
Austrian literature, 190

Balances, compensation. On, 133
Barrows. Opening of, 182
Beauvois on mushrooms, 272
Bees. A prize question, 90
Bell's patent, 287
Berlin. Academy of, 89
Bernoulli's time-keepers. On, 47
Biography, 353
Biot on affinities of bodies for light, 151, 292
Blind. Education of the, 92
Boats for canals. On, 219
Bohemian Royal Society, 188
Bonefos on fumigation, 71
Books. New, 93, 171, 176, 186, 190, 238, 264, 272, 329: patent for binding, 287
Boots. Varnish for, 3
Botany. New books on, 186
Bowman's patent, 191
Boydell on the word "Burgh," 182
Bramah's patent for printing, 95
Buchanan on canal boats, 219
Burgh. Meaning of the word, 182

Capam's patent, 95
Canal boats. On, 219
Capillary tubes. Notice on, 243
Card-making. Patent for, 191
Carey's meteorological tables, 96, 192, 288, 375
Carriages. Patent, 94
Caspian Sea. On the, 183
Ceylon. Mineralogy of, 169
Chabert the navigator. Death of, 357
Charcoal. Antiseptic power of, 92
Chronometers. Le Roy on, 40, 129, 193
Cinnabar, native. On, 146

Clegg's patent, 95
Cloth. To varnish, 3
Cock, autocratic. The, 314
Coffee. Uses, preparation, and analysis of, 17
Cooke's patent, 94
Comets, 237, 242, 283, 373
Cooking apparatus. Patent, 94
Couching. Two cases of, 364
Cuvier on living and fossil elephants, 158, 204, 302

Davy on chemical effects of electricity, 181, 266, 269
Day (*H.*) elected junior secretary of Royal Society, 364
De Carro on mineralogy of Ceylon, 169
Deaths, 94, 353
Descroizilles on distilling plants, 8
Desgenet on fumigation, 81
Deucalion's flood. On, 183
Deyeux on distilling plants, 10
Diabetes. On, 97
Dispensary. The Finsbury, 253
Distillation of waters from plants, 6
Drawing machines. Patents for, 286, 287
Dupuytren on diabetes, 97
Dutertre's pendulum. On, 44

Eckbard's patent, 287
Eclipse of sun, 246
Electrical fishes. A prize question, 89
Electricity. Prize questions, 89; Davy on, 181, 266, 269
Elephants, living and fossil. On, 158, 204, 302
Equinoxes. Precession of, 269
Erfurt Society, 188, 275

Farey on music, 171
Fat. Memoir on, 72, 105
Finsbury dispensary. Report of cases in, 253
Fire-arms. Patent for, 286
Flageolette. Patent for, 286
Fletcher's patent, 95
Force of percussion. On, 229
Fossil elephants. On, 158, 204, 302
French National Institute, 183, 272, 368
Fruits. To produce new species of, 268
Fumigations, acid. On, 71, 81

Gaiters. Varnish for, 3
Galvanic experiments, 181
Galvanism. Prize questions, 89
Gases. On refractive power of, 151, 292
Gehlen on acetic acid and ether, 85
Geography, 183, 247
Gold. Patent for separating, from copper, 374
Gould's patent, 374
Granaries. To free, from vermin, 91

Hail storms. To avert, 212
Hanged man restored to life, 257
Harrison's time-keepers. On, 45, 61
Hats or helmets. Varnish for, 3
Hautefeuille's improvement on watches, 50
Hawkes's theory of music, 171
Hernia. Cases of, 254
Henry on ipecacuanha, 13; on acetic ether, 86
Home on cataract, 364
Horses. A prize question, 188
Hospitals. On fumigating, 71, 81
Hurricanes. On, 347
Huygens's pendulum. On, 44
Hydraulic machine, 314

Ipecacuanha. On the ligneous part of, 13; analysis of, 14
Johns's patent, 287
Jones's patent, 94
Jouville on mineralogy of Ceylon, 169
Keith on Bonnycastle's Spherical Trigonometry, 329

Klaproth

Klaproth on native cinnabar, 146
Knight on alburnous vessels of trees, 120

Lagrange on turquoise, 220
Lalande's history of astronomy for 1805, 237, 347
Laugier on meteoric stones, 11
Learned Societies, 88, 179, 266, 365
Lectures, 283, 374
Le Roy's memoir on chronometers, 40, 129, 193
Lescbevin on averting hail storms, 212
Levelling. On, 243
Lichen islandicus very nutritious as food. 68
Light. Effects of, on fat, 72; affinities of bodies for, 151, 292
Linen. To varnish, 3
Linseed oil. Boiled, to prepare, 4
Literary intelligence, 93, 171, 176, 186, 190, 264, 272
Litharge. To detect, in wine, 362
Lloyd's patent, 191
Longitude. On chronometers for determining, 40, 129, 193; on degrees of, 240
Lunar planispheres. Russel's, 264
Luttick. Society at, 89
Lycopodium seeds. On, 188

