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THE PHILOSOPHICAL MAGAZINE:

COMPREHENDING

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

NUMBER CLIX.

For JULY 1811.

CONTAINING THE FOLLOWING ENGRAVINGS BY PORTER:

1. Mr. Donkin's Tachometer.
2. Mr. Allan's Mathematical Dividing Engine.
3. A 4to Plate (intended to have been given in the preceding volume), containing figures illustrative of Mr. Loeschman's Patent Piano-Forte, and Mr Liston's Patent Enharmonic Organ.

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

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

Vol. XXIX. is embellished with a Portrait of the late Dr. DARWIN; from an original Picture in the Possession of Dr. THORNTON—A New Micrometer, invented by Mr. BREWSTER—A Representation of the Comet now visible.—Mr. FIELD's Plan for Building Towns and Villages composed of Circular Buildings—Mr. PEPYS's New Eudiometer—A Quarto Plate of the Apparatus employed by Messrs. ALLEN and PEPYS in their Experiments on Carbonic Acid—A fourth Plate to illustrate M. CUVIER's Paper on Elephants—Lieut. Col. CRICHTON's Bed for the easy Conveyance of Sick and Wounded Soldiers: engraved by PORTER—And Capt. PASLEY's Improved Telegraph: engraved by PORTER.

Vol. XXX. Sir H. C. ENGLEFIELD's Mountain Barometer, engraved by LOWRY—A Plate to illustrate M. CUVIER's Paper on Elephants—Another Plate on the same Subject.—1 and 2. Two Plates to illustrate M. CUVIER's Paper on Elephants, engraved by PORTER.—3. Figures to illustrate Mr. HERSCHEL's Paper on Coloured Rings.—A Skeleton of the American Mammoth, engraved by PORTER.—A Portrait of M. CARNOT—The Oil-mill of Bangalore.

Vol. XXXI. Mr. DAVY's new Eudiometer.—Geological Sections of Strata, in Matlock, Derbyshire, by Mr. WHITEHURST and Mr. FAREY.—Illustration of the Chinese Method of propagating Fruit Trees by Abscission—And Mr. BROAD's Gauge for measuring Timber.—Illustration of Mrs. D'OYLEY's Method of breeding Poultry; Mr. DREW's Balance level for laying-out Land for Irrigation; and an Experiment in Optics.—M. E. TURRELL's Construction of Chemical Muffles.—Mr. GILPIN's Machine for raising Coals and Ores.—WILSON's Secure Boat, or Life Boat and BOSWELL's improved Capstan: engraved by PORTER.—PEPYS's Apparatus for Decomposing the Alkalis under Naphtha: engraved by LOWRY.—ATKINS's improved Hydrometer for weighing Solids and Fluids.

Vol. XXXII. Mr. CLEALL's Machine for thrashing Hemp; and Mr. BOND's Machine for breaking Hemp.—Mr. WARD's Compensation Pendulum.—Mr. GROOMBRIDGE's Diagram of the Motion of the Planet Vesta.—A Portrait of Sir H. C. ENGLEFIELD.—Mr. CHARLES LE CAN's Tram-Plate—and Mr. COLLIER's Ship Stove.—Apparatus employed by Messrs. ALLEN and PEPYS in their Experiments on Respiration.—Mr. HENRY's Apparatus for Decomposing Compound Inflammable Gases.—Apparatus employed in the Royal Institution for the Decomposition of Potash by Iron.—BELL's Method of saving Shipwrecked Mariners.

Vol. XXXIII. Mr. JAMES ELMES's Portable Bridge.—Mr. KNIGHT's new Method of training Fruit Trees.—Mr. HERSCHEL's Figures of the Comet of 1807.—Two Plates to illustrate Dr. WILLIAM RICHARDSON's Paper on the Basaltic Surface of the Counties of Derry and Antrim: viz. A View of PORTMOON.—A View of PLESKIN, on the N. W. Side of Bengore Promontory.—Mr. CLEGG's Apparatus for making Carbonated Hydrogen Gas from Pit-Coal.—Mr. RICHARDSON's Machine for raising large Stones out of the Earth;—and Mr. GOUGH's new Hygrometer.—PILTON's Light Fence for Inclosures, which becomes invisible at a short Distance.—Proposed Improvements in Telescopes, by M. BURCKHARDT and by Dr. BREWSTER; and M. BOULLAY's Apparatus for Phosphoric Ether.—Major LE HARDY's Telegraph.—Capt. BOLTON's improved Jury Mast; and Capt. BAIL's Method of Fishing Anchors.—Plan and Section of the Thames Archway.—Plan of Stonyhurst Scientific Establishment.—Mr. TAD's Method of causing a Door to open over a Carpet;—and Mr. BARLOW's Wrench for Screw Nuts of any Size.

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NUMBER CLX.

For AUGUST 1811.

CONTAINING THE FOLLOWING ENGRAVINGS BY PORTER:

1. A Plate to illustrate M. Haüy's Paper on the Electricity of Minerals.
2. Mr. Walker's improved Micrometer.
3. Mr. Moult's Filtering Apparatus.
4. Mr. Smith's Method of relieving a Horse fallen in the Shafts of a loaded Cart.
5. Mr. Taylor's Air-Exhauster for Mines.

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VOL. 38. SEPTEMBER 1811. No. 161.

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NUMBER CLXI.

For SEPTEMBER 1811.

CONTAINING THE FOLLOWING ENGRAVINGS BY PORTER:

1. A Representation of the Comet now visible in Ursa Major.
2. Sir HOWARD DOUGLAS's Patent Reflecting Semicircle.

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

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GEOLOGY, AGRICULTURE,
MANUFACTURES AND COMMERCE.

NUMBER CLXII.

For OCTOBER 1811.

CONTAINING THE FOLLOWING ENGRAVINGS:

1. A coloured Map to illustrate Dr. Campbell's Paper on the inferior Strata of the Earth occurring in Lancashire.
2. Mr. Sadler's Apparatus for Smelting of Lead.

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

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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. XXXVIII.

For JULY, AUGUST, SEPTEMBER, OCTOBER, NOVEMBER, and DECEMBER, 1811.

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PHILOSOPHICAL MAGAZINE.

I. *Extract from a Memoir on Phosphorescence, presented to the Institute, by M.* Dessaignes*, *on April* 5, 1809.

The above memoir is divided into five chapters. In the first, the author treats of phosphorescence in general, and of its various modes. He defines phosphorescence to be "an appearance of light, durable or fugitive, not sensibly containing any heat, and without any subsequent alteration in inorganic bodies." He distinguishes four kinds of phosphorescence, viz. Phosphorescence by the temperature being raised, that which bodies exhibit when exposed to the light or to electrical discharges, that which arises from percussion, from pressure, or from friction, and last of all spontaneous phosphorescence.

The author recognised the first of these various modes of phosphorescence in almost all bodies presented to us by nature, with but very few exceptions. For this purpose he placed them on a metallic stalk in a dull heat, in order that the lustre of a red hot support might not hinder him from observing the phosphoric light in bodies in which it shines but feebly. He made his experiments on bodies which light renders phosphorescent in a dark room, furnished at the side exposed to the sun with a small trapdoor which opened and shut at pleasure. Exposure to the rays of the sun for a few seconds is sufficient for exhibiting the shining of these bodies, the instant we intercept them, with all the brilliancy of which they are susceptible. The light which they give out in this case, is, in general, in an inverse ratio to their degree of humidity: but they do not lose entirely this kind of phosphorescence, except when they are mixed with a quantity of water sufficient for holding them in solution, or when they themselves pass to the

* *Journal des Mines*, tom. xxvii. p. 213.

liquid state. The third kind of phosphorescence, which may be, generally speaking, called *phosphorescence by collision*, supposes a texture of a gravelly rather than of a glassy nature, and we do not obtain it at its highest degree, except by the contact of a body of one and the same species, or of a still harder body. It also supposes the hardness of the constituent molecules, but it is not necessary that they should be strongly united together: thus, sugar, in which the molecules feebly adhere, is, as we know, very phosphorescent upon collision.

Spontaneous phosphorescence is presented during certain combinations wherein the molecular action is energetic, like that which combines lime and water, the phosphorescence lasts but a few seconds, and the slower the combination takes place, it is the more permanent: this happened in the case of all the woods which the author submitted to experiment, whether they were growing or cut down. They become equally shining, in both cases when they are penetrated with humidity, in contact with the atmospheric air, and at a temperature of 8 or 10° of the centigrade thermometer.

We shall now give an idea of the principal facts ascertained by the author, relative to these four kinds of phosphorescence. So far from the phosphoric light being regarded as a consequence of the incandescence of the supporter, it disappears completely in the case of various bodies, which shine very well on a support heated merely to 200°. It results from the experiments of M. Dessaignes, that bodies which are phosphorescent at an elevation of temperature are equally so on metallic supporters, on those which are bad conductors of caloric, like glass or porcelain, and in boiling mercury or even in water, when substances are acted upon which require a temperature of 100° only to shine. The fluate of lime, the phosphate of lime of Estremadura, and the adular stone in powder, shine at 100° or 112° of the centigrade thermometer: glass, sand, porcelain, and in general all the vitreous stones, do not shine completely except at 375°. All bodies which are phosphorescent at an elevated temperature require middle degrees of heat between these two extremes: the whole shine more or less at 256°, a temperature determined by the fusion of bismuth.

It results from many experiments that the light which escapes is in a direct ratio with the degree of temperature, and its duration in an inverse ratio. A substance which, like fluate of lime, shines well at a low temperature, ceases

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to possess this property if it has previously undergone a stronger heat: but it preserves that of becoming luminous on a body still hotter. The same substance subjected several times successively to a temperature of 300° gave a fine light which lasted at the first projection 30″, at the second 15″, and at the third 10″. Fifteen other successive projections gave a light of the same duration and intensity as the fourth. Vitreous bodies lose their phosphoric properties with great difficulty: for this purpose they must be calcined strongly for half an hour, or even an hour, while all the metallic bodies, their phosphorescent oxides, and all the metallic salts, lose it on the first projection in an iron spoon slightly heated. Lime, barytes, strontian, magnesia, alumine, and silex, cannot lose their phosphoric property, whatever may be the degree of heat to which they are subjected. These earths heated at first to 100° or 125° do not omit any light on a support heated to 250°, whereas they shine well if thrown upon it cold. The carbonates of lime, barytes, and strontian, lose their phosphorescence upon a moderate calcination, and resume it afterwards if we calcine them to whiteness for half an hour, which seems to arise from their then passing partly to the state of caustic alkaline earths. All the earthy or alkaline salts lose their phosphorescence upon calcination: those which are soluble resume it in proportion to their solubility, when they remain exposed to the air, particularly if humid. The insoluble salts, in the same way as quartz, adular, glass, &c. lose it entirely. The vegetable and animal substances lose it in the same way, but only when reduced to the state of charcoal.

The author, after having explained these facts, passes to some general considerations. He has ascertained that the most phosphorescent bodies are those, in the composition of which some of their elements have passed from the gaseous or liquid to the solid state. He remarks that the light of phosphorescence is, like every other, decomposable by the help of a prism into rays of various colours. It is of itself coloured generally in blue in all the phosphorescent bodies which do not contain metallic oxides, and becomes so in bodies which contain them,—like the powder of calcined bones, the light of which is yellow, the phosphate of lime of Estremadura, and the green fluate of lime, which present a green light, when we free them from the oxides which they contain, by dissolving them in the muriatic acid and precipitating them with ammonia.

The phosphorescence of mineral substances undergoes no

 variation

variation on inserting them into the various gases; which proves that it is not the result of a combustion. That of vegetable and animal matters becomes brisker, on the contrary, in oxygen, and disappears in azote, hydrogen, and carbonic acid: nevertheless linseed oil obtained by pressure, and heated in a vacuum, or carbonic acid at 125°, becomes sensibly luminous; but when this light has disappeared it remains constantly obscure while it is in the same circumstances. When heated in the open air nearly to ebullition, it burns with a shining light, of a quite different nature, since it is extinguished in the carbonic acid, and is revived in the atmospheric air. The first of these phosphorescences is analogous to that of mineral substances, the second is a true combustion. All the metallic oxides made by calcination are inphosphorescent, at least if they have not passed to the state of semi-vitrification. Those which we obtain by the humid way lose their phosphorescence by drying them on a filter above burning coals. The phosphate of mercury resists a little longer; but when the filtering paper begins to turn red, there arises a mass of light, after which it remains dark like the rest. All bodies which have too great a quantity of water in their composition are inphosphorescent. Slaked lime is very luminous in dry, and very dull in damp weather. The concrete carbonate of lime only gives a few sparks, that of primitive formation is very phosphorescent at an elevated temperature. All substances which are melted or softened on the heated supporter, remain obscure. Many mixed substances which do not soften at 256°, and which shine very well at this degree of heat, become obscure as soon as the temperature is high enough for the particles to be disunited. The facility with which the acidulated salts, with the exception of the borates, enter into fusion, renders them inphosphorescent. The acid tartrite of potash shines, however, but only by the combustion of its vegetable principles. The volatile salts manifest the same inphosphorescence. Lastly, those which contain a great quantity of metallic oxides not dissolved, but mixed with their substance, do not exhibit any phosphorescence. In general, the substances which attract humidity very strongly, do not shine except when they are united to a less quantity of water than that which could saturate them, or, as is commonly said, slake them completely; but it is not requisite that they should be totally deprived of it. The author particularly observed the influence of this circumstance on the phosphorescence produced by the alkaline earths in contact with concentrated

sulphuric

sulphuric acid. Small pieces of caustic barytes as dry as it was possible to procure them, emit no light when thrown into this acid, and are not at first dissolved in it. When moistened by the breath simply, they give out light at the moment of touching the acid, and are converted into sulphate of barytes. When too much moistened they again become dull. Lime and strontian exhibit the same phænomena. According to this remark as to the quantity of water necessary to phosphorescence, the author made sulphates and muriates of soda and of potash and fluate of lime, deprived of all phosphorescence, because they contained no water, by using alcoholic solutions, instead of aqueous solutions, in the ordinary operations of precipitations or double decompositions which give birth to these salts. It is to be remarked, that the salts which have lost by calcination their phosphorescence, with the water which they contained, take up part of it again after having undergone igneous fusion. It is the same with glass and metallic scales become dull by calcination, which shine once more on the warm supporter after having been fused.

The examination which the author afterwards enters into of the relations between electricity and phosphorescence by elevation of temperature, presents facts of great interest. Among the metallic powders, those of zinc and antimony are the most phosphorescent, and those of gold and silver are least so. All of them, when prepared in damp weather with all the precautions most proper for making them shine, are inphosphorescent, as well as the metallic oxides. In dry weather the contrary is the case. Antimony even in very dry weather loses its phosphorescence, if it be pounded and strongly rubbed against a metallic mortar. In an insulated vessel, it acquires a high degree of phosphorescent properties. Glass pounded in dry weather is more luminous than when the operation has been performed in a damp atmosphere. It loses almost all its phosphorescence when pounded in wet linen; but it does not lose it, like antimony, when the operation is performed in a metallic mortar, because there is no transmission of the electrical fluid. It is not the case with adular spar, the powder of which does not retain a fine phosphorescence unless it has been prepared in an insulating mortar. In all cases where a vessel of this nature has been employed, the author took care to use one with an insulating pestle. In the last place, glass rendered dull by calcination resumes all its phosphorescence when exposed on an insulating supporter between the two balls of an electrical excitator, after it has received four or five electrical discharges. This method equally succeeds with every substance which has lost its phosphorescence by calcination. There is not even a necessity for a discharge, and we restore phosphorescency to calcined adular, for example, by a simple current of electricity determined by a conductor terminated in a point, and which is passed through this substance reduced to a liquid paste, with a quantity of water sufficient for leaving an easy passage to the electric fluid. After three or four minutes of electrization, this paste, afterwards dried spontaneously, yielded a very phosphorescent powder on the hot supporter. What is most remarkable in this way of restoring phosphorescence to bodies which have lost it by calcination, is, that it never reestablishes it in those which have been deprived of it by exposure to the light of the sun.