Magnetism. On, 45, 349; prize questions, 89
Malconformation. Singular cases of, 275, 281
Mattbey's patent, 286
Meat, salt. On preserving, 92
Mechanical power. A patent, 94
Medicinal preparations. On, 72, 105
Medicine, pneumatic. On, 69, 257
Memory. Art of, 282
Mercurial ointments. On, 105
Meridian. On arc of, 239
Meteoric stones. New principle in, 11, 371
Meteorology, 96, 192, 212, 288, 375
Millstones, 183
Minium, native. On, 114
Mist. Effects of, on vision, 31
Montauban. Society at, 89
Mountains. A work on, 93
Muriatic acid. On composition of, 372
Muscular motion. On, 180
Mushrooms. On, 272
Music. Farey on, 171

Navigation. On chronometers for, 40, 129, 193
Naucbe's test for wines, 362
Newcastle Society, 270
New York. Latitude of, 247
Nicholson's patent, 191
Oil, boiled linseed. To prepare, 4
Oils. Patent for purifying, 286
Oil, poppy. Valuable substitute for olive oil, 331
Olbers on a comet, 373
Optics. Facts applicable to, 151, 292
Oxymuriatic acid fumigations. On, 71, 81

Park, Mungo. News of, 91
Passions, the. A prize question, 89
Patents, 94, 191, 286, 374
Peirson on muscular motion, 180
Percussion. Wollaston on, 229
Perspective. Patent instrument for drawing in, 286
Phosphorized fat. On, 75
Pinel on fumigation, 81
Plants. On distilling waters from, 6; on growth of, 120; on food of, 317
Platina. On, 371
Pneumatic medicine. 69, 257
Poppy. On cultivation of, 331
Porcelain. On, 68
Pratt's patent, 94
Printed goods. Patents respecting, 94, 95
Prize questions, 89, 188

Prosser's patent, 95
Proust on porcelain and on the Lichen islandicus, 68
Provisions. On preserving, 92
Publications. New, 93, 171, 176, 186, 190, 238, 264, 272, 329, 350

Ratie, de. The philosopher, death of, 353
Refraction, atmospheric. Instances of, 31
Refraction. On, 151, 292, 372
Robiquet on asparagus, 33, 115
Romme, professor. Death of, 356
Rosarios. On, 270
Royal Society, London, 179, 266, 365
Royal Society. The old French, 355
Royston's patent, 191
Rumford. New exper. by, 274
Ruptures. Patent truss for, 94
Russel's lunar planispheres, 264
Russian literature, 190

Salmon's patent, 94
Sampson's patent, 94
Sanford's patent, 95
Schmalcalder's patent, 287
Scott's patent, 286
Shoes. Varnish for, 3
Silver. Patent for separating, from copper, 374
Smoking chimneys. Patent for curing, 95
Smithfield club, 271
Smithson (James) on native minium, 114
Societies. Learned, 88, 179, 266, 365
Sonorous bodies. On, 50
Spathic iron ore. On, 370
Speer's patent, 286
Steering wheel. Patent, 95
Steevens's autocratic cock, 314
Stars. Distance of, 242
Steam. Patent for application of, 191
Steam-engine. Woolf's piston for, 316
Stills. On the form of, 8
Stones, meteoric. New principle in, 11
Storms. To dissipate, 212; remarks on, 347
Sugar extracted from mulberries, 187
Sulphur pomatum. On, 73
Sully's lever clock. On, 43
Sun. On spots of, 241; eclipse of, 246
Surgical cases, 253
Suspended animation restored. Singular case of, 257
Sutton's patent, 94
Sylvester's improved air-pump, 38

Taunton's dispensary report, 253
Thenard on diabetes, 97
Thornton on pneumatic medicine, 69, 257
Time. Methods of measuring, 42, 129, 193
Townshend on food of plants, 317
Track-boats. On, 219
Travellers, 90
Trees. On growth of, 120
Tromsdorff on acetic acid, 111
Tumuli opened, 182
Turin. Society of Agriculture, 187
Turquoise. Analysis of, 220
Types. Patent for casting, 95

Utrecht. Society at, 88

Vaccination, 189; voyage to propagate the blessings of, 276
Vaccine Institution, Broad-street, 365
Valentia, lord. Return of, 90
Varnish for hats, gaiters, leather, &c. 3
Vauquelin on asparagus, 289
Vazie's patent, 191

Vegetation. Experiments on, 317
Vermin. To destroy, 91
Vision. E. Walker on, 29; curious facts concerning, 364
Voyages, 90, 92, 351, 352

Waggons. Patent, 94
Walker on vision, 29
Watches. On, 40, 129
Water. On condensation of, 273
Weaving. Patent for, 286
Wedgwood's patent, 94
Weevils. To destroy, 91
Weighing machine. Patent, 374
Whalebone. Patent for using, 191
White's patent, 95
White swelling cured by vital air, 70
Williamson's patent, 286
Wines. To detect adulterations of, 362
Wollaston on percussion, 229; patent to, 286
Woolf's piston for steam-engines, 316
Writings. Patent for producing duplicates of, 94
Wyatt's patent for separating gold and silver from copper, &c. 374

END OF THE TWENTY-SIXTH VOLUME.

ERRATUM.—Page 4, line 11 from the bottom, for *oil of marmite* read *boiled oil*.

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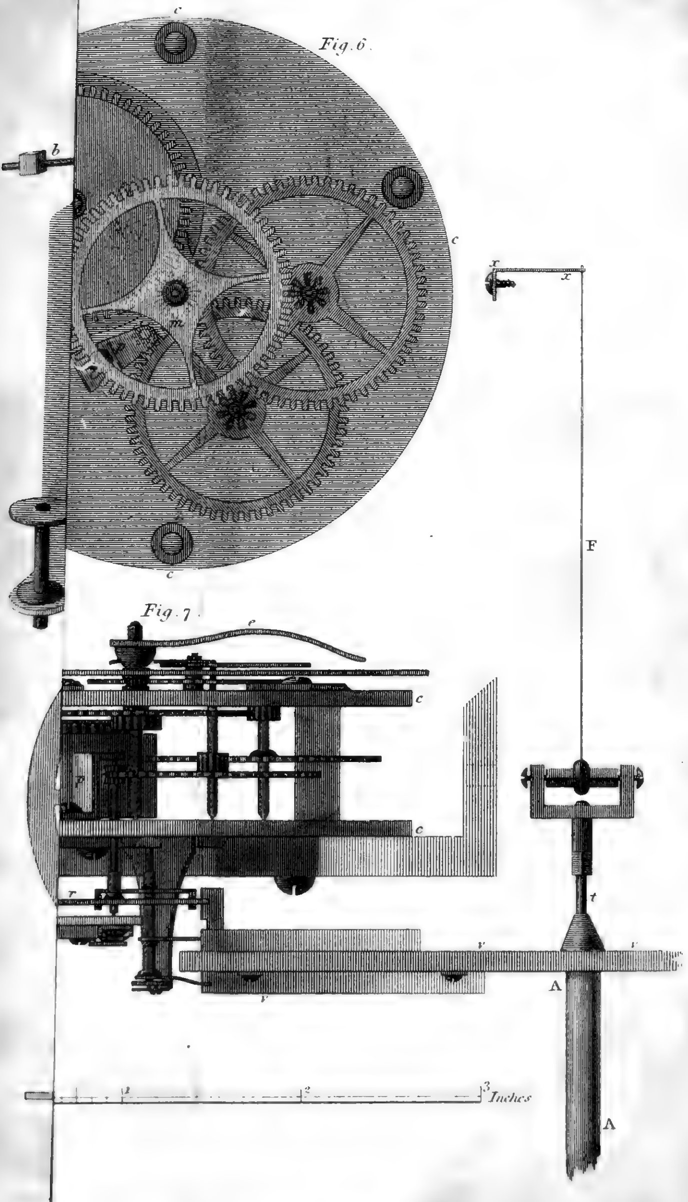
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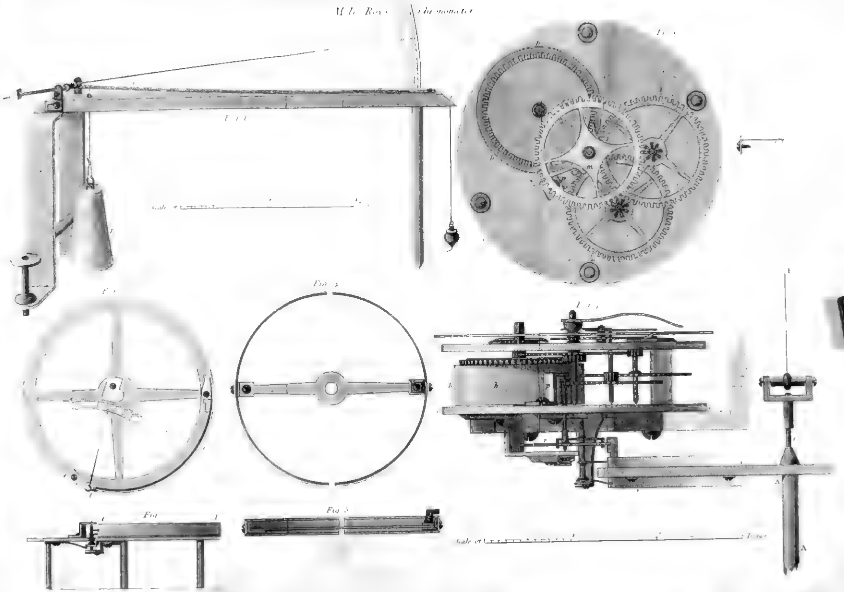
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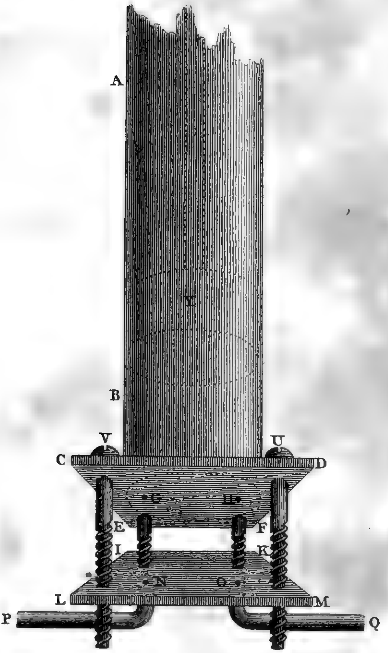