The third chapter of M. Dessaignes' memoir contains his inquiries as to the phosphorescence produced by light or by electricity. He first refutes the opinion of a great number of persons, as to the cause of the lustre which is preserved during a longer or shorter period by bodies exposed to the light of the sun. The sulphuret of lime, known by the name of *Canton phosphorus*, the dried nitrate of lime, glucine and Bologna phosphorus, after having been hit by red rays only, shone with precisely the same yellow, white, green, and reddish colours which they afforded when exposed to the ordinary light of the sun. The same thing took place after their exposure to the light of each ray of the solar spectrum, or to that of coloured flames; and what confirms the idea that these phænomena are not owing, as has been supposed, to the imbibition of light by the phosphorescent substance, is, that the Canton phosphorus prepared without having experienced in any shape the contact of light, shines equally upon a heated supporter, or even by the simple heat of the hand. This phosphorus shines, on exposure to the light of the moon or to that of a lamp. The light of the moon is not sufficient for Bologna phosphorus, which takes fire, however, on being exposed to the light of a lamp with a stream of air. Most of the other phosphorescent substances require the light of the sun. It is sufficient, even after having been reflected, for several salts: but cat's eye, hyoline quartz, and phosphate of Estremadura lime do not shine until after having been exposed to the direct light. Other substances, such as zircon, ruby, cymophane, and other vitreous stones, absolutely resist the action of the light even when it is concentrated in the focus of a burning mirror.

The

The general result of the experiments made upon a very great number of bodies exposed to the light of the sun, leads the author to determine that those which are demi-conductors of the electric fluid are all susceptible of taking fire by these means; that isolating bodies do not shine equally, some of them doing so feebly or with difficulty, and the rest not at all; lastly, that bodies which are conductors remain dull: and this happens to the metals, to charcoal, to carburet of iron, to all the sulphurets and metallic oxides, with the exception of orpiment, the semi-vitreous oxides of arsenic and tin, and those of zinc and lead made in the humid way. Among the metallic salts, the author has only found that the muriate of tin, the sulphate and the phosphate of lead, shine after being exposed to the sun.

A remarkable difference between the inphosphorescence which insulating bodies and good conductors present, either in the light or when subjected to feeble electrical discharges, while all the demi-conductors shine by these two methods, consists in the faculty which the former have of becoming luminous after very strong discharges; the latter remain constantly dull, however strong the explosions may be.

When bodies have been calcined until they have become inphosphorescent, either upon a heated supporter, by exposure to the light, or by a first electrical shock, the two first methods cannot restore their phosphorescence; but they resume it by reiterated discharges, and then they again become equally sensible as formerly to heat, light, and a single electrical discharge.

The author afterward relates various experiments, which prove that these three modes of phosphorescence increase or diminish by the same circumstances. Thus, the sulphate of soda, subjected to the action of light and to that of electricity in four different states, crystallized, deprived of half its water of crystallization, of three-fourths of it, and entirely calcined, burnt for the same length of time, whether the phosphorescence was excited by the shock of the light, or by that of electricity: viz. in the first instance during 6″, in the second 8″, in the third 200″, in the last 4″ only, and with a very feeble light. Sulphate of potash rendered inphosphorescent by calcination, thus deprived of its water of crystallization, and immediately inclosed in a tube furnished with excitators, resumed after five or six electrical discharges the property of shining by isolation, as it did before being calcined: hence we may conclude, that the complete desiccation of phosphorescent substances does not deprive

deprive them of this property, except by rendering them less susceptible of permitting the passage of the electric fluid, and not because a small quantity of water is indispensable for the production of this phænomenon.

The fourth chapter of M. Dessaignes' Memoir treats of phosphorescence by collision. All bodies susceptible of shining in this manner are also, with very few exceptions, luminous on the heated supporter, by exposure to light and by electrization. This property also diminishes in proportion as bodies are more or less completely calcined: nevertheless glass, calcined until it has become inphosphorescent by every other method, also emits a brisk light under the action of the file; but in order to produce this effect the friction must be much stronger than before calcination. This kind of phosphorescence, which takes place like the foregoing in vacuo, and in the irrespirable gases, seems besides, from the whole phænomena, to be produced by the same cause. The author attributes it to the oscillations of a particular fluid which heat, light, electricity, and a blow or friction put in motion; and that calcination, or a long exposure to the light, drives it from bodies which are exposed to it: but we do not find, on this hypothesis, how strong electrical discharges reproduce the phosphorescence, unless this fluid and the electrical fluid are one and the same. The author thinks this opinion ought to be rejected, because we perceive no sign of electrical attraction, or repulsion, in bodies which have recovered in this way the phosphorescence which they had lost, and because it is excited by an electrical discharge in various bodies plunged under water. But if we recollect that water is a very bad conductor of the electrical fluid, and that we are but imperfectly acquainted with the various modifications of which this fluid is susceptible, and the cause of the brilliant light which it gives in vacuo, we may naturally look for new facts, before we decide that the fluid of phosphorescence is essentially different from that to which we ascribe the phænomena of electricity.

M. Dessaignes examines, in the fifth chapter of his memoir, the spontaneous phosphorescence of animal and vegetable substances. He concludes from his experiments, that it is owing to a true combustion, in which water and carbonic acid are formed: we easily ascertain the presence of this acid in the residuum by means of lime-water. Wood loses more than half its weight before ceasing to shine. This phosphorescence is not extinguished until after some time in the irrespirable gases, but this is on account of the air

air contained in the pores of the phosphorescent substance, and the presence of which is easily ascertained by putting this substance in water under the receiver of the pneumatic machine. In proportion as the air escapes, the phosphorescence diminishes, and soon disappears entirely. The author ascertained that this air, already vitiated, only contains about three-fifths of the oxygen of an equal volume of atmospheric air.

This kind of phosphorescence is destroyed without remedy, by immersion in boiling water: it is suspended in water at 50°, and at the freezing temperature: it subsists from 6° to 37°.

The author having put a piece of phosphorescent fish in a saline solution favourable to phosphorescence, but which had been previously deprived of air by ebullition, found it completely dull after leaving it for two hours. Upon introducing a bubble of air into the phial, phosphorescence for a few minutes was restored; new bubbles of air produced the same effects; but afterwards the phosphorescence became constant as in the atmospheric air, apparently because the water took up that of which it had been deprived by ebullition.

ADDITION TO THE ABOVE MEMOIR.

On the Power of Points upon the Fluid of Phosphorescence. —M. Dessaignes has made several additions to the above experiments. The most remarkable has for its object the new analogy which he has established, between the electrical fluid and that of phosphorescence, by ascertaining the influence of points on the phænomena presented by phosphorescent bodies. Fluor spar fractured, and presenting angles or asperities at its surface, easily takes fire on a supporter slightly heated; but an entire crystal of the same substance, the surfaces of which have the natural polish, remains dull. This is the case also with limpid Iceland spar, Madagascar crystal, limpid adular, vitreous phosphate of lime, emerald, and sal gemma. A piece of glass five millimetres thick remains dull even on a red-hot supporter, and becomes very luminous when it has been made rough on both sides: if it has been made rough on one side only, it shines only when it is upon this surface that it rests on the supporter. The phosphate of lime in acute-angled masses of the first formation, presents the same phænomenon.

Crystallized calcareous spar in prisms with six panes, terminated by three pentagonal faces; is formed of inclined

laminæ

laminæ of about 45° to the axis of the prism, and the edges form the surfaces of it by their superposition: this crystal, when lying on the heated supporter on one of these faces, shines throughout its whole substance, whatever its thickness may be: if we make a section in it parallel to the laminæ, and place this section on the supporter, the crystal remains inphosphorescent.

Arragonite takes fire in the same way very well when a crystal of this substance rests on the supporter by one of the faces of the prism, and remains constantly obscure when it is the base which is exposed to the action of the caloric.

The author tried three small diamonds crystallized in octahedrons, and formed, as is well known, of laminæ parallel to the faces of this solid: they remained without lights; but upon fracturing one to produce some asperities, it became equally phosphorescent with a cut diamond which served the author for a term of comparison. Among other diamonds cut in the same manner, some easily emitted light; and others remained obscure. Two of them being slightly luminous, the author ascertained with a microscope that the laminæ of the one were perpendicular, and those of the other almost parallel to the faces. The first was luminous on the warm supporter, and the second remained obscure.

M. Dessaignes also examined the influence of points and asperities on phosphorescence by insulation. The limpid rhomboidal crystal of Iceland, when exposed to the light, acquires very little phosphorescence from it, while its faces have their natural polish: it becomes luminous when one of its faces is rubbed, and when this face is presented to the light.

Prismatic and limpid arragonite in entire crystals presents but a very feeble light, which disappears almost instantly: but when it is broken, it becomes very phosphorescent on the faces of its fractures in whatever direction they are made. The apathite of Werner and the chrysolite of the jewellers present similar phænomena, but less marked. Acid phosphate of lime, which the author had crystallized in a mass by slow cooling, was easily electrified by friction, but did not shine after having been exposed to the light: on fracturing it in order to destroy the polish of its surface, it became very phosphorescent, but was no longer susceptible of electricity as in the first case: so that the same asperities which communicated to it the property of shining after having been exposed to the light, rendered it to a certain

tain extent a conductor of the electrical fluid. The author multiplied and varied his experiments upon diamonds: all of them tended to prove that the faces parallel to the laminæ of which their substance is composed are electrified more easily and more strongly, but do not produce any phosphorescence when they are exposed to the light, even to that of the direct rays; whereas the faces, either natural or artificial, formed by the united edges of these laminæ, are feebly electrified by friction, lose their electricity much sooner, and are at the same time very phosphorescent. The importance and novelty of these various results have induced us to explain them here as fully as possible; at the same time the work itself will be perused with much gratification.

II. *On a Combination of Oxymuriatic Gas and Oxygen Gas. By* Humphry Davy, *Esq. LL.D. Sec. R.S. Prof. Chem. R.I.**

I shall beg permission to lay before the Society the account of some experiments on a compound of oxymuriatic gas and oxygen gas, which, I trust, will be found to illustrate an interesting branch of chemical inquiry, and which offer some extraordinary and novel results.

I was led to make these experiments in consequence of the difference between the properties of oxymuriatic gas prepared in different modes; it would occupy a great length of time to state the whole progress of this investigation. It will, I conceive, be more interesting that I should immediately refer to the facts; most of which have been witnessed by members of this body, belonging to the Committee of Chemistry of the Royal Institution.

The oxymuriatic gas prepared from manganese, either by mixing it with a muriate and acting upon it by sulphuric acid, or by mixing it with muriatic acid, is when the oxide of manganese is pure, and, whether collected over water or mercury, uniform in its properties; its colour is a pale yellowish green; water takes up about twice its volume, and scarcely gains any colour; the metals burn in it readily; it combines with hydrogen without any deposition of moisture: it does not act on nitrous gas or muriatic acid, or carbonic oxide, or sulphureous gases, when they have been carefully dried. It is the substance which I employed in

* From Philosophical Transactions for 1811, Part I.

all

all the experiments on the combinations of oxymuriatic gas, described in my last two papers.

The gas produced by the action of muriatic acid on the salts which have been called hyper-oxymuriates, on the contrary, differs very much in its properties, according as the manner in which it is prepared and collected is different.

When much acid is employed to a small quantity of salt, and the gas is collected over water, the water becomes tinged of a lemon colour; but the gas collected is the same as that procured from manganese.

When the gas is collected over mercury, and is procured from a weak acid, and from a great excess of salt, by a low heat, its colour is a dense tint of brilliant yellow green, and it possesses properties entirely different from the gas collected over water.

It sometimes explodes during the time of its transfer from one vessel to another, producing heat and light, with an expansion of volume; and it may be always made to explode by a very gentle heat, often by that of the hand*.

It is a compound of oxymuriatic gas and oxygen, mixed with some oxymuriatic gas. This is proved by the results of its spontaneous explosion. It gives off, in this process, from one-sixth to two-fifths its volume of oxygen, loses its vivid colour, and becomes common oxymuriatic gas.

I attempted to obtain the explosive gas in a pure form, by applying heat to a solution of it in water; but in this case there was a partial decomposition; and some oxygen was disengaged, and some oxymuriatic gas formed. Finding that in the cases when it was most pure, it scarcely acted upon mercury, I attempted to separate the oxymuriatic gas with which it is mixed, by agitation in a tube with this metal; corrosive sublimate formed, and an elastic fluid was obtained, which was almost entirely absorbed by one-fourth of its volume of water.

This gas in its pure form is so easily decomposable, that it is dangerous to operate upon considerable quantities.

In one set of experiments upon it, a jar of strong glass,

* My brother, Mr. J. Davy, from whom I receive constant and able assistance in all my chemical inquiries, had several times observed explosions, in transferring the gas from hyper-oxymuriate of potash, over mercury, and he was inclined to attribute the phænomenon to the combustion of a thin film of mercury, in contact with a globule of gas. I several times endeavoured to produce the effect, but without success, till an acid was employed for the preparation of the gas, so diluted as not to afford it without the assistance of heat. The change of colour and expansion of volume, when the effect took place, immediately convinced me that it was owing to a decomposition of the gas.

containing

containing 40 cubical inches, exploded in my hands with a loud report, producing light; the vessel was broken, and fragments of it were thrown to a considerable distance.

I analysed a portion of this gas, by causing it to explode over mercury in a curved glass tube, by the heat of a spirit lamp.

The oxymuriatic gas formed, was absorbed by water; the oxygen was found to be pure, by the test of nitrous gas.

Fifty parts of the detonating gas, by decomposition, expanded so as to become 60 parts. The oxygen, remaining after the absorption of the oxymuriatic gas, was about 20 parts. Several other experiments were made, with similar results. So that it may be inferred, that it consists of two in volume of oxymuriatic gas, and one in volume of oxygen; and the oxygen in the gas is condensed to half its volume. Circumstances conformable to the laws of combination of gaseous fluids, so ably illustrated by M. Gay Lussac, and to the theory of definite proportions.

I have stated on a former occasion, that approximations to the numbers representing the proportions in which oxygen and oxymuriatic gas combine, are found in 7·5 and 32·9. And this compound gas contains nearly these quantities *.

The smell of the pure explosive gas somewhat resembles that of burnt sugar, mixed with the peculiar smell of oxymuriatic gas. Water appeared to take up eight or ten times its volume; but the experiment was made over mercury, which might occasion an error, though it did not seem to act on the fluid. The water became of a tint approaching to orange.

When the explosive gas was detonated with hydrogen, equal to twice its volume, there was a great absorption, to more than one-third, and solution of muriatic acid was formed; when the explosive gas was in excess, oxygen was always expelled, a fact demonstrating the stronger attraction of hydrogen for oxymuriatic gas than for oxygen.

* In page 245 of the Phil. Trans. for 1810, I have mentioned that the specific gravity of oxymuriatic gas is between 74 and 75 grains per 100 cubical inches. The gas that I weighed, was collected over water and procured from hyper-oxymuriate of potash, and at that time I conceived, that this elastic fluid did not differ from the oxymuriatic gas from manganese, except in being purer. It probably contained some of the new gas; for I find that the specific gravity of pure oxymuriatic gas from manganese, and muriatic acid is to that of common air, as 244 to 100. Taking this estimation, the specific gravity of the new gas will be about 238, and the number representing the proportion in which oxymuriatic gas combines, from this estimation, will be rather higher than is stated above.

I have

I have said that mercury has no action upon this gas in its purest form at common temperatures. Copper and antimony, which so readily burn in oxymuriatic gas, did not act upon the explosive gas in the cold: and when they were introduced into it, being heated, it was instantly decomposed, and its oxygen set free; and the metals burnt in the oxymuriatic gas.

When sulphur was introduced into it, there was at first no action, but an explosion soon took place: and the peculiar smell of oxymuriate of sulphur was perceived.

Phosphorus produced a brilliant explosion, by contact with it in the cold, and there were produced phosphoric acid and solid oxymuriate of phosphorus.

Arsenic introduced into it did not inflame; the gas was made to explode, when the metal burnt with great brilliancy in the oxymuriatic gas.

Iron wire introduced into it did not burn, till it was heated so as to produce an explosion, when it burnt with a most brilliant light in the decomposed gas.

Charcoal introduced in it ignited, produced a brilliant flash of light, and burnt with a dull red light, doubtless owing to its action upon the oxygen mixed with the oxymuriatic gas.

It produced dense red fumes when mixed with nitrous gas, and there was an absorption of volume.

When it was mixed with muriatic acid gas, there was a gradual diminution of volume. By the application of heat the absorption was rapid, oxymuriatic gas was formed, and a dew appeared on the sides of the vessel.

These experiments enable us to explain the contradictory accounts that have been given by different authors of the properties of oxymuriatic gas.

That the explosive compound has not been collected before, is owing to the circumstance of water having been used for receiving the products from hyper-oxymuriate of potash, and unless the water is highly saturated with the explosive gas, nothing but oxymuriatic gas is obtained; or to the circumstance of too dense an acid having been employed.

This substance produces the phænomena which Mr. Chenevix, in his able paper on oxymuriatic acid, referred to the hyper-oxygenized muriatic acid; and they prove the truth of his ideas respecting the possible existence of a compound of oxymuriatic gas, and oxygen in a separate state.