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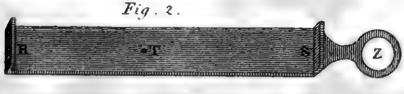

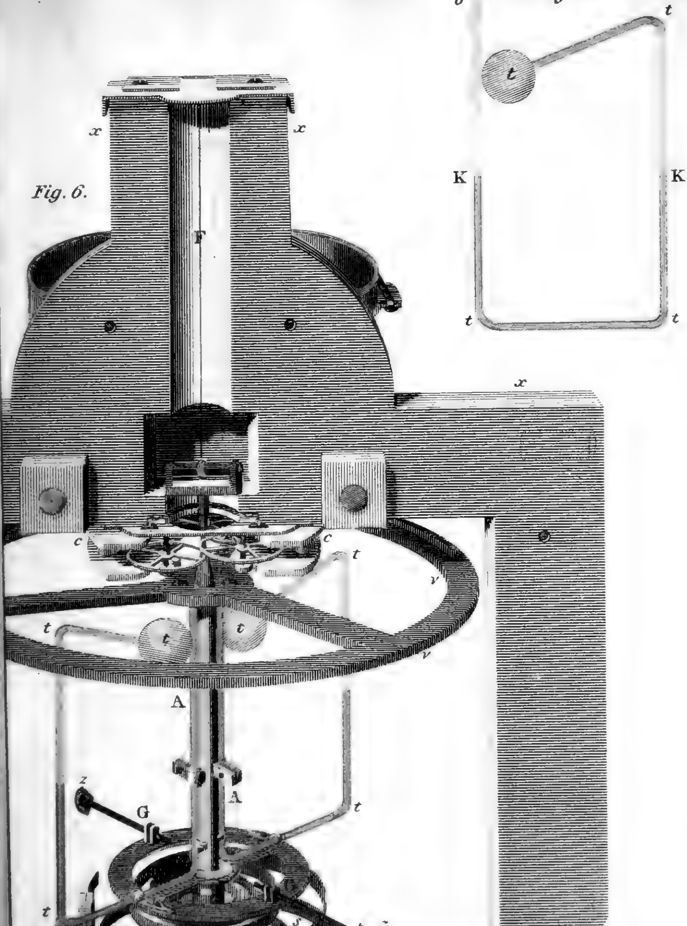

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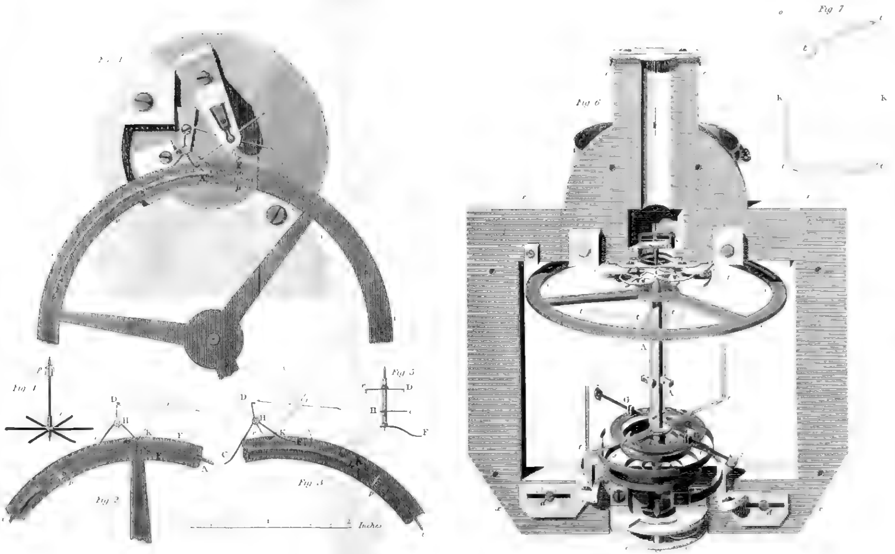
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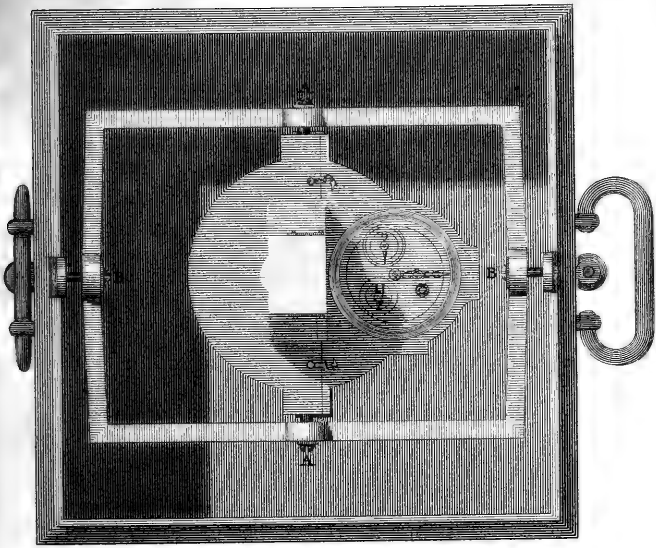

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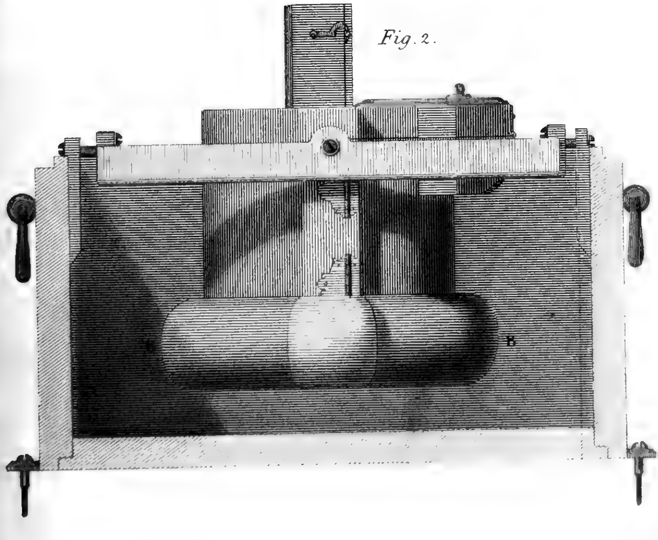

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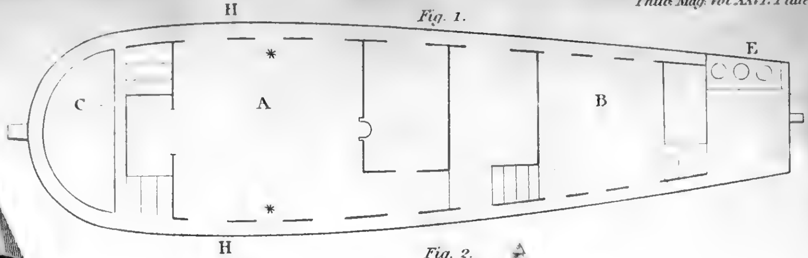

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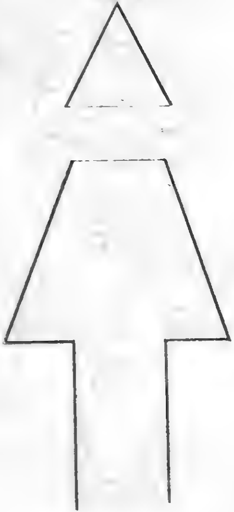

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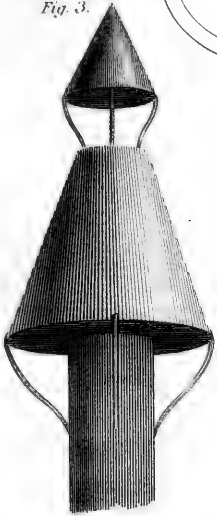

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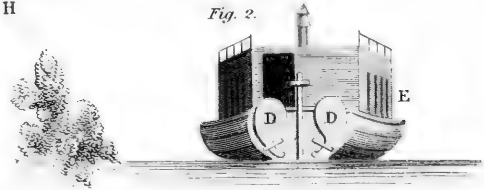

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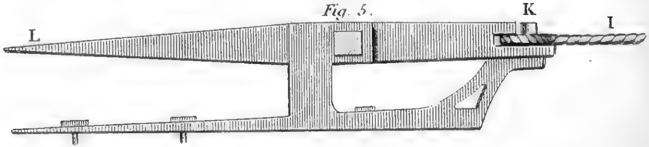

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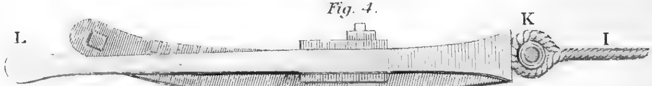