The explosions produced in attempts to procure the products of hyperoxymuriate of potash by acids are evidently owing

owing to the decomposition of this new and extraordinary substance.

All the conclusions which I have ventured to make respecting the undecompounded nature of oxymuriatic gas, are, I conceive, entirely confirmed by these new facts.

If oxymuriatic gas contained oxygen, it is not easy to conceive why oxygen should be afforded by this new compound to muriatic gas, which must already contain oxygen in intimate union. Though, on the idea of muriatic acid being a compound of hydrogen and oxymuriatic gas, the phænomena are such as might be expected.

If the power of bodies to burn in oxymuriatic gas depended upon the presence of oxygen, they all ought to burn with much more energy in the new compound; but copper and antimony, and mercury, and arsenic, and iron, and sulphur have no action upon it till it is decomposed; and they act then according to their relative attractions on the oxygen, or on the oxymuriatic gas.

There is a simple experiment which illustrates this idea: Let a glass vessel containing brass foil be exhausted, and the new gas admitted, no action will take place; throw in a little nitrous gas, a rapid decomposition occurs, and the metal burns with great brilliancy.

Supposing oxygen and oxymuriatic gas to belong to the same class of bodies; the attraction between them might be conceived very weak, as it is found to be, and they are easily separated from each other, and made repulsive by a very low degree of heat.

The most vivid effects of combustion known, are those produced by the condensation of oxygen or oxymuriatic gas; but in this instance, a violent explosion with heat and light are produced by their separation, and expansion, a perfectly novel circumstance in chemical philosophy.

This compound destroys dry vegetable colours, but first gives them a tint of red. This and its considerable absorbability by water would incline one to adopt Mr. Chenevix's idea, that it approaches to an acid in its nature. It is probably combined with the peroxide of potassium in the hyperoxymuriate.

That oxymuriatic gas and oxygen combine and separate from each other with such peculiar phænomena, appears strongly in favour of the idea of their being distinct, though analogous species of matter. It is certainly possible to defend the hypothesis that oxymuriatic gas consists of oxygen united to an unknown basis; but it would be possible

 likewise

likewise to defend the speculation that it contains hydrogen.

Like oxygen, it has not yet been decomposed; and I some time ago made an experiment, which, like most of the others I have brought forward, is very adverse to the idea of its containing oxygen.

I passed the solid oxymuriate of phosphorus in vapour, and oxygen gas together through a green glass tube heated to redness.

A decomposition took place, and phosphoric acid was formed, and oxymuriatic gas was expelled.

Now, if oxygen existed in the oxymuriate of phosphorus, there is no reason why this change should take place. On the idea of oxymuriatic gas being undecompounded, it is easily explained. Oxygen is known to have a stronger attraction for phosphorus than oxymuriatic gas has, and consequently ought to expel it from this combination.

As the new compound in its purest form is possessed of a bright yellow green colour, it may be expedient to designate it by a name expressive of this circumstance, and its relation to oxymuriatic gas. As I have named that elastic fluid Chlórine, so I venture to propose for this substance the name Euchlorine, or Euchloric gas, from ευ and χλωρος. The point of Nomenclature I am not, however, inclined to dwell upon. I shall be content to adopt any name that may be considered as most appropriate by the able chemical philosophers attached to this Society.

[*** In page 418, line 22, of the Bakerian lecture, for "water separated and Libavius's liquor was formed," *read* "a compound of water and Libavius's liquor separated." In page 414, it is stated that magnesia is not decomposed by oxymuriatic gas at a red heat. From some experiments of MM. Gay Lussac and Thenard, *Bullet. de la Societ. Phil. Mai*, 1810, it appears that oxygen is procured by passing oxymuriatic gas over magnesia, at a high temperature, and that a muriate indecomposable by heat is proved. They attribute the presence of this oxygen to the decomposition of the acid; but according to all analogies, it must arise from the decomposition of the earth.]

III. *On the Non-existence of Sugar in the Blood of Persons labouring under Diabetes Mellitus. In a Letter to* Alexander Marcet, *M.D. F.R.S. from* William Hyde Wollaston, *M.D. Sec. R.S.**

My dear Sir, In reply to your inquiry respecting my experiments upon the non-existence of sugar in the serum of

* From the Philosophical Transactions for 1811, Part I.

of diabetic persons, which I have mentioned to you at different periods, I am really ashamed to reflect how long I have suffered them to remain neglected, when I consider their tendency to elucidate a curious point of physiological research.

My first endeavours to detect sugar in the serum of the blood were made soon after perusing the second edition of Dr. Rollo's Treatise on the Diabetes, (which was published in 1798,) at the request of Dr. Baillie, who was so obliging as to furnish me with various specimens of diabetic blood and serum for this purpose.

The other set of experiments which I made with reference to the same question were not thought of till the following year. The inquiry was then left unfinished, and I never resumed it; for as I soon after* relinquished the practice of physic, I desisted in a great measure from prosecuting any inquiries connected with medicine.

However, since so much of this subject as is strictly physiological, relating to the natural course of circulating fluids, and more especially so much of the investigation as is conducted by chemical means, is within the range of those pursuits which are generally interesting to the Royal Society, I will endeavour to give you as distinct an account as I am able of the progress of my own experiments, requesting that you will in return state, more fully than you have hitherto done, the result of that further step in the inquiry which you took at my suggestion; and if it is agreeable to you, we will without delay make a joint communication of our researches to the Society.

Although Dr. Rollo had been assisted in the chemical part of his inquiry by the well-known talents of Mr. Cruickshank, it appears that they "had not been so fortunate as to obtain a sufficient quantity of serum for chemical experiment†;" and were unable fully to satisfy themselves, by the taste or by other means which they could employ, concerning the existence or non-existence of sugar in the blood of persons labouring under diabetes; but nevertheless they were persuaded of its presence.

For the purpose of forming some judgement on this question, Mr. Cruickshank made trial of the quantities of oxalic acid that could be formed from serum or from blood in their natural state, and from the same serum or blood after the addition of a certain proportion of sugar; and from the difference perceptible in these trials, he formed a

* In 1800. † Rollo on Diabetes, p. 408.

probable conjecture respecting the presence or absence of sugar in the serum of diabetic persons.

This method, it is evident, is liable to a two-fold objection: first, that an excess of other ingredients beside sugar will cause an increase of the quantity of oxalic acid formed; and secondly, that slight variations in the process for forming oxalic acid will unavoidably occasion differences in the result.

The method which I employed appears to me capable of detecting much smaller quantities of such an ingredient; for though it might not enable us to distinguish exactly the nature of any small quantity that may be discovered, still the mere question of absence or presence admits of determination with great precision.

For this purpose I investigated, in the first place, how the albuminous part of healthy serum could be most completely coagulated, and by what appearances the presence of sugar that had been added to it would be most easily discerned.

When heat alone had been employed for the coagulation of serum, to which water had been added, that which exuded from it was still found to contain a portion of albumen dissolved in it; and if this were allowed to remain, any saccharine matter which might be present would be disguised, and could not with certainty be detected.

I found, however, that this residuum of coagulable matter might be altogether prevented by the addition of a small quantity of dilute acid to the serum before coagulation*. To six drachms of serum I added half a drachm of muriatic acid previously diluted with one drachm and a half of water, and immersed the phial containing them in boiling water during four minutes. The coagulation was thus rendered complete. In the course of a few hours a drachm or more of water exudes from serum that has been so coagulated. If a drop of this water be evaporated, the salts which it contains are found to crystallize, so that the form of the crystals may be easily distinguished; they are principally common salt.

If any portion of saccharine matter has been added to the serum previous to coagulation, the crystallization of the salts is impeded, or wholly prevented, according to the quantity of sugar present.

If the quantity added does not exceed two grains and a

* I presumed that this portion of albumen was retained in solution by the alkali redundant in serum, and added the acid for the purpose of neutralizing it.

half

half to the ounce, the crystallization is not prevented; but even this small quantity is perceptible by a degree of blackness that appears after evaporation; occasioned, as I suppose, by the action of a small excess of acid on the sugar.

If five grains have been added, the crystallization is very imperfect, and soon disappears in a moist air by deliquescence of the sugar. The blackness is also deeper than in the former case.

By addition of ten grains to the ounce, the crystallization of the salts is entirely prevented, and the degree of blackness and disposition to deliquesce are of course more manifest than with smaller quantities.

As I was aware that the sugar obtained from diabetic urine is a different substance from common sugar (approaching more nearly to the sugar of figs), I had the precaution to repeat the same series of experiments upon serum, to which I made corresponding additions of dry sugar, that I had formerly extracted from the urine of a person who voided it in considerable quantity; and I found the effects to be perfectly similar in every respect.

As a further test of the absence or presence of sugar, I found it convenient to add a little nitric acid to the salts that remained after crystallization of the drop. If the serum has been successfully coagulated without any addition of sugar, the addition of nitric acid merely converts the muriatic salts into nitrates, and nitrate of soda is seen to crystallize without foam or blackness. But when sugar has been added, a white foam rises round the margin of the drop; and if further heat be applied, it becomes black in proportion to the quantity of sugar present.

Such are the appearances when the proportions have been duly adjusted, and the proper heat for coagulation applied. I must own, however, that I could not always succeed to my satisfaction at the time when these experiments were conducted, and I am inclined to ascribe occasional failures to having used more muriatic acid than was really necessary, which by excess of heat might redissolve a part of the coagulated albumen, and thence occasion appearances which, without careful discrimination, might be ascribed to sugar.

After having, by this course of experiment, satisfied myself as to the phænomena exhibited by serum in its natural state, and the effects of any small additions of sugar, I then proceeded to the examination of such specimens of diabetic blood or of serum, as I was able to procure.

The first which I examined was a portion of blood that had been taken from a person whose urine had been analysed, and found to contain sugar. This blood had been dried, when fresh, by a gentle heat, so as not to coagulate the serum. After being reduced to powder, it was mixed with water, in order that every thing which remained soluble might be extracted. A little muriatic acid was then added, and sufficient heat applied for coagulation of the albumen. The water that separated after coagulation was found to contain the salts of the blood, but no trace whatever of sugar.

A second specimen of dried blood, that had been ascertained to be diabetic on the same evidence as the preceding, was examined in a similar manner, with the same result, as no appearance of sugar could be discerned.

In a third instance, I had some serum from the blood of a person whose urine had been tasted, and found "*very sweet.*" (I had no opportunity of procuring any of this urine for analysis.) After a portion of this serum had been coagulated, with the addition of the usual proportion of muriatic acid, there was no appearance whatever of sugar. But when three grains of diabetic sugar had been added to another ounce of the same serum, the presence of this quantity was manifest by the same process.

I had also a fourth opportunity of examining serum of a person whose urine contained so much saccharine matter, that an ounce of it yielded, by evaporation, thirty-six grains of extract. In this instance I was not so successful in my experiment; for, though I was satisfied that no sugar was present, there certainly was a degree of blackness, which might have been occasioned by about one grain and a half of sugar in the ounce of serum. But this black matter appeared not to be sugar: it was more easily dried than sugar: it was not fusible by heat, as sugar is: and its refractive power* was too great for that of sugar.

I unfortunately had no opportunity of repeating the experiment on a second portion of the same serum, having inconsiderately employed it for other experiments, and coagulated it at the same time with the former.

In the next experiment I added half a drachm of the urine of the same person to six drachms of the serum, and with a due proportion of diluted muriatic acid coagulated as before. Although the quantity of extract added did not exceed $\frac{36}{16}$, or two grains and a quarter of extract, the difference was very manifest by the darkness of the colour and the defective crystallization of the salts.

* The method by which this was tried has since that time been described in the Philosophical Transactions for 1802.

To

To the remaining quantity of the serum I had added twice the former proportion of the urine, and found that this quantity did not wholly prevent the crystallization of the salts during the evaporation of the drop.

The result of these trials was such as to satisfy me that the serum in this instance contained no perceptible quantity of sugar, or at least that the water separable from the coagulated serum did not contain one-thirtieth part of that proportion which I had found in the urine of the same person.

In order to account for the presence of sugar in the urine, we must consequently either suppose a power in the kidneys of forming this new product by secretion, which does not seem to accord with the proper office of that organ; or, if we suppose the sugar to be formed in the stomach by a process of imperfect assimilation, we must then admit the existence of some channel of conveyance from the stomach to the bladder, without passing through the general system of blood-vessels. That some such channel does exist, Dr. Darwin* endeavoured to ascertain, by giving large doses of nitre, which he could perceive to pass with the urine, but could not detect in its passage through the blood; and he imagined the channel by which it was conveyed to be the absorbent system, upon the supposition that they might admit of a retrograde motion of their contents.

Without adopting the theory of Dr. Darwin, it did appear to me that the fact deserved to be ascertained by some test more decisive than nitre; and I conceived that if prussiate of potash could be taken with safety, its presence would be discerned by means of a solution of iron in as small proportion as almost any known chemical test. Upon trial of this salt, I found that a solution of it might be taken without the least inconvenience, and that in less than one hour and a half the urine became perceptibly impregnated, and continued so to the fifth or sixth hour, although the quantity taken had not amounted to more than three grains of the salt.

After a few previous trials of the period when the principal impregnation of the urine might be expected, and when the presence of the prussiate (if it existed in the blood) might with most reason be presumed to occur, a healthy person about thirty-four years of age was induced to take a dose corresponding to three grains and a half of the dry

* Account of the retrograde Motion of the absorbent Vessels, by Charles Darwin.

salt, and to repeat it every hour to the third time. The urine being examined every half hour, was found in two hours to be tinged, and to afford a deep blue at the end of four hours. Blood was then taken from the arm, and the coagulum, after it had formed, was allowed to contract, so that the serum might be fully separated. The presence of the prussiate was then endeavoured to be discovered by means of a solution of iron, but without effect; and as I thought that the redundant alkali (which had been ascertained to prevail in this serum) might tend to prevent the appearance of the precipitate, I added a small quantity of dilute acid; but still I could not discern that any degree of blueness was occasioned by it.

This experiment having been repeated a second time with the same result, seemed to me nearly conclusive with respect to the existence of some passage, by which substances certainly known to be in the stomach may find their way to the bladder without being mixed with the general mass of circulating fluids.

Being desirous of ascertaining whether the prussiate could be discovered in any other secretions, I have repeatedly examined my saliva, at times when the urine has manifested a very strong blue, by adding solution of iron, but I could at no time perceive the saliva to be tinged.

I have also, during a severe cold, accompanied with profuse running of water from the nose, made a similar examination of this discharge, but have not been able to perceive any trace of the prussic acid.

It was nearly in this state that I left the inquiry at the period I have mentioned, and I do not remember to have made any other experiments, when I requested your assistance in making trial of the serum that is secreted in consequence of the application of a blister. Your report upon the result of your experiments, in addition to those which I have above related, nearly satisfied me as to the existence of some unknown channel of conveyance by which substances may reach the bladder.

With respect to Dr. Darwin's conception of a retrograde action of the absorbents, it is so strongly opposed by the known structure of that system of vessels, that I believe few persons will admit it to be in any degree probable.

Since we have become acquainted with the surprising chemical effects of the lowest states of electricity, I have been inclined to hope that we might from that source derive some explanation of such phænomena. But though

I have

I have referred* secretion in general to the agency of the electric power with which the nerves appear to be indued, and am thereby reconciled to the secretion of acid urine, from blood that is known to be alkaline, which before that time seemed highly paradoxical; and although the transfer of the prussiate of potash, of sugar, or of other substances may equally be effected by the same power as acting cause, still the channel through which they are conveyed remains to be discovered by direct experiment.

I have, indeed, conjectured that, by examining the blood in the abdominal vessels, or contents of the lacteals, it might be possible to detect them *in transitu;* but I have not been inclined to make such experiments on living animals, as would perhaps throw light upon the subject.

I remain, dear sir,
with great regard, yours very truly,

January 1, 1811. W. H. WOLLASTON.

Reply of Dr. MARCET *on the same Subject.*

Russell Square, January 8, 1811.

MY DEAR SIR,—I AM much gratified to find that you have at last been induced to communicate to the Royal Society your curious inquiry respecting the state of the blood in diabetes. I was anxious that the specious hypothesis of the presence of sugar in diabetic blood, which had been sanctioned by the authority of Dr. Rollo and Mr. Cruickshank, and which I had myself urged in support of their theory, fourteen years ago, in an inaugural publication, should no longer obtain an undue weight amongst physiological inquirers.

With regard to the experiments which I tried at your request some years ago, with a view to ascertain whether prussiat of potash taken into the stomach, and found to exist in the urine, could also be detected in other secretions, I find, on referring to my memorandums, the following particulars, which I shall transcribe verbatim.