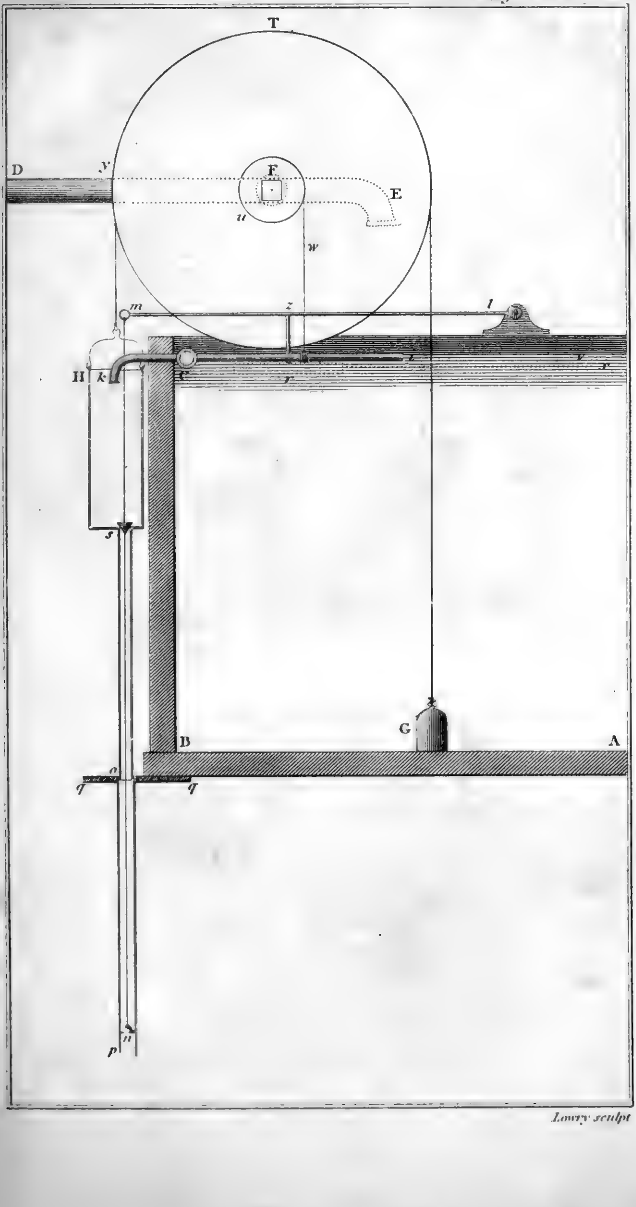

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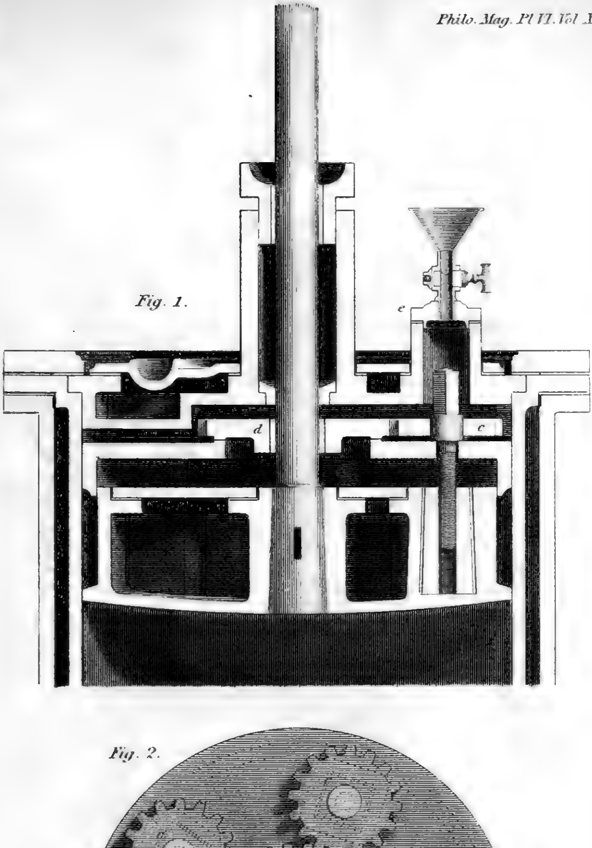

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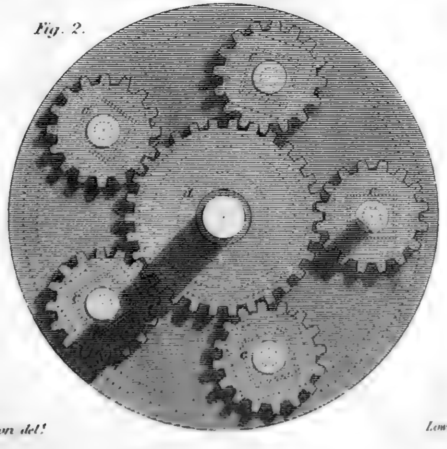

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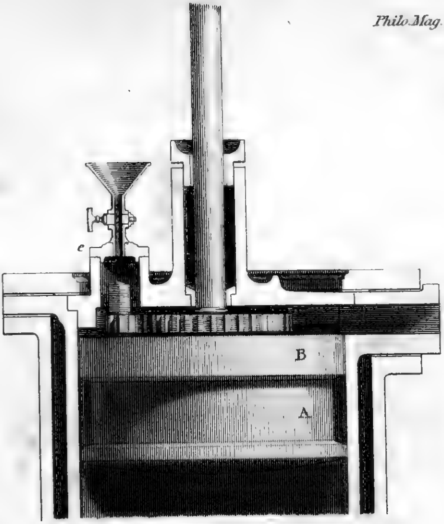

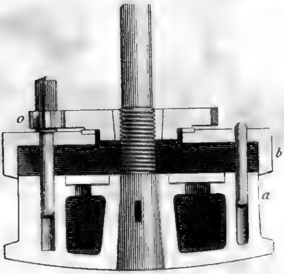

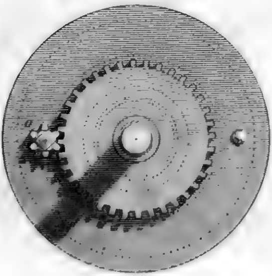

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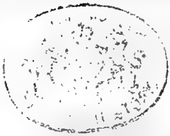

CONTENTS of NUMBER CIII.

Page

XXXIII. A Memoir on the best Method of measuring Time at Sea, which obtained the double Prize adjudged by the Royal Academy of Sciences; containing the Description of the Longitude Watch presented to His Majesty the 5th of August 1766. By M. Le Roy, Clock-maker to the King. Translated from the French by Mr. T. S. Evans, F. L. S., of the Royal Military Academy, Woolwich - - 193

XXXIV. Memoir upon living and fossil Elephants. By M. Cuvier - - - - 203

XXXV. Memoir upon a Process employed in the ci-devant Maçonnais of France, to avert Showers of Hail, and to dissipate Storms. By M. Leschevin, chief Commissary for Gunpowder and Saltpetre at Dijon - - - 212

XXXVI. On Canal Track-Boats. By Robertson Buchanan, Esq. Civil Engineer, Glasgow - 219

XXXVII. Analysis of the Substance known by the Name of Turquoise. By M. Bouillon Lagrange - 220

XXXVIII. The Bakerian Lecture on the Force of Percussion. By William Hyde Wollaston, M. D. Sec. R. S. 229

XXXIX. History of Astronomy for the Year 1805. By Jerome de Lalande - - - 237

XL. Report of Surgical Cases in the Finsbury Dispensary from the 1st of September to the 1st of November 1806; with Observations on two Cases of Hernia which proved fatal. Communicated by John Taunton, Esq. Surgeon to the City and Finsbury Dispensaries, and Lecturer on Anatomy, Surgery, &c. - - - - - 253