"August 19, 1807. Having heard from Dr. Wollaston, that prussiat of potash could be taken into the stomach with perfect safety, and that its presence could afterwards be discovered in the urine, but not in the serum; and being invited by him to follow up this inquiry, with a view to connect it with the theory of diabetes, I tried the following experiments.

* Phil. Mag. for June 1809.

Experiment

Experiment 1.—" After having satisfied myself, by trials made by some medical gentlemen upon themselves, that considerable doses of prussiat of potash might be taken without the least inconvenience, I gave to a young woman labouring under diabetes mellitus, five grains of prussiat of potash dissolved in water, and this was repeated every hour till she had taken thirteen or fourteen such doses. After the fifth dose, her urine, by the addition of a drop or two of a solution of sulphat of iron, turned blue instantly. At this period of the experiment, a blister was applied to her stomach, and after a few hours, whilst still taking the prussiat of potash, and whilst the urine strongly indicated its presence, the blister was cut and the serum collected. This serous fluid being, in the same manner as the urine, subjected to the action of a solution of sulphat of iron, did not suffer any change of colour in the least indicative of the presence of prussic acid. Yet the urine still remained capable of imparting a blue colour to a solution of iron, 15 hours after taking the last dose of the prussiat of potash.

Experiment 2.— " The same person being soon afterwards put upon a course of ferruginous medicines, and having taken considerable quantities of sulphat of iron, an idea naturally occurred to me that the phænomenon might perhaps be reversed; but upon adding prussiat of potash to the urine, no vestige of iron could be discovered, and the same attempt was repeated several times with the same negative result.

Experiment 3.— " Dec. 2, 1807. The fluid obtained by means of a blister (as in Experiment 1,) being not immediately derived from the circulation, since it may be considered as the product of a secretion, I was desirous of repeating Dr. Wollaston's experiment on the serum itself, under circumstances of impregnation similar to those in which the serum of the blister was examined.

" For this purpose, a young woman after taking, in divided doses, about a drachm of prussiat of potash in the course of twelve hours, lost some blood by cupping, an operation which had been ordered for a local complaint under which she laboured. The serum having been allowed to separate, and a little nitric acid having been added to it, not the least vestige of prussic acid appeared in applying the test of sulphat of iron, although the urine made during the six hours which preceded and followed the cupping, was strongly impregnated with that acid, and struck a vivid blue upon adding the smallest quantity of iron."

I have

I have only to observe, in addition to these particulars, that the susceptibility by which prussiat of potash is transmitted to the bladder, seems to vary in different individuals; for in five trials, made at Guy's Hospital in Nov. 1805, I failed of discovering any vestige of that salt in the urine of persons who had taken it in quantities sufficient to produce its appearance in others. Three of these individuals, I should observe, were at the time under mercurial treatment, and an idea occurred to me that mercury having a great affinity for prussic acid, the presence of that metal in the system might prevent the effect in question. But as in the two other failures no mercury was present, I cannot lay any stress upon that conjecture. It may be proper to mention, that in the frequent trials which I have made with the prussiat of potash, no symptom or inconvenience whatever has ever occurred which could be ascribed to that salt.

I remain ever, my dear sir,
with great esteem, yours sincerely,
ALEX. MARCET.

P.S.—Whilst revising the proof of this sheet, it has been observed to me by some friends, and in particular by Dr. Henry of Manchester, and Dr. R. Pearson of London, that in order to show distinctly that certain substances find their way to the bladder without passing through the general circulation, it would be necessary to examine the arterial, as well as the venous blood, since it is not impossible that the whole of the sugar in diabetes, or the prussiat of potash in the experiments above related, may be conveyed to the urinary organs by the arteries, without entering the venous system. According to this hypothesis, it may be conceived that the same substances when conveyed by the arteries to distant parts of the body, may return by the absorbent system, and might in that case be discovered in the thoracic duct. This view of the subject may deserve further investigation; and I hope that this curious question will soon be decided by appropriate experiments.

IV. *On the Decomposition of the Muriate of Soda, by means of the Waters flowing from the Lixiviation of the efflorescent pyritous Earths.* By M. LEFROY, *Mining Engineer**.

THE soda used in France is almost entirely of foreign manufacture; and the present maritime war, added to the

* *Journal des Mines*, vol. xxvii. p. 221.

disturbances

disturbances in Spain, has rendered it so scarce that its value has been of late doubled.

The French government, however, has felt the importance of encouraging the manufacture of this article at home, and the imposts on muriate of soda employed for such purposes have been accordingly removed.

The process most in use for the preparation of soda consists, 1st. in freeing the muriatic acid from sea salt, and forming a sulphate of soda: 2d, in decomposing the sulphate of soda, by the intermedium of chalk and charcoal.

The last of these operations is always the same, the first only being variable.

In most establishments, the sulphuric acid is employed in the decomposition of the muriate of soda. The proportion for a metrical quintal of sea salt is a quantity of sulphuric acid at 45°, representing a metrical quintal at 66° of Beaumé's areometer.

If on the one hand this operation is the simplest and most expeditious, since we speedily form dry sulphate of soda, it is on the other hand very expensive, as the sulphur, the base of the sulphuric acid, is a foreign product, which is procured with difficulty even at the extravagent price of 140 francs for 100 kilogrammes.

It may be easily conceived, therefore, that these new establishments are precarious, that they may be occasionally paralysed, and that at the conclusion of peace they could not come in competition with those of other countries.

The only method of avoiding this inconvenience would be to substitute some indigenous substance for the sulphuric acid, and this is the object which I have had in view.

After many experiments, the process which appeared to me to be most advantageous consists in substituting for the sulphuric acid the waters produced by the lixiviation of the efflorescent vitriolic earths. When there is a proper proportion of these waters, and the operation is well managed, we effect a complete decomposition of the muriate of soda, and obtain for 100 parts of dry and pure muriate of soda, 107 of dry sulphate of soda, or 243 of crystallized sulphate of soda*.

The vitriolic waters which I have hitherto employed for the extrication of sea salt come from the black pyritous earths

* According to the experiments made on a large scale, by order of the Committee of Public Safety, it was ascertained that the sulphuric acid might be dispensed with on using the sulphuret of iron or the sulphate of iron; but we do not procure by these agents the entire decomposition of the muriate of soda, whereas by the vitriolic waters the whole of the muriate of soda

earths which are found in the departments of the Aisne and Oise. They contain on an average for 100 parts at 30° (Beaumé's areometer):

Sulphuric acid	13
Oxide of iron	7
Alumine	2

As the sulphates of iron and of alumine are not entirely decomposed by calcination, since there are only about nine parts of sulphuric acid which remain free, it results that, for the decomposition of a metrical quintal of sea salt, there must be eight metrical quintals of vitriolic waters at 30°, or 634 kilogrammes at 40°, a quantity which answers to 72 kilogrammes of dry sulphuric acid, or to 100 kilogrammes at 66°.

The operation consists,

1. In lixiviating the efflorescent vitriolic substances.

2. In pouring them into leaden pots until they mark 40°.

3. In concentrating them in melting pots of the capacity of 100 litres down to 5 or 6 centimetres below the edges, which gives for each pot about 126 kilogrammes of water at 40°.

4. Put afterwards into each pot 20 kilogrammes of sea salt, raise the fire, and brew the mixture until the whole salt is dissolved, and the matter has acquired the consistence of a thick syrup.

5. Pour the matter into wooden troughs, and throw it when it has become solid into a reverberating furnace where it is calcined in three or four hours.

6. Withdraw this calcined substance, and proseed afterwards to the lixiviation and concentration of the leys, in order to obtain by crystallization the pure sulphate of soda.

As the sulphate of soda of commerce is not pure, and as it contains more than 10 per cent. both in humidity and in earthy substances and foreign salts, and as it undergoes several losses in the various manipulations, we could only on a large scale obtain about 205 or 215 kilogrammes of crystallized sulphate of soda per metrical quintal of muriate of soda.

For upwards of a year I have gone through this process, in the manufactory of M. Carpentier at Chailleret, in the

soda is converted into sulphate of soda. This decomposition is facilitated, 1st, because the iron, being highly oxidated in the vitriolic waters, has less of the sulphuric acid: 2dly, because the muriate of soda having been dissolved, each of its molecules is in contact with a molecule of sulphuric acid: 3dly, because the matter preserves to the end of the calcination the humidity necessary to the extrication of the muriatic acid.

department

department of the Aisne. In a single reverberating furnace, the floor of which is 24 feet square, we decomposed in 24 hours 500 kilogrammes of muriate of soda.

The departments of the Aisne and of the Oise present great advantages for the manufacture of factitious sodas. They are traversed by navigable rivers and canals which secure a vent. The price of labour is low in that neighbourhood, and vitriolic earths are very abundant. Numerous valleys contain immense quantities of turf for fuel; and when the canal of St. Quintin is finished, the coals of the çidevant Belgium may be easily conveyed thither. Several chemists have already erected manufactories in these favourable districts, and bid fair to succeed in furnishing France with an ample supply of soda in future.

V. *On the Composition of Zeolite.* *By* JAMES SMITHSON, *Esq. F.R.S.**

MINERAL bodies being, in fact, *native chemical preparations*, perfectly analogous to those of the laboratory of art, it is only by chemical means that their species can be ascertained with any degree of certainty, especially under all the variations of mechanical state and intimate admixture with each other to which they are subject.

And accordingly we see those methods which profess to supersede the necessity of chemistry in mineralogy, and to decide upon the species of it by other means than hers, yet bringing an unavoidable tribute of homage to her superior powers, by turning to her for a solution of the difficulties which continually arise to them, and to obtain firm grounds to relinquish or adopt the conclusions to which the principles they employ lead them.

Zeolite and natrolite have been universally admitted to be species distinct from each other, from Mr. Klaproth having discovered a considerable quantity of soda and no lime in the composition of the latter, while Mr. Vauquelin had not found any portion of either of the fixed alkalies, but a considerable one of lime, in his analysis of zeolite†.

The natrolite has been lately met with under a regular crystalline form, and this form appears to be perfectly similar to that of zeolite, but Mr. Haüy has not judged himself warranted by this circumstance to consider these two

* From the Philosophical Transactions for 1811, Part I.

† *Journal des Mines*, No. xliv.

bodies

bodies as of the same species; because zeolite, he says, "does not contain an atom of soda*."

I had many years ago found soda in what I considered to be zeolites, which I had collected in the island of Staffa, having formed Glauber's salt by treating them with sulphuric acid; and I have since repeatedly ascertained the presence of the same principle in similar stones from various other places; and Dr. Hutton and Dr. Kennedy had likewise detected soda in bodies, to which they gave the name of zeolite.

There was, however, no certainty that the subjects of any of these experiments were of the same nature as what Mr. Vauquelin had examined, or were of that species which Mr. Haüy calls mesotype.

Mr. Haüy was so obliging as to send me lately some specimens of minerals. There happened to be amongst them a cluster of zeolite in rectangular tetrahedral prisms, terminated by obtuse tetrahedral pyramids whose faces coincided with those of the prism. These crystals were of a considerable size, and perfectly homogeneous, and labelled by himself "*Mesotype pyramidée du depart. du Puy de Dôme.*" I availed myself of this very favourable opportunity, to ascertain whether the mesotype of Mr. Haüy and natrolite, did or did not differ in their composition, and the results of the experiments have been entirely unfavourable to their separation, as the following account of them will show.

Ten grains of this zeolite being kept red hot for five minutes lost 0·75 grains, and became opake and friable. In a second experiment, ten grains being exposed for ten minutes to a stronger fire, lost 0·95 grains, and consolidated into a hard transparent state.

Ten grains of this zeolite, which had not been heated, were reduced to a fine powder, and diluted muriatic acid poured upon it. On standing some hours, without any application of heat, the zeolite entirely dissolved, and some hours after, the solution became a jelly: this jelly was evaporated to a dry state, and then made red hot.

Water was repeatedly poured on to this ignited matter till nothing more could be extracted from it. This solution was gently evaporated to a dry state, and this residuum made slightly red hot. It then weighed 3·15 grains. It was *muriate of soda*.

The solution of this muriate of soda being tried with so-

* *Journal des Mines*, No. cl. Juin 1810, p. 458.

lutions

lutions of carbonate of ammonia and oxalic acid, did not afford the least precipitate, which would have happened had the zeolite contained any lime, as the muriate of lime* would not have been decomposed by the ignition.

The remaining matter, from which this muriate of soda had been extracted, was repeatedly digested with marine acid, till all that was soluble was dissolved. What remained was silica, and, after being made red hot, weighed 4·9 grains.

The muriatic solution, which had been decanted off from the silica, was exhaled to a dry state, and the matter left made red hot. It was alumina.

To discover whether any magnesia was contained amongst this alumina, it was dissolved in sulphuric acid, the solution evaporated to a dry state, and ignited. Water did extract some saline matter from this ignited alumina, but it had not at all the appearance of sulphate of magnesia, and proved to be some sulphate of alumina which had escaped decomposition; for on an addition of sulphate of ammonia to it, it produced crystals of compound sulphate of alumina and ammonia, in regular octahedrons.

This alum and alumina were again mixed and digested in ammonia, and the whole dried and made red hot. The alumina left, weighed 3·1 grains.

Being suspected to contain still some sulphuric acid, this alumina was dissolved in nitric acid, and an excess of acetate of barytes added. A precipitate of sulphate of barytes fell, which, after being edulcorated and made red hot, weighed 1·2 grains. If we admit one-third of sulphate of barytes to be sulphuric acid, the quantity of the alumina will be $= 3{\cdot}1 - 0{\cdot}4 = 2{\cdot}7$ grains.

From the experiments of Dr. Marcet †, it appears that 3·15 grains of muriate of soda afford 1·7 grains of soda.

Hence, according to the foregoing experiments, the ten grains of zeolite analysed, consisted of

Silica	4·90
Alumina	2·70
Soda	1·70
Ice	0·95
	10·25

* These names are retained for the present, as being familiar, though, since Mr. Davy's important discovery of the nature of what was called oxymuriatic acid, the substances to which they are applied are known not to be salts, but metallic compounds analogous to oxides.

† Philosophical Transactions, 1807.

As

As these experiments had been undertaken more for the purpose of ascertaining the nature of the component parts of this zeolite than their proportions, the object of them was considered as accomplished, although perfect accuracy in the latter respect had not been attained, and which, indeed, the analysis we possess of natrolite by the illustrious chemist of Berlin renders unnecessary.

I am induced to prefer the name of zeolite for this species of stone, to any other name, from an unwillingness to obliterate entirely from the nomenclature of mineralogy, while arbitrary names are retained in it, all trace of one of the discoveries of the greatest mineralogist who has yet appeared, and which, at the time it was made, was considered as, and was, a very considerable one, being the first addition of an earthy species, made by scientific means, to those established immemorially by miners and lapidaries, and hence having, with tungstein and nickel, led the way to the great and brilliant extension which mineralogy has since received. And of the several substances which, from the state of science in his time, certain common qualities induced Baron Cronstedt to associate together under the name of zeolite, it is this which has been most immediately understood as such, and whose qualities have been assumed as the characteristic ones of the species.

Indeed, I think that the name imposed on a substance by the discoverer of it, ought to be held in some degree sacred, and not altered without the most urgent necessity for doing it. It is but a feeble and just retribution of respect for the service which he has rendered to science.

Professor Struve, of Lausanne, whose skill in mineralogy is well known, having mentioned to me, in one of his letters, that from some experiments of his own he was led to suspect the existence of phosphoric acid in several stones, and particularly in the zeolite of Auvergne, I have directed my inquiries to this point, but have not found the phosphoric, or any other acknowledged mineral acid, in this zeolite.

Many persons, from experiencing much difficulty in comprehending the combination together of the earths, have been led to suppose the existence of undiscovered acids in stony crystals. If quartz be itself considered as an acid, to which order of bodies its qualities much more nearly assimilate it than to the earths, their composition becomes readily intelligible. They will then be neutral salts, silicates, either simple or compound. Zeolite will be a compound salt, a hydrated silicate of alumina and soda, and

 hence

hence a compound of alumina not very dissimilar to alum. And topaz, whose singular ingredients, discovered by Mr. Klaproth, have called forth a query from the celebrated Mr. Vauquelin, with regard to the mode of their existence together *, will be likewise a compond salt, consisting of silicate of alumina and fluate of alumina.

Our acquaintance with the composition of the several mineral substances is yet far too inaccurate to render it possible to point out with any degree of certainty the one of which zeolite is an hydrate; however, the agreement of the two substances in the nature of their constituent parts, and in their being both electrical by heat, directs conjecture towards tourmaline.

St. James's Place, Jan. 22, 1811.

Addition to the Account of native Minium.

After I had communicated to the president the account of the discovery of native minium, printed in the Philosophical Transactions for 1806, I learned that this ore came from the lead mines of Breylau in Westphalia.

VI. *Report made to the Institute on a Memoir, by M.* Tarry, *on the Composition of Writing Ink. By Messrs.* Berthollet, Vauquelin, *and* Deyeux †.

The object proposed by M. Tarry in his memoir is to explain:

1. The processes employed for discharging writing from paper.
2. The processes for reviving writings which have been apparently obliterated.
3. The best way to improve common ink.
4. Finally, the discovery of an ink which should resist all chemical agents.

We shall now give an abridgement of these four articles:

ARTICLE I.

Processes for discharging Writing.—The art of discharging writing is very ancient, and the means employed are very simple. In fact, we know that it is sufficient to moisten a written paper with any acid, when the writing will gradually disappear. But all the acids cannot be employed with equal success. Some leave a stain on the paper which is not

* *Annales du Museum d'Hist. Nat.* tome vi. p. 24.
† *Annales de Chimie,* tome lxxv. p. 194.

easily

easily removed: others corrode and render the paper unserviceable. The way to avoid these inconveniences is to make choice of an acid which shall act on the writing only, without injuring the paper or giving it a colour different from that which it had before it was written upon.

In order to discover such of the acids as are best suited for the operation in question, the author determined to submit common writing ink to the action of different acids, and to observe carefully the phænomena which these bodies present at the time of their mixture. According to him, the sulphuric acid easily takes out writing, but at the same time it gives an oily tint to the paper.

The acid oxalate of potash produces more certain and more prompt effects. The oxygenized muriatic acid, if it be newly made, seems to be preferable to the above two acids, because at the same time that it takes out the writing it bleaches the paper without altering it.

It is not the same case with the nitric acid, which always takes out the ink, but soon penetrates the paper and forms above it undulated lines of a yellow colour.

We may succeed, however, in softening both these effects, by taking the precaution to dilute the nitric acid with a sufficient quantity of water, or to wash the paper immediately after the writing has been taken out.

A mixture of the muriatic and nitric acids has but a slow action upon writing. It bleaches the paper and does not oppose its desiccation, as when we employ the nitric acid alone.

In general, whatever be the kind of acid employed to discharge writing, it is always proper when the operation is performed to dip the paper in water, in order to dissolve the new combinations which the acids have formed with the particles of ink which have been discharged.

M. Tarry, at the conclusion of this article, does not fail to observe that China ink does not act like common ink with the acids, as its composition is quite different from that which we use for writing of all kinds. So far from the acids attacking China ink, they make it, on the contrary, of a deep black: it cannot be discharged therefore without erasing it.

ARTICLE II.

Processes for ascertaining what Writing has been substituted for something taken out, and Methods of reviving the Writing which has disappeared.

All the methods which have been given for discharging

 writing

writing consist, as abovementioned, in decomposing the ink and in forcing its constituent parts to form other combinations. These combinations, being decomposed in their turn by different agents, may regain a tint, which, if it be not that of ink, at least exhibits a shade which becomes perceptible enough for ascertaining the letters and words which had been traced on the paper before it was touched by the acids.

The gallic acid is, according to the author, one of those agents, which in this case succeeds very well.

The liquid prussiate of lime also produces a good effect.

It is the same case with the alkaline hydrogenated sulphurets. But it is very certain that we never obtain any success from the employment of these agents, when we have left any acid long in contact with the writing, and particularly if we have washed the paper afterwards.

In short, we may easily conceive, that in this case the constituent parts of the ink which were combined with the acid, and had formed with it compounds soluble in water, having been taken up by this fluid, ought not to leave any trace of their existence longer; and consequently it is impossible that the agents employed for discovering them can render them visible.

It is also for this reason that the gallic acid, the liquid prussiate of lime, the alkaline hydrogenated sulphurets, and so many other reagents which have been so much praised, can no longer be regarded as infallible methods for reviving writing.

ARTICLE III.

Improvement of Common Ink.

Most of the inks now in use are of a bad quality. Some are spontaneously destroyed; others imperceptibly lose their black colour, and assume a yellow one; several, after a length of time, enter into the paper, and spoil it: lastly, there are some which are first pale and then become very black.

All these differences arise from the nature of the substances which have been employed in the making of the ink.

Convinced of the advantage of having a good article of this kind, the author commenced a series of experiments, but is forced to admit that he has not discovered any recipe superior to that which has been published by Lewis. This ink, according to our author, combines every advantage: but we must observe that it is no more exempt than the rest from being dissolved in the acids, and in this respect it has an an inconvenience which those who wish to discharge writing from paper know very well how to profit by. This circumstance, no doubt, induced M. Tarry to make some new experiments in order to obtain an ink which should be inalterable by chemical agents; and he appears to us to have succeeded in his object.

ARTICLE IV.

Discovery of an Ink which resists the Action of chemical Agents.

The author describes his invention in the following words: "My ink is founded upon principles different from those of all others. It contains neither gall-nuts, Brazil wood, or Campeachy, gum, nor any preparation of iron: it is purely vegetable, resists the action of the most powerful vegetables, the most highly concentrated alkaline solutions, and, finally, all the solvents.

"The nitric acid acts very feebly upon the writing performed with this ink. The oxymuriatic acid makes it assume the colour of pigeons' dung. After the action of this last acid, the caustic alkaline solutions reduce it to the colour of carburet of iron: the characters of the writing nevertheless remain without alteration, and it cannot pass through these different states except after long macerations. The principles of which it is composed render it incorruptible, and it can retain its properties many years."

The results which we obtained, coincided entirely with those of the author, and we have no hesitation in saying, that his is the best we have ever seen of the kind which is called indelible ink. It is liable, however, to deposit a sediment, a disadvantage which we think might be removed by M. Tarry after a few more experiments. We have tried to discharge it with all the known chemical agents, but without effect; and we think the inventor deserves the thanks of the Institute, and of the community at large.

VII. *Experiments to prove that Fluids pass directly from the Stomach to the Circulation of the Blood, and from thence into the Cells of the Spleen, the Gall Bladder, and Urinary Bladder, without going through the Thoracic Duct.* By EVERARD HOME, *Esq. F.R.S.**

HAVING on a former occasion laid before the Society some experiments, to prove that fluids pass directly from the car-

* From the Philosophical Transactions for 1811, Part I.

diac portion of the stomach, so as to arrive at the circulation of blood without going through the thoracic duct, the only known channel by which liquids can arrive there; the present experiments are brought to confirm that opinion: but in stating them, I wish to correct an error I was led into, in believing that the spleen was the channel by which they are conveyed.

At the time I made my former communications, I was conscious that the facts I had ascertained were only sufficient to open a new field of inquiry; but as I might never be able to make a further progress in an investigation beset with so many difficulties, I thought it right to put them on record. Since that time I have lost no opportunity of devising new experiments to elucidate this subject; and the circumstance of Mr. Brodie, the assistant of my philosophical as well as professional labours, having tied the thoracic duct in some experiments which will come before the Society, suggested to me the idea, that if the thoracic duct was tied, and proper experiments made, there could be no difficulty in ascertaining whether there was any other channel between the stomach and the circulation of the blood.

With this view I instituted the following experiment, which was made on the 29th of September 1810, by Mr. Brodie, assisted by Mr. William Brande and Mr. Gatcombe. I was unavoidably prevented from being present during the time of the experiment.

Experiment 1.—A ligature was passed round the thoracic duct of a rabbit, just before it enters at the junction between the left jugular and subclavian veins: an ounce of strong infusion of rhubarb was then injected into the stomach. In three quarters of an hour some urine was voided, in which rhubarb was distinctly detected by the addition of potash. An hour and a quarter after the injection of the rhubarb the animal was killed: a drachm and a half of urine was found in the bladder highly tinged with rhubarb, and the usual alteration of colour took place on the addition of potash. The coats of the thoracic duct had given way opposite the middle dorsal vertebra, and nearly an ounce of chyle was found effused into the cavity of the thorax, beside a considerable quantity in the cellular membrane of the posterior mediastinum. Above the ruptured part the thoracic duct was entire, much distended with chyle; and on tracing it upwards, the termination of the duct in the vein was found to be completely secured by the ligature. The lacteal and lymphatic vessels had given way in several parts of of the abdomen, and chyle and lymph were extravasated underneath the peritoneum.

In this and the following experiments the infusion of rhubarb was employed in preference to the prussiate of potash, in consequence of its having been found in those I formerly made, that one drop of tincture of rhubarb could be detected in half an ounce of serum, and nothing less than a quarter of a grain of prussiate of potash in the same quantity could be made to strike a blue colour when the test was added.

Experiment 2.—The experiment was repeated upon a dog. In this I was assisted by Mr. Brodie, Mr. William Brande, Mr. Clift, and Mr. Gatcombe. After the thoracic duct had been secured, two ounces of strong infusion of rhubarb were injected into the stomach, and in an hour the dog was killed. The urine in the bladder, on the addition of potash, became deeply tinged with rhubarb. The bile in the gall bladder, by a similar test, was found to contain rhubarb. The lacteal vessels in several parts of the mesentery had burst, and chyle was extravasated into the cellular membrane; the thoracic duct had given way in the lower part of the posterior mediastinum, and chyle extravasated. Above the ruptured part of the thoracic duct was much distended with chyle; it was readily traced to the ligature, by which it was completely secured.

These experiments appeared to establish the fact, that the thoracic duct was not the channel through which the infusion of rhubarb was conveyed to the circulation of the blood, and it now became easy to ascertain, whether it passed through the spleen, by extirpating that organ, and repeating the last experiment.

On the 21st of October, 1810, the following experiment was made, with the assistance of Mr. Brodie, Mr. Clift, Mr. Gâtcombe, and Mr. Money.

Experiment 3.—The thoracic duct near its termination was secured in a dog whose spleen had been removed four days before, and three ounces of infusion of rhubarb were injected into the stomach: in an hour and half the dog was killed, and the urine was found strongly impregnated with rhubarb; and on examination the thoracic duct was found to be completely secured by the ligature. Several of the lacteals had burst, but the duct itself had not given way; it was greatly distended with chyle and lymph.

By this experiment it was completely ascertained that the spleen is not the channel through which the infusion of rhubarb is conveyed into the circulation of the blood, as

I had been led to believe, and therefore the rhubarb, in my former experiments detected in the spleen, must have been deposited there in the same manner as in the urine and in the bile.

The detection of this error made me more anxious to avoid being misled respecting the thoracic duct; and therefore, although there was little probability that the infusion of rhubarb could have passed into the lymphatic vessels, which open into the blood-vessels of the right side of the neck, I thought it right, before I proceeded further, to repeat the experiment, securing the termination of the thoracic duct on the left side, and the lymphatic trunk of the right side, where it empties itself into the angle between the jugular and subclavian vein. This was done on the 28th of October, 1810, with the assistance of the same persons as in the last experiment.

Experiment 4.—The thoracic duct of a dog was tied, as in the former experiment; in doing it the duct was wounded, and about a drachm of chyle flowed out; the lymphatic trunk of the right side was then secured. After this, three ounces of infusion of rhubarb were injected into the stomach, and in an hour the dog was killed. The urine and the bile were found distinctly impregnated with rhubarb. On opening the thorax, some absorbent vessels, distended with lymph, were seen on the right side of the spine, entering an absorbent gland on the second dorsal vertebra, and the vasa efferentia from the gland were seen uniting with other absorbent vessels, and extending towards the right shoulder, where they formed a common trunk with the absorbents from the neck and axilla; this trunk was found included in the ligature. The thoracic duct was moderately distended with a mixture of chyle and lymph; in tracing it upwards, an opening was seen in it immediately below the ligature, through which the contents readily passed out when pressure was made on the duct: above this opening the duct was completely secured by the ligature. Nearly a drachm of the fluid contained in the thoracic duct was collected and tested by potash, but there did not appear to be any impregnation of rhubarb.

Experiment 5.—The last experiment was repeated on another dog, on the 21st of January, 1811, with the assistance of Mr. Brodie, Mr. W. Brande, Mr. Clift, and Mr. Gatcombe. The dog was killed an hour after the thoracic duct and lymphatic trunk had been secured, and the infusion of rhubarb had been injected into the stomach.

In tying the right lymphatic trunk, a lymphatic vessel, from

from the thorax going to join it, was wounded, from which chyle flowed out in considerable quantity during the whole time of the experiment: a short time before the dog was killed, some of it was collected, but on testing it with potash no rhubarb was detected in it.

The urine was found impregnated with rhubarb, as was also the bile from the gall bladder; but both in a less degree than in the last experiment. The lacteal vessels and mesenteric glands were much distended with chyle; and on cutting into the glands chyle flowed out in considerable quantity. Some of this was collected and tested with potash, but showed no evidence of rhubarb being contained in it. The thoracic duct was much distended; it was traced to the ligature, and was found to be completely secured.

Lymphatic vessels from the right side of the posterior mediastinum were seen extending towards the ligature that had been tied on that side; they were nearly empty; and the trunk formed by the junction of these with the lymphatic vessels from the right axilla, and from the right side of the neck, was seen distinctly included in the ligature.

While Mr. Brodie was tracing the thoracic duct, Mr. William Brande was making an infusion of the spleen, and showed me a section of it, in which the cells were larger, and more distinct, than I had ever seen them in a dog. There was a slight tinge of rhubarb in the infusion from the spleen. A similar infusion was made of the liver; but the quantity of blood contained in it being much greater than in the spleen, the appearance was not sufficiently distinct to decide whether it contained rhubarb or not. These experiments appear completely to establish the fact, that the rhubarb did not pass through the thoracic duct, and therefore must have got into the circulation of the blood by some other channel. They likewise completely overturn the opinion I had adopted, of the spleen being the medium by which the rhubarb had been conveyed, and show that the spleen answers some other purposes in the animal œconomy.

The rhubarb found in the spleen does not arrive there before it enters the circulation; it is therefore most probably afterwards deposited in the cells in the form of a secretion. That the rhubarb goes into the circulation is proved by my former experiments, in which it was detected in the splenic vein. The prussiate of potash is hardly to be discovered in the blood of a living animal, since the proportion which strikes a blue colour on the addition of solution of iron, is

greater

greater than the circulating fluids can be expected to contain at any one time, as it goes off by the secretions nearly as fast as it is received into the blood-vessels. In a moderately sized ass, more than two drachms must be dissolved in the blood before its presence there can be detected.

That the fluid contained in the cells of the spleen is secreted there, is rendered highly probable, since it is most abundant while the digestive organs are employed, and scarcely at all met with when the animal has been some time without food. The great objection to this opinion is, there being no excretory duct but the lymphatic vessels of the spleen; these, however, are both larger and more numerous than in any other organ; they are found in the ass to form one common trunk, which opens into a large gland on the side of the thoracic duct, just above the receptaculum-chyli; and when the quicksilver is made to pass through the branches of this gland, there is a trunk equally large on the opposite side, which makes an angle, and then terminates in the thoracic duct. This fact I ascertained at the Veterinary College, assisted by the deputy professor Mr. Sewell, and Mr. Clift. These lymphatic vessels are equally as large as the excretory ducts of any other glands, and therefore sufficient to carry off the secretion formed in the cells of the spleen; and where a secretion is to be carried into the thoracic duct, it would be a deviation from the general plan of the animal œconomy, were any but lymphatic vessels employed for that purpose.

It is a strong circumstance in favour of the secretion being so conveyed, that, in the last experiment, the lacteals and cells of the spleen were unusually turgid, being placed under similar circumstances, the thoracic duct being so full as not to receive their contents.

The purposes that are answered by such a secretion from the spleen into the thoracic duct cannot at present be ascertained.

VIII. *Description of an Instrument to ascertain the Velocities of Machinery.* By *Mr.* Bryan Donkin*.

Sir, I beg leave, through your means, to lay before the Society of Arts, &c. an instrument of my invention, for indicating the velocity of machines, and which may not improperly

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1810.——The Society voted their gold medal to Mr. B. Donkin for this invention.

improperly be called a tachometer. You will at the same time receive a drawing, with an account of the instrument, and the mode of its application.

I am, sir, most respectfully,
Your obedient humble servant,
BRYAN DONKIN.

Fort Place, Bermondsey,
April 11, 181 .

To C. Taylor, M.D. Sec.

Reference to, and Description of, Mr. Donkin's *Tachometer, or Instrument for indicating the Velocity of Machinery,* Pl. I.

In the employment of machinery it is evidently of great importance to be provided with an easy and ready method for discovering at all times whether the motion of the machine is quicker or slower than what is known to be best adapted for the object in view. This advantage, it is hoped, may be derived from the tachometer; for it is an instrument which requires only to be adjusted once for all, to any particular machine, and then it will always be ready without the help of calculation or of a time-piece, to indicate instantly upon inspection the slightest excess or defect in the actual velocity.

A front view of the tachometer is represented in fig. 1, and a side view in fig. 2, of plate I. XYZ, fig. 1, is the vertical section of a wooden cup, made of box, which is drawn in elevation at X, fig. 2. The whiter parts of the section, in fig. 1, represent what is solid, and the dark parts what is hollow. This cup is filled with mercury up to the level LL, fig. 1. Into the mercury is immersed the lower part of the upright glass tube AB, which is filled with coloured spirits of wine, and open at both ends, so that some of the mercury in the cup enters at the lower orifice, and when every thing is at rest, supports a long column of spirits, as represented in the figure. The bottom of the cup is fastened by a screw to a short vertical spindle D, so that when the spindle is whirled round, the cup, (whose figure is a solid of revolution,) revolves at the same time round its axis, which coincides with that of the spindle.

In consequence of this rotation, the mercury in the cup acquires a centrifugal force, by which its particles are thrown outwards, and that with the greater intensity, according as they are more distant from the axis, and according as the angular velocity is greater. Hence, on account of its fluidity, the mercury rises higher and higher as it recedes from the axis,

axis, and consequently sinks in the middle of the cup; this elevation at the sides and consequent depression in the middle increasing always with the velocity of rotation. Now the mercury in the tube, though it does not revolve with the cup, cannot continue higher than the mercury immediately surrounding it, not indeed so high, on account of the superincumbent column of spirits. Thus the mercury in the tube will sink, and consequently the spirits also; but as that part of the tube which is within the cup is much wider than the part above it, the depression of the spirits will be much greater than that of the mercury, being in the same proportion in which the square of the larger diameter exceeds the square of the smaller.

Let us now suppose, that by means of a cord passing round a small pulley F, and the wheel G, or H, or in any other convenient way; the spindle D is connected with the machine whose velocity is to be ascertained. In forming this connexion, we must be careful to arrange matters so, that when the machine is moving at its quickest rate, the angular velocity of the cup shall not be so great as to depress the spirits below C into the wider part of the tube. We are also, as in the figure, to have a scale of inches and tenths applied to AC, the upper and narrower part of the tube, the numeration being carried downwards from zero, which is to be placed at the point to which the column of spirits rises when the cup is at rest.

Then the instrument will be adjusted, if we mark on the scale the point to which the column of spirits is depressed, when the machine is moving with the velocity required. But, as in many cases, and particularly in steam-engines, there is a continued oscillation of velocity, in those cases we have to note the two points between which the column oscillates during the most advantageous movement of the machine.

Here it is proper to observe, that the height of the column of spirits will vary with the temperature, when other circumstances are the same. On this account the scale ought to be moveable, so that by slipping it upwards or downwards the zero may be placed at the point to which the column reaches when the cup is at rest; and thus the instrument may be adjusted to the particular temperature with the utmost facility, and with sufficient precision. The essential parts of the tachometer have now been mentioned, as well as the method of adjustment; but certain circumstances remain to be stated.

The form of the cup is adapted to render a smaller quantity

tity of mercury sufficient, than what must have been employed either with a cylindrical or hemispherical vessel. In every case two precautions are necessary to be observed: —First, That when the cup is revolving with its greatest velocity, the mercury in the middle shall not sink so low as to allow any of the spirits in the tube to escape from the lower orifice, and that the mercury, when most distant from the axis, shall not be thrown out of the cup. Secondly, That when the cup is at rest, the mercury shall rise so high above the lower end of the tube, that it may support a column of spirits of the proper length.

Now in order that the quantity of mercury, consistent with these conditions, may be reduced to its minimum, it is necessary—first, that if MM, fig. 1, is the level of the mercury at the axis when the cup is revolving with the greatest velocity, the upper part MMXY of the cup should be of such a form as to have the sides covered only with a thin film of the fluid; and secondly, that for the purpose of raising the small quantity of mercury to the level LL, which may support a proper height of spirits when the cup is at rest; the cavity of the cup should be in a great measure occupied by the block KK, having a cylindrical perforation in the middle of it for the immersion of the tube, and leaving sufficient room within and around it for the mercury to move freely both along the sides of the tube and of the vessel.

The block KK is preserved in its proper position in the cup or vessel XYZ, by means of three narrow projecting slips or ribs placed at equal distances round it, and is kept from rising or floating upon the mercury by two or three small iron or steel pins inserted into the underside of the cover, near the aperture through which the tube passes.

It would be extremely difficult, however, nor is it by any means important, to give to the cup the exact form which would reduce the quantity of mercury to its minimum; but we shall have a sufficient approximation, which may be executed with great precision, if the part of the cup above MM is made a parabolic conoid, the vertex of the generating parabola being at that point of the axis to which the mercury sinks at its lowest depression, and the dimensions of the parabola being determined in the following manner: Let VG, fig. 3, represent the axis of the cup, and V the point, to which the mercury sinks at its lowest depression; at any point G above V, draw GH perpendicular to VG; let n be the number of revolutions which the cup is to perform

form in 1″ at its quickest motion; let v be the number of inches which a body would describe uniformly in 1″, with the velocity acquired in falling from rest, through a height = to GV, and make GH $= \frac{v}{3 \cdot 14\, n}$. Then, the parabola to be determined is that which has v for its vertex, VG for its axis, and GH for its ordinate at G. The cup has a lid to prevent the mercury from being thrown out of it, an event which would take place with a very moderate velocity of rotation, unless the sides were raised to an inconvenient height; but the lid, by obstructing the elevation at the sides of the cup, will diminish the depression in the middle, and consequently the depression of spirits in the tube: on this account a cavity is formed in the block immediately above the level LL, where the mercury stands when the cup is at rest; and thus a receptacle is given to the fluid which would otherwise disturb the centrifugal force, and impair the sensibility of the instrument.

It will be observed, that the lower orifice of the tube is turned upwards. By this means, after the tube has been filled with spirits by suction, and its upper orifice stopped with the finger, it may easily be conveyed to the cup and immersed in the quicksilver without any danger of the spirits escaping, a circumstance which otherwise it would be extremely difficult to prevent, since no part of the tube can be made capillary, consistently with that free passage to the fluids which is essentially necessary to the operation of the instrument.

We have next to attend to the method of putting the tachometer in motion whenever we wish to examine the velocity of the machine. The pulley F, which is continually whirling during the motion of the machine, has no connexion whatever with the cup, so long as the lever QR is left to itself. But when this lever is raised, the hollow cone T, which is attached to the pulley and whirls along with it, is also raised, and embracing a solid cone on the spindle of the cup, communicates the rotation by friction. When our observation is made, we have only to allow the lever to drop by its own weight, and the two cones will be disengaged, and the cup remain at rest.

The lever QR is connected by a vertical rod to another lever S, having, at the extremity S, a valve, which, when the lever QR is raised, and the tachometer is in motion, is lifted up from the top of the tube, so as to admit the external air upon the depression of the spirits; on the other hand,

hand, when the lever QR falls, and the cup is at rest, the valve at S closes the tube and prevents the spirits from being wasted by evaporation.

It is lastly to be remarked, that both the sensibility and the range of the instrument may be infinitely increased; for, on the one hand, by enlarging the proportion between the diameters of the wide and narrow parts of the tube, we enlarge in a much higher proportion the extent of scale corresponding to any given variation of velocity; and on the other hand, by deepening the cup so as to admit when it is at rest a greater height of mercury above the lower end of the tube, we lengthen the column of spirits which the mercury can support, and consequently enlarge the velocity, which, with any given sensibility of the instrument, is requisite to depress the spirits to the bottom of the scale. Hence the tachometer is capable of being employed in very delicate philosophical experiments, more especially as a scale might be applied to it, indicating equal increments of velocity. But in the present account it is merely intended to state how it may be adapted to detect in machinery every deviation from the most advantageous movement.

IX. *Description of the Drawings of Mr.* Loeschman's *Patent Harmonic Piano-Forte.*

[Continued from last volume, p. 327.]

Note,—This description, in the specification which is enrolled in the Six Clerks' Office, is written upon the drawings annexed to the specification of Mr. L.'s patent. As it would exceed the limits of three, or indeed four of our plates, to have given the figures of sufficient size to have engraved the names of each part upon the drawings, we are compelled to omit these names in the plate; but to render the whole equally explanatory, we have put the references in this form. The reader must be aware that on this account it is not to be considered as an official copy, though it contains the substance of the specification, indeed the same words, but in a different form.

In the profile or longitudinal section of this instrument, (Plate IX, fig. 1, vol. xxxvii.) the following parts are the same as the instruments now in use: A the finger-keys, B the raised black keys for flats or sharps, C centre-piece of the keys, and D centre-pins; E the key-frame, F the bottom of the instrument, G the name-board, H the rest-pin block, and *h* the pins for the strings; *i* the leaver attached

tached to the keys for throwing up the hammers, *k* the sockets for guiding the levers, *l* is the receiver for the hammers attached to the keys, *m* the dampers, and *nn* its sockets fixed to the block *o* which supports the sounding-board *p*; *q* is the bridge for the strings. The upper movements, marked 1,2,3, are fastened with screws and small brass plates upon the standard block 4. These by the three left-foot pedals bring on the flats from the bass to the treble.

The three under movements, marked *a*, *b*, *c*, are fastened with screws, as the upper movements. The first of these three movements (*a*) is let into a groove in the standard block 4, under the upper movements.

The second movement (*b*) is under the standard block.

The third movement (*c*) is fastened on the bevel of the standard block. These bring on the sharps, by the three right-foot pedals, from the treble to the bass. These movements or pedals may be reversed, the same purpose will be answered.

The regulating screw 5, in the regulating board 6, is for regulating the hammers 8. 9 is a small double square of iron, one end of it is screwed to the standard block 4, the other end is screwed to the regulating board 6, to keep it steady: each hammer has a separate centre-pin fastened in a piece of brass marked 10. Two of these pieces of brass with the hammers are screwed to each of the six movements (1, 2, 3 and *a*, *b*, *c*) within the octave.

The keys are independent of the hammers, and the action. In the two profiles (fig. 2), which are taken on a plane parallel to fig. 1, but at the opposite ends of the movement; *s s s* are the springs acting on the ends of the respective movements 1, 2, 3 and *a*, *b*, *c*, the springs are fastened to the action stool 4; *p p* are the pedal irons for the same: these are fastened under the bottom of the instrument, and go through the bottom of the right and left side of the action-stool, as is described in the profiles; when therefore a right- or left-foot pedal presses the movement, a spring at the other end, belonging to the movement, will, (when the pedal is left off,) bring the movement in an instant to its former place.

Fig. 3 is a plan looking down upon the movements, when there are 6 of them or 6 pedals, 1, 2, 3 are the movements, the same as in the former figures, 8 the hammers, and 10 the pieces of brass supporting them, and affixed with the movements, the letters in the row 14 (which are supposed to be marked on the hammers) are the 12 standing or fixed notes as in our common compass within the octave, viz.

three

three sharps, two flats, and seven naturals: the letters in the upper row 15 are the 12 additional notes which I have introduced, the six sharps are obtained by the three right-foot pedals, the six upper flats are obtained by the three left foot pedals. We have, therefore, twenty-four notes in the octave with our common scale.

Each of the three right-foot pedals for the sharps bring on in addition, two sharps in the octave from the treble to the bass, as follows:

The first right-foot pedal brings on by the piece of brass marked in fig. 3	D* A* 10^{1st} 10^{1st}	When drawn towards the bass end.
The second......do....do...... By the brass	E* B* 10^{2d} 10^{2d}	
The third........do....do...... By the brass	F** C** 10^{3d} 10^{3d}	

In like manner, each of the three left-foot pedals for the flats, brings on in addition two flats, in the octave, from the bass to the treble.

The first left-foot pedal..........	Ab Db	When drawn towards the treble end.
The second	Gb Cb	
The third	Fb Bbb	

In fig. 4, the range of letters marked 14, are the 12 standing or fixed notes as in our common compass within the octave, viz. three sharps, two flats, and seven naturals; of the eight upper additional notes, the four sharps are obtained by two right-foot pedals, and the four flats are obtained by two left-foot pedals: we have, therefore, only twenty notes in the octave with our common scale, and they are obtained in the same manner as before described of fig. 3, with 6 pedals. But, 4 of the standing or fixed notes, viz. G, A, D, E, remaining unmoveable, being fastened to the standard block 4, as is shown also in fig. 1, whereby we cannot obtain F**, C**, Fb, Bbb. We may reverse the movements or the pedals to bring on the flats from the treble to the bass, and the sharps from the bass to the treble, by which contrivance we gain the same end.

In fig. 3, with 6 pedals marked 1, 2, and 3 movements; and in fig. 4, with 4 pedals marked 1, 2 movements, will be easily and clearly shown how 2 notes in each octave are fastened to each movement, and how the movements slide in brass plates under screw heads when used by the pedals.

Description of the Drawings of LOESCHMAN's *Patent Harmonic Organ.*

Fig. 5, of Plate IX, is a profile of the keys and movements, showing the new improvements; but the parts common to all organs being first pointed out will render what follows more explicit: these are, A the finger keys, B the key frame, 8 8 the stickers, which are double the number of those in the organs now in use, and D their sockets, E the back-falls for opening the valves of their respective pipes.

The front movements 1, 2, 3 above each other are about three-fourths of an inch above the keys, and they are fastened upon the standard block 4, in the same manner as described in the piano; but the sloped end of the three movements marked 5, rest behind upon the keys behind, and they are hinged to the front part of the three movements. The block 6, underneath the stickers 8, works in the centre-pin 7; it is fastened beneath the under socket D, and it rests upon the upper end of the movement 5, the lower end of the sticker 8 rests upon the centre block 6. The backfalls are fastened to the upper end of the sticker 8; therefore when a key is pressed down in front, the keys behind lift up the part of the movement 5, with the centre-block 6; and the sticker 8, with the backfall, which opens the pallet to give the desired note; but when more sharps are wanted, by pressing the right-foot pedal, the front movement with the hinged part of the movement 5, will shift itself to the back end of the centre-block 6, which will give the note that is wanted in the same manner as before, only that the front note is now silent.

The manner of this is shown in the plan, fig. 6, where 1 is the movement, and *k* a bar to which the movements 5 are hinged; when 1 is moved endways by a pedal under its screw-heads, it throws *k* away from it, under its screw-heads *l* by means of the brass rods *m m*. This motion shifts the movement 5, fig. 5, from beneath, one end of the centre-block 6, to the other.

The back movements act towards the front in the very same manner as the front movements which have been described.

Instead of the centre-block, No. 6, it may be made with springs placed in different ways, on a strong under-block, as is shown in elevation at X, fig. 7, and in the plan, fig. 8. The six movements may be placed together in front, above the keys, or behind the keys, the flats and sharps may also be produced in the same manner as in the pianos, the effects will be the same.

The

The springs and pedals to the organ act in the same manner as in the pianos. The pedals for the organ as well as for the piano may be fastened, if those notes made by pedals are wanted to be used for a length of time in playing, in the same manner as on harps. The mechanism in the organ and piano, for the flats as well as for the sharps, is so constructed that the pedal 3 must likewise bring on the notes belonging to pedal No. 2 and 1; and the pedal 2 must also bring on the notes belonging to pedal 1. This effect may also be obtained by the tread of the pedals.

Note.—In the specification, another large drawing of the harmonic organ is given. It describes a second method of performing the same movement, as above described: the chief difference is, that the stickers 8 8 are all arranged side by side in one row, instead of two; but as this method crowds the organ up very much, it has never been put in practice by the patentee: on this account, and that it would exceed the limits of our plate, we have omitted this second drawing; it contains nothing material which is not shown by the figures we have given.

X. *On Rail Roads. By a* CORRESPONDENT.

To Mr. Tilloch.

SIR, As the proposed railway from Sanquhar to Dumfries has been of late the subject of some conversation, it is hoped the following short account of that useful invention will not be unacceptable to some of your readers.

I am, sir,

Your most obedient servant,

Dumfries, July 2, 1811. X. Y. Z.

Rail-ways are roads of very easy inclination, having cast-iron rails, on which waggons, with wheels adapted to those rails, move.

These rails are usually about three feet long, and are rested at each end on stone, wood, or cast-iron.

The origin of this invention may be traced back to the year 1680. About that period, coal came to be substituted for wood as fuel in London and other places. The consequent consumption of Newcastle coal became so considerable, that the difficulty and expense of maintaining a great number of horses employed to convey the coals from the pits to the vessels, and the cost of maintaining the roads, gave rise to the introduction of waggon-roads, or wooden rail-ways. On these rail-ways, a horse could draw a waggon

gon of a large size, owing to the regular and easy descent with which the rails were laid. It was not until the year 1738 that this improvement was introduced at the Whitehaven collieries. Afterwards, attempts were made in different parts to introduce cast-iron instead of wooden railways, but, owing to the great weight of the waggons then in use, these attempts did not succeed.

About the year 1768, a remedy was contrived for the principal objection to cast-iron rail-ways; namely, the making use of several small waggons linked together, instead of one large one; thus diffusing the weight over a greater surface of the road, and consequently throwing less stress on any one part of it. Soon after the year 1797, they began to be constructed as branches to canals: since that period they have rapidly increased, and their great utility is now unquestionably established.

As on canals, *locks* are required in order to raise the vessels from a lower to a higher level, and *vice versâ;* so, on rail-ways, what are called *inclined planes* are often necessary to attain the difference of level.

These inclined planes are generally, compared with the rest of the rail-way, very steep. A perpetual chain raises and lowers the waggons. It is so contrived, that the waggons disengage themselves the moment they arrive at the upper or lower extremity of the inclined plane. In some cases, the laden waggons descending serve as a power to bring up the empty ones; but where there is an ascending as well as a descending traffic on the rail-way, steam-engines, water-wheels, or other machines to answer the same purpose, are used. At Chapel le Frith, there is an inclined plane about 550 yards long, so that the chain extended is, of course, more than double that length.

Most rail-ways of considerable extent require the use of this species of machinery for attaining the difference of level requisite, more particularly in cases where minerals form any considerable part of the traffic. On the proposed rail-way between Glasgow and Berwick, several inclined planes will be required; the summit of that rail-way being 753 feet above the level of the end of Berwick quay.

The waggons are constructed on various plans, and are probably, in most cases, far from the degree of improvement of which they are susceptible. But, with all their disadvantages, the following facts will evince the great saving of animal force to which rail-ways gave rise.

1. With 1¼ inch per yard declivity, one horse takes downward three waggons, each containing two tons.

2. In

2. In another place, with a rise of $1\frac{6}{10}$ inch per yard, one horse takes two tons upwards.

3. With 8 feet rise in 66 yards, nearly $1\frac{1}{4}$ inch per yard, one horse takes two tons upwards.

4. On the Penrhyn rail-way, (same slope as the above,) two horses draw downwards 4 waggons, each containing one ton of slate*.

5. With a slope of 55 feet per mile, one horse takes 12 to 15 tons downwards, and 4 tons upwards, and all the empty waggons †.

6. At Ayr, one horse draws on a level 5 waggons, each containing a ton of coal.

7. On the Surry rail-way, one horse, on a declivity of one inch in ten feet, is said to draw 30 quarters of wheat ‡.

Other actual cases might be given, but these will suffice to show the great saving of animal force.

From these cases, and the known laws of mechanics, we may perhaps safely infer, that where the apparatus is tolerably well constructed, and the slope 10 feet per mile, one horse may draw 5 tons upwards, and 7 tons downwards. Now, if I am rightly informed, horses at present draw from Sanquhar to Dumfries only about $9\frac{1}{2}$ cwt. of coal at an average each. But say half a ton; then, on the slope stated above, *one horse* would, taking weight upward, do the work of *ten* on the turnpike road, and downward, of *fourteen*. Hence, in this point of view, it may be said that a rail-way would bring the coal-mines ten times, at least, nearer to Dumfries than they are at present.

The principal rail-ways in England and Wales, a short time ago, were—the *Cardiff* and *Merthyr*, $26\frac{3}{4}$ miles long; runs very nearly by the side of the Glamorganshire canal.

The *Caermarthenshire.*—In the deep cuttings for this rail-way, several unknown veins of coal were discovered, and some of lead ore.

The *Serhowry*, 28 miles, in the counties of Monmouth and Brecknock.——The *Surry*, 26 miles.

The *Swansey* and *Oystermouth*, $7\frac{1}{2}$ miles; and many others, as branches to canals. Since these were executed, many have been added, and they are daily increasing in number.

In Scotland they have been long used about some of the coal-works, and are now fast increasing. A public rail-way is now nearly completed between Kilmarnock and the Troon harbour.

* See Plymley's Agricultural Report of Shropshire.
† Repertory of Arts, &c. vol. iii. 2d series.
‡ Malcolm's Agricultural Report of Surry.

 XI. *Ob-*

XI. *Observations on the Expansion and Contraction of Water.* By William Crane, *F.R.M.S. Edin.*

Every deviation from the general effects of caloric forms an important subject for investigation, and claims the attention both of the student and the philosopher. Amongst these curious and interesting facts, that water at the temperature of 40° has its maximum density, and on being reduced to a lower degree begins to expand until it is converted into ice, has given rise to many hypotheses and theories respecting its cause. Some have supposed this to be owing to the contraction of the vessel in which the water is contained. One of the most strenuous supporters of this opinion is Mr. Dalton, who says, "it is only apparent;" although the experiments both of Dr. Hope and Count Rumford were made with the greatest care and precision, as were also those of Lefevre Gineau. The result of Mr. Dalton's experiments, when a glass vessel is employed, is certainly very much in favour of what he maintains, as, according to the tables in Dr. Thomson's Chemistry, the contraction of the glass and the expansion of the water coincide; yet this is not the result of the experiments made upon water contained in different vessels, as in brown earthen ware, queen's ware, iron, copper, &c. The coincidence, therefore, as the doctor observes, is only apparent; for the other bodies deviate as their expansion increases. Mr. Leslie, in his Inquiries upon the Nature, &c. of Heat, seems to be nearly of the same opinion. Others have adopted the idea of its arising from a peculiar arrangement of its particles which observe a certain polarity, as is shown by the position of its crystals; and this was the opinion of the illustrious Dr. Black.

As water is a body the particles of which possess great mobility among themselves, and the shape of a body that moves with the greatest ease being a sphere; let us consider that this is the form of a particle of water when at the 40° and above, or, according to Mr. Dalton*, at the 36°, which he estimates to be its maximum density. In the following part of this paper I prefer the 40°, as between that and 39° is the point agreed to by the majority of writers, and which agrees with the experiments I have made. The difficulty of proving this to be the shape of an atom of water is perhaps in some measure removed by considering the figure which a globule of water assumes when thrown

* Dr. Henry's Elements of Chemistry.

upon a hot iron. Haüy* observes that this was the opinion of Descartes, who thus endeavoured to account for the formation of the six radii which are observed to form a floccule of snow. But Descartes says, when treating on the shape of the particles of water, "Deinde† suppono exiguas illas partes, quibus aqua componitur, longas, læves et lubricas esse anguillarum parvularum instar, &c.:" and that they only assumed the shape of a sphere when converted into vapour, from the rapid motion into which they are thrown, in these words; "Sed‡ contra quum vaporis formam habent, agitatio illarum adeo est concitata, ut celerrimè rotentur in omnes partes, et eadem opera in longitudinem suam porrigantur; unde fit ut singulæ illarum reliquas suis similes, irruptionem in parvas sphærulas, quas describunt, molientes, arcere atque abigere possint, &c." Hence he had recourse to this reasoning to account for the formation of the radii already mentioned, as his theory respecting the particles of water could not be adapted to this phænomenon.—But to return to our subject.

Then, at the degree above mentioned, I would say that the particles of water are in contact only at certain points; but from the caloric, granting it to be a fluid, filling up the interstices, their mutual affinity is prevented from acting so forcibly as to change their figure. In illustration of this, we may take a pile of balls, as a rough comparison, each ball having for those around it a strong affinity, and which are prevented from acting upon each other, or running into a solid mass, by sand or some substance being poured into the various crevices, which nevertheless does not prevent their touching in certain points. But as by the reduction of temperature part of the caloric is withdrawn, which being interspersed throughout the water, as just explained, prevented these particles from affecting each other, the affinity they exert among themselves now begins to take place, and their shape becomes altered from that of a sphere to some other figure. Hence, as a sphere contains the greatest quantity of matter under the least given superficies, the superficial contents of these atoms will be increased in proportion as they deviate from that form.

Although they are thus enabled to act upon each other, still they attract around them a quantity of caloric, by means of which they are kept so far separate as to remain

* Haüy's Natural Philosophy; trans. by Dr. O. Gregory.
† *Renati des Cartes Specimina Philosophiæ:* Amstelodami, anno cIɔ Iɔc lxxii.
‡ Ibid.

in a fluid state. But owing to the reciprocal affinity of these moleculæ this attraction is very feeble, and on suddenly shaking the water they rush together, forming a crystalline mass, setting free the caloric they held around them, causing by that liberation a rise in the thermometer. In the same manner we can bring so near as to touch, globules of mercury, which have been previously moistened with water, without their running into one homogeneous mass; but giving the vessel in which they are placed a sudden shake, they become united, parting with the water each had attracted around it. This experiment is easily shown by throwing quicksilver upon any flat surface that has had some water poured upon it; then gently pushing the globules of mercury, so as just to touch each other, they will not unite, owing to the pellicle of water which surrounds each. Upon the vessel being agitated, an union instantly takes place.

The next remarkable occurrence is the great and sudden expansion that takes place upon the water being converted into ice. I would now suppose that these atoms have reached their maximum of expansion, or that they deviate in the greatest possible degree from their spherical shape, and assume probably that of the primitive crystal. For after having obtained the primitive crystal of any body, we have, if we continue the chipping and diminish it ever so much, always the same figure. Again, if we apply heat, from the 32° there is observed a contraction, until the thermometer rises to the 39° or 40°, owing to these integrant molecules of the crystals again assuming the spherical form: after this the water begins to expand, which I should imagine is owing to the caloric gradually forcing these spheres further apart, and, if continued, separates them beyond the limit of the attraction they exert amongst themselves. These atoms, being lighter than air, fly off in a state of vapour; and as they are now out of the sphere of each other's attraction, they are enabled to attract more forcibly around them the particles of caloric; and hence the increase of capacity for caloric which is observed to take place when water is converted into vapour.

In the above paper, the words *contact* and *touch* have been frequently employed: these terms are not to be understood in an abstract sense, but merely to denote that the particles of matter approach each other extremely near;—as in the experiment on the globules of mercury it is said they are placed so near as to touch. That this is not the case is evident,

evident, for they are separated by the pellicle of water around each. Lavoisier, in his Chemistry, says, that the particles of the hardest bodies are not in actual contact. If that were the case, it is probable that their cohesive affinity would be so powerful as not to be affected by caloric.

XII. *Description of Improvements in a Mathematical Dividing Engine. By Mr.* J. ALLAN*.

SIR, I BEG leave to send to you, herewith, for the inspection of the Society of Arts, &c. a model of my improvement on the mathematical dividing engine which I have lately made, containing that part which differs in principle from those made by the late Mr. Ramsden and others; the drawings or engravings of which are, I suppose, in the Society's possession. I therefore am of opinion the Society will think that the wooden wheel I have sent with the moveable ring on its edge, will be sufficient to demonstrate its good effect in correcting the teeth or rack where the screw acts. You will please to observe, that it is cut by a screw-cutter, and it is required to go many times round the engine before the teeth are full. To effect this, I reversed the moveable ring not less than twenty times, so that I have not the least doubt of the one ring having corrected the other to a degree of perfection which had not hitherto been obtained in engines.

This simple, easy, and correct way of making engines, may be applied with great advantage to circular instruments, for the purposes of astronomy and land-surveying. If the Society will do me the honour to appoint a committee to view the engine itself, I will demonstrate its effects.

I am, sir,

Your very humble servant,

JAMES ALLAN,
Divider of Mathematical Instruments.

No. 12, Blewit's Buildings,
Fetter-Lane, Nov. 20, 1809.

To C. Taylor, M.D. Sec.

Mr. Allan's *Description of his Mathematical Dividing Engine, and his Method of forming it.*

My engine is of bell-metal, thirty inches in diameter. I turned a brass ring about three-sixteenths of an inch thick,

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, vol. xxviii.——The Society voted the gold medal to Mr. Allan for this communication.

and

and fitted in on the underside of the above bell-metal wheel, which I made fast by twenty-four rivets; I then fixed in the axis, and turned the wheel and ring together on the lathe, as nearly as possible to the required shape on its own axis. This being done, and having mounted it on its own stand, where it now acts, I fixed a tool, with an adjustment to turn the edge of the bell-metal wheel where the uppermost or moveable ring of the same thickness as the other is fitted on; for if the circle, where the moveable ring fits the bell-metal, is not turned as true as possible, (which cannot be done properly by any other means than by a fixed tool,) the moveable ring will not reverse correctly. When this was done, I fitted on the moveable ring. I then divided the lower under ring into twenty-four parts, for the screws which keep the rings together. I also divided it into four parts for the steady pins, the holes of which I made by an upright drill fixed and adjusted for the purpose. I then cut two opposite divisions, in order to reverse the uppermost ring correctly, which were my guide in broaching for my steady pins, and which I did with a broach to a stop fixed on it. In broaching I reversed the moveable ring many times, taking care at the same time that my opposite divisions were correct.

My first idea was to have two wheels or circles, acting on the same centre, so as to constitute a double edge, to afford me an opportunity to reverse in the act of cutting the rack or teeth; but I thought the method in which I have done it would with care be equally correct. Either of the methods comes to the same point, and I preferred the way I have employed, thinking it the least expensive. By this self-correcting method, instruments may be made for astronomical purposes, racked and divided on their own centre, and if carefully done would border on perfection itself, consequently I consider it to be the greatest improvement ever made in the art of dividing. I call it self-correcting, because every time it is reversed in cutting the teeth, the screw has a fresh opportunity to correct errors insensible to the eye.

I have well considered the subject, and think that a circle of twelve inches diameter, made on this principle, would measure angles equally if not more accurately than astronomical instruments divided by engines, or by any other methods hitherto used by instruments of any size. It is therefore my opinion, that the supposed necessity of making very large circles, for the sake of obtaining correct divisions, will be done away.

JAMES ALLAN.

SIR,—

SIR,—THE method you have taken to produce a perfect equal racking, for the constructing an accurate dividing engine, is the greatest advance towards perfection that has been communicated to the public within my knowledge, and I believe it to be a method never before practised in this country. It is applicable to the construction of machines of any dimensions, that mathematical or nautical instruments can be graduated by.

It is my belief, that the greater number of the machines now in use, are far short of the perfection they are reputed to have.

I am, sir, your humble servant,

Piccadilly, Jan. 8, 1810. M. BERGE.

Reference to the Drawing of Mr. Allan's *Improvement on the Dividing Engine of Ramsden.* Pl. II.

The dividing engine invented by Mr. Jesse Ramsden, and for which he received the reward of the Board of Longitude in the year 1775, is minutely explained in a quarto pamphlet, published by order of the Commissioners of Longitude; also, in the article *Engine*, in Dr. Rees's New Cyclopædia, as well as some other works of a similar nature; it therefore becomes unnecessary for the Society to give any more of Mr. Allan's engine in their drawings than is explanatory of the improvement, the engine being used in the same manner as Ramsden's: this part is the great circle upon which the arch to be divided is placed, and the circle turned about a determinate quantity at each division, by means of a screw whose threads engage fine teeth, cut around the periphery of the circle. The improvement by Mr. Allan consists in the method of cutting or racking these teeth, to ensure their being perfectly of equal size in all parts of the circle.

The plan, fig. 1, in Plate II, represents the upper surface of a bell-metal circle mounted upon an axis, A, fig. 2, and its surface made truly plane, and perpendicular to the axis: the section shows the figure of the axis, and the central ring B, to give the greatest strength to the circle; C is a section of a portion of the frame of the engine; and D a socket into which the axis A is fitted; the circumference of the large circle is turned to such a figure as to receive a ring of brass, *a*, fig. 3, which is united firmly to it by a number of pins, one of which is shown in the figure. Upon this ring, a second *b* is placed, the two making the same thickness as the circle. The inside of the ring *b*, and the outside

side of the bell-metal circle, are fitted to each other with the utmost accuracy, and great care taken to turn the said truly fitting concentric with the axis of the circle; the brass rings *a* and *b* are held together by twenty-four screws, as shown in the plan; and a groove, corresponding to the curvature of the screw which moves the circle, is turned in the outside of the two: in this state the racking of the teeth is performed by a screw similar to that afterwards used to turn the circle to its divisions, but notched across the threads so that it cuts like a saw, when pressed against the circle and turned round, and removes the metal from the spaces between the teeth, which are by this means formed around the edge of the circle; when this has been performed all round, two fine lines are drawn across the brass and bell-metal circles, diametrically opposite each other; the twenty-four screws are then withdrawn, and the upper brass ring turned exactly half round, which is determined by the lines before mentioned; and by this means the teeth of the circle are divided into two thicknesses, and being put together again in opposite directions, if any error arose in racking the teeth, it would be shown by the upper and lower halves of the teeth not coinciding when reversed, and by racking them while reversed the screw would cut away the inequalities, and make all the teeth of the same size and distance from each other: this reversing the teeth is performed several times, till the teeth are brought to a perfect equality in all parts of the circle; four steady pins are accurately fitted into the two rings to hold them together in any of the positions in which they have been racked together, and it is upon these the dependence is placed for the coincidence of the teeth, the twenty-four screws being merely to hold them fast together, and fitted rather loosely in their holes, that they may not strain the steady pins.

XIII. *Extract from a Memoir on Vegetable and Animal Analysis.* By MM. Gay-Lussac and Thenard.

Read at the Institute 15th January, 1810.*

When we had conceived the project of analysing animal and vegetable matters, the first consideration which presented itself to our serious attention was to transform, by means of oxygen, the vegetable and animal substances into water, carbonic acid, and azote. It was evident that if we

* *Annales de Chimie*, tome lxxiv. p. 47.

could

could succeed in operating the transformation so as to collect all the gases, this analysis would be accomplished with very great precision and simplicity. Two obstacles presented themselves: one was to burn completely the hydrogen and the carbon of these substances, and the other to operate the combustion in close vessels.

We could expect to surmount the first difficulty only by means of the metallic oxides which easily give up their oxygen, or by the hyper-oxygenated muriate of potash. Some experiments soon made us give the preference to the above salt, which succeeded beyond all expectation. It was not quite so easy, however, to overcome the latter difficulty; for we could not attempt combustion in a retort full of mercury. To prevent the matter from being burnt, the retort must have been broken: it became necessary to find an apparatus, therefore, in which we might—

1. Burn portions of substance so small as not to fracture the vessels.

2. To make a great number of successive combustions, in order that the results might be perceptible.

3. To collect the gases as they were formed.

We now exhibit to the class an apparatus of the above description. It is formed of three distinct pieces: one is a very thick glass tube, closed at its lower extremity by the blow-pipe, and open at its upper end, about two decimetres in length, and eight millimetres in breadth; it has laterally five centimetres from its aperture a very small tube also of glass, which is soldered to it, and which resembles that which we should adapt to a retort for receiving the gases. The other piece is a copper ferule into which we insert the open extremity of the large glass tube, and with which it is united by means of a mastic which melts only at 40°. The last piece is a peculiar kind of stopcock, in which the whole merit of the apparatus consists. The key of this stop-cock has no hole through it, and turns in every direction without giving vent to the air: there is simply about the middle of it a cavity capable of receiving a small pea: but this cavity is such that being in its upper position, it corresponds to a small vertical funnel which penetrates the socket, and of which it forms in some measure the extremity of the beak, and which when brought back to its lower position communicates with, and is a continuation of, the body of the stop-cock, which is hollow, and is screwed to the ferule. Thus, when we put small fragments of any matter into the funnel and turn the key, the cavity is soon filled, and carries the matter into the body of the stop-cock, from

from which it falls into the ferule, and from thence to the bottom of the glass tube.

If this substance, therefore, be a mixture of hyper-oxygenated muriate of potash and of vegetable substance in proper proportions, and if the lower part of the glass tube be sufficiently warm, it will briskly take fire: the vegetable substance will then be instantaneously destroyed and transformed into water and carbonic acid, which will be collected over mercury with the oxygen gas issuing by the small lateral tube.

In order to execute this operation easily, we may conceive that it is necessary that the matter be detached entirely from the cavity and fall to the bottom of the tube. For this purpose it is made up into small balls, as will be mentioned presently: we may also conceive that it is necessary to inquire what is the proper quantity of hyper-oxygenated muriate for burning completely vegetable substance. We must even take the precaution to employ at least one half more than this substance requires, in order that the combustion may be complete.

But of all the inquiries which ought to precede the operation, the most important is the analysis of the hyper-oxygenated muriate employed; for upon this all the calculations of the experiments are in a great measure founded.

All this being well understood, it will be easy to analyse a vegetable substance with the hyper-oxygenated muriate. The substance is to be ground on a porphyry slab with the greatest care, as also the hyper-oxygenated muriate; quantities of both are to be weighed in very accurate scales; they are to be well mixed, moistened, and rolled into cylinders; these are to be divided into small balls, which are to be exposed to a boiling heat in order to render them as dry as the original materials were. If the substance to be analysed is a vegetable acid, it is to be combined with lime or barytes before mixing it with the hyper-oxygenated muriate: the salt which results is to be analysed, and an account is to be taken of the carbonic acid which remains united to the base after the experiment: lastly, if the substance to be analysed contains some bodies which are foreign to its nature, they are also to be taken account of.

Thus we know accurately that a given weight of this mixture represents a known weight of hyper-oxygenated muriate, and of the substance which we wish to analyse.

Now in order to finish the operation, nothing more is requisite than to make the bottom of the tube red hot; to drive off all the air by means of a certain number of balls, which

which we do not weigh, and which we throw in one after another; then to decompose in the same manner a weight of them precisely determined, and carefully to collect all the gases in flasks full of mercury and gauged beforehand.

If all the flasks are of the same capacity, they will be filled with gas by equal weights of mixture; and if we examine these gases, we shall find them perfectly identical, an evident proof of the extreme accuracy of this method of analysis.

The tube ought to be kept during the whole operation at the highest degree of heat which it can support without melting, in order that the gases may not contain any oxy-carburetted hydrogen gas. In all cases the analysis ought to be performed over mercury. This is a proof to which it is indispensable to subject them: for this purpose it is sufficient to mix them with one-fourth of their volume of hydrogen, and to pass an electric spark into them. As they contain a great excess of oxygen, the hydrogen which we add, and of which an account must be kept, burns as well as the whole oxy-carburetted hydrogen which they may contain; and we thus acquire the certainty that they are no longer formed of any thing but carbonic acid and oxygen, which must be separated by potash.

But this necessity of raising the temperature obliges us on the other hand to take some precautions in order that the stop-cock may not be heated. With this view the glass tube is passed through a brick to which it is fastened with clay, and which at the same time gives solidity to the apparatus: besides this, we must solder to the body of the stop-cock a small hollow cylinder in which water is put, or rather ice.

We have thus all the necessary data for knowing the proportion of the principles of the vegetable substance: we know how much of this substance has been burnt, since we have the weight of it to a demi-milligramme: we know how much oxygen is wanted to transform it into water and into carbonic acid, since the quantity of it is given by the difference which exists between that contained in the hyper-oxygenated muriate and that contained in the gases; lastly, we know how much carbonic acid is formed, and we calculate how much water ought to be formed.

By following the same order of analysis, we also succeed in determining the proportion of the constituent principles of all the animal substances. But as these substances contain azote, and as there would be a formation of nitrous acid gas, if we employed an excess of hyper-oxygenated muriate

muriate in order to burn them, we need only employ a quantity sufficient for reducing them completely into carbonic acid gas, oxy-carburetted hydrogen, and azote, of which we perform the analysis in the eudiometer with mercury by the common methods, and from which we may conclude exactly that of the animal substance itself.

The method in which we proceed to the analysis of vegetable and animal substances being exactly known, we can tell what quantity of it we decompose without any fear of weakening the confidence which we ought to have in our results. This quantity rises at most to six decigrammes: besides, if there was the smallest doubt as to their exactness, we could get rid of it upon recollecting that we fill successively with gas, two and sometimes three flasks of the same capacity; that these gases are identical, and always proceed from one and the same weight of materials.

We might add, that the exactness of any analysis consists rather in the accuracy of the instruments, and of the methods which we employ, than in the quantity of matter upon which we operate. The analysis of the air is more exact than any analysis of the salts, and yet it is performed upon 2 or 300 times less matter than the latter. This is because in the former, where we judge of weights by volumes which are very considerable, the errors which we may commit are perhaps 1000 or 1200 times less perceptible than in the latter, where we are deprived of this resource. Now as we transform into gas the substances which we analyse, we bring our analyses not only to the certainty of the common mineral analyses, but to that of the most precise mineral analyses; more particularly as we collect at least a litre of gas, and as we find even in our way of proceeding the proof of an extreme exactitude and of the most trifling errors.

We have already methodically analysed, with all the precautions just mentioned, sixteen vegetable substances; viz. the oxalic, tartarous, mucous, citric and acetic acids; turpentine in resin; copal, wax, olive oil; sugar, gum, starch, sugar of milk, oak and ash wood, and the crystallizable principle of manna. The results which we obtained seem to us to be of the first rate importance, for they led to three very remarkable laws to which the composition of vegetables is subjected, and which may be thus expressed:

First Law.—A vegetable substance is always acid when the oxygen is to the hydrogen in a greater proportion than in water.

Second Law.—A vegetable substance is always resinous, oily,

oily, or alcoholic, &c. when the oxygen is in a less proportion to the hydrogen than in water.

Third Law.—Lastly, a vegetable substance is neither acid nor resinous, and is analogous to sugar, gum, starch, sugar of milk, to the ligneous fibre, to the crystallizable principle of manna when the oxygen is in the same proportion as in water.

Thus, supposing for a moment that hydrogen and oxygen were in the state of water in vegetable substances, which we are far from thinking is the case, the vegetable acids would be formed of carbon, water and oxygen in various proportions.

The resins, the fixed and volatile oils, alcohol and ether, would be formed of carbon, water and hydrogen, also in various proportions.

Lastly, sugar, gum, starch, sugar of milk, the ligneous fibre, the crystallizable principle of manna, would only be formed of carbon and water, and would only differ in the greater or less quantities which they contained.

This may be shown by citing various analyses of acid and resinous substances, and of substances which are neither acid nor resinous.

One hundred parts of oxalic acid contain:

Carbon	26·566
Oxygen	70·689
Hydrogen ..	2·745
	100

Or, rather

Carbon	25·566
Oxygen and hydrogen in the proportions in which they exist in water	22·872
Oxygen in excess	50·562
	100

One hundred parts of acetic acid contain:

Carbon	50·224
Oxygen	44·147
Hydrogen ..	5·629
	100

Or, rather

Carbon	50·224
Oxygen and hydrogen in the proportions in which they exist in water	46·911
Oxygen in excess	2·865
	100

The oxalic acid contains, therefore, more than half its weight of oxygen in excess, in proportion to the hydrogen, whereas in the acetic acid this excess is not quite three centiemes.

These two acids occupy the extremes of the series of the vegetable acids: of all the acids the one is the most, and the other is on the contrary the least oxygenated: this is the reason why it requires so much nitric acid to convert sugar and gum, &c. into oxalic acid; and this is the reason, on the contrary, that so many vegetable and animal substances produce so easily acetic acid in a great many circumstances, and that wine in particular is changed into vinegar without any intermediate acid being formed; a phænomenon which had not been hitherto explained, because vinegar has been regarded as the most highly oxygenated of all the acids.

One hundred parts of common resin contain:

Carbon	75·944
Hydrogen and oxygen in the proportions in which they exist in water	15·156
Hydrogen in excess	8·900
	100

One hundred parts of olive oil contain:

Carbon	77·213
Hydrogen and oxygen in the proportions in which they exist in water	10·712
Hydrogen in excess	12·075
	100

One hundred parts of crystallized sugar contain:

Carbon	40·704
Oxygen	52·101
Hydrogen	7·105
	100

Or, rather

Carbon	40·194
Hydrogen and oxygen in the proportions in which they are in water	59·806
Oxygen in excess	0·
Hydrogen in excess	0·
	100

One hundred parts of ash wood contain:

Carbon	51·192
Oxygen	42·951
Hydrogen	5·857
	100

Or, rather

Carbon	51·192
Hydrogen and oxygen in the proportions in which they are in water	48·808
Oxygen in excess	0·
Hydrogen in excess	0·
	100

These results prove a very important fact: viz. that water *per se* or its principles are seized upon by the vegetable in the

the act of vegetation: for, all the vegetables being almost entirely formed of ligneous fibres and mucilage, which contain oxygen and hydrogen in the same proportions as water, it is evident that when carried into the substance of the vegetable it is then combined with carbon in order to form them.

If, therefore, it were in our power to unite these two bodies in every given proportion, and to bring their molecules together in a proper manner, we should certainly make all the vegetables which hold the middle rank between the acids and the resins, such as sugar, starch, the ligneous fibres, &c.

Among the animal substances, we have only as yet analysed fibrine, albumen, gelatine, and the caseous substance.

It results from our analyses, that in these four substances, and probably in all analogous animal substances, the hydrogen is in a greater proportion to the oxygen than in water; that the greater the excess of hydrogen, the greater is the quantity of azote which they contain also; that these two quantities are almost both in the same proportion as in ammonia, and that it is probable that this proportion, which we nearly approach, does actually exist: the more, probably, because we always find a little too much hydrogen, and as all the errors which we can make tend to increase the quantity of it. We shall judge of this by the two following analyses.

One hundred parts of fibrine contain:

Carbon	51·675
Hydrogen and oxygen in the proportion in which they exist in water	26·607
Hydrogen in excess	5·387
Azote	16.331
	100

One hundred parts of caseous matter contain:

Carbon	57·190
Hydrogen and oxygen in the proportion in which they exist in water	18·778
Hydrogen in excess	5·660
Azote	18·352
	100

Admitting this report to be correct, these substances would correspond, with respect to the rank which they ought to hold among the animal substances, to the rank occupied by sugar, gum, ligneous fibre, &c. among the vegetable

 substances:

substances: for in the same way as hydrogen and oxygen, the gaseous principles of the former, may be reciprocally saturated and form water; in the same way hydrogen, oxygen and azote, the gaseous principles of the latter, may be also reciprocally saturated and form water and ammonia: so that the carbon, which is the only fixed principle which all of them contain, does not possess any property relative to that saturation. If we are guided by analogy, we might compare under this point of view the animal acids with the vegetable acids, and the animal fats (if there are any which contain azote) with the resins and vegetable oils: consequently the hydrogen could not be in a sufficient quantity in the uric acid, for saturating the oxygen and azote which this acid contains, or to form water and ammonia by combining with these two bodies, and the contrary would take place in the animal fats. A numerous train of consequences may certainly be drawn from all the preceding results; but we shall defer the further consideration of the subject till a future occasion.

XIV. *On the Influence of the Atmosphere in certain Diseases. By* THOMAS FORSTER, *Esq.*

SIR, IN your last Number, you did me the favour to print some observations on the effects produced, by varieties in the state of the atmosphere, on M. De Luc's electric column: allow me to trouble you with some further reflections on this subject, which I request your insertion of, rather with a view to excite the attention of others to several circumstances connected with this phænomenon, than because I think my observations of any value in themselves.

That a variety of disorders are produced by the influence of the atmosphere is, I believe, very generally admitted: such a supposition is indeed founded on reason; for if a great number of persons, at different places, of divers ages, and of various constitutions and habits of life, become at the same time the subjects of a very similar kind of disorder, it is rational to attribute their malady to some general cause then prevailing. The recurrence of such disorders at certain times of the year, or on the return of particular kinds of weather, naturally suggests an idea that such cause resides in the air. Thus, for example, such disorders as are usually called cholera morbus, colica, &c. usually occur in August and September; many persons are subject to cutaneous

taneous eruptions every spring; numbers of people are much affected by the prevalence of particular kinds of weather, and the stomach often feels uneasy before thunder-storms. Europeans, whose health suffers deterioration from removal into tropical climates, are affected with different kinds of disorders in different countries situated in nearly the same latitudes, and in which the mean heights of the thermometer may be nearly the same. These and many similar observations which I am about to relate, have induced me to think that those disorders which occur apparently in consequence of atmospheric influence, are not caused chiefly by the heat or frigidity, dampness or drought of the air, nor by changes from one to another of these states; but are produced, or rather excited, by the operation of certain peculiarities in the electric state of the atmosphere with which we are at present but little acquainted. In a recent publication, Mr. Abernethy has shown that, though there may be pre-established tendencies in the constitutions of various persons to peculiar forms of disease,—nevertheless most, if not all those complaints which are termed local, as well as those usually denominated constitutional, are excited by a disordered state of the system in general, which seems to consist in a combination of nervous irritability and weakness with disorder of the digestive organs in particular *.

That the opinions advanced by this ingenious and eminent gentleman were correct, I should have been ready to admit on the authority on which they rest, even had I not been previously induced to think so, from the accidental occurrence of a particular circumstance, by which my attention was strongly excited towards disorders of the chylopoietic organs. But still the various causes which operate to the production of such a state of the system remain to be reflected on. The various evil habits of artificial life; such as sedentary occupations, bad air, stimulating diet taken in too great quantity, the too free use of spirituous and fermented liquors, and indeed excessive indulgence and luxury of every kind, may cooperate to produce such a state of disorder, which may be aggravated, and often at first caused, by the great influence of the mind upon the digestive