XLI. Thirty-third Communication from Dr. Thornton, relative to Pneumatic Medicine - - 257

XLII. Notices respecting New Publications - 264

XLIII. Proceedings of Learned Societies - 266

LXIV. Intelligence and Miscellaneous Articles:—Vaccination—Extraordinary Phænomenon—Art of Memory—New Comet—Lectures—List of Patents for New Inventions, &c. 276—288

*** Communications for this Work, addressed to the Editor, at No. 1, Carey-street, Lincoln's Inn, will meet with every attention.

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ENGRAVINGS.

Vol. XXIV. is embellished with a Plate of Figures to illustrate Mr. DALTON's Theory of the Absorption of Gases by Liquids—On the same;—Also a Refrigeratory for Distillation, acting on the Principle of the Syphon—The Effects produced by Mr. Ez. WALKER's newly invented Cometarium—A Quarto Plate illustrative of Experiments by Sir JAMES HALL, Bart., on the Effects of Compression in modifying the Action of Heat—Mr. STEEVENS's improved Gasometer, for Purposes where uniform Pressure is essential—A Plate illustrating Mr. WALKER's Paper on the apparent Magnitude of the horizontal Moon—A Quarto Plate illustrative of Sir JAMES HALL's Experiments on the Effects of Heat modified by Compression—A Portrait of Mr. AUBERT: engraved by Mr. E. Mackenzie—A Quarto Plate of Natural History, viz. the Fishes *Eremophilus Mutisii, Astroblepus Grixalvii*, and *Pimelodus Cyclopum*; and a South American Monkey, the *Simia leonina*: engraved by Lowry.

Vol. XXV. contains an Octavo Plate illustrative of Experiments by Sir JAMES HALL on the Effects of Compression in modifying the Action of Heat—A Quarto Plate on the same Subject—A Quarto Plate to illustrate Sir JAMES HALL's Paper on the Effects of Heat modified by Compression—Mr. SALMON's improved Geometrical Quadrant, Level, and Calculator, for ascertaining inaccessible Distances on Land or at Sea.—M. MONTGOLFIER's new Calorimeter, for determining the comparative Quantities of Caloric evolved by the Combustion of different Kinds of Fuel—Mr. GILBERT GILPIN's improved Crane with Flexible Chains—Mr. HERBERT's Book-case Bolt, and Mr. LE CAAN's Check for Carriage Wheels on Rail Roads—SALMON's Improvement on Canal Locks—Figures to illustrate M. GROTTHIUS's Memoir on the Decomposition of Water by Galvanism—A Bust in Silex, found coated over with Chalcedony.

Vol. XXVI. is illustrated with a Quarto Plate of M. LE ROY's Chronometer—Mr. SYLVESTER's improved Air-Pump—A large Plate relating to LE ROY's Chronometer—An Octavo Plate on the same Subject—The Irish Canal Track-Boat.

CONTENTS of NUMBER CIV.

Page

XLV. Discovery of a new Vegetable Principle in Asparagus (*Asparagus sativus* of Linnæus). By M. VAUQUELIN - 289

XLVI. Upon the Affinities of Bodies for Light; and particularly upon the refractive Powers of different Gases - 292

XLVII. Memoir upon living and fossil Elephants. By M. CUVIER - - - - 302

XLVIII. Description of an Autocratic Cock, useful in Breweries, Distilleries, &c. By JOSEPH STEEVENS, Esq. 314

XLIX. Description of Mr. ARTHUR WOOLF's improved Piston for Steam-Engines - - - 316

L. On the Food of Plants. By the Rev. JOSEPH TOWNSHEND, Rector of Pewsey, Wilts - - - 317

LI. Letter from THOMAS KEITH, Esq. Secretary to the Master of his Majesty's Household, &c., respecting Mr. BONNYCASTLE's Treatise on Plane and Spherical Trigonometry 329

LII. On the Cultivation of the Poppy. By T. COGAN, M. D. - - - - - - 331

LIII. History of Astronomy for the Year 1805. By JEROME DE LALANDE - - 347

LIV. Method of ascertaining whether Wines are adulterated with Litharge. By M. NAUCHE, Physician - 362

LV. Proceedings of Learned Societies - - 363

LVI. Intelligence and Miscellaneous Articles:—New Comet—List of Patents for New Inventions—Lectures - 373—375

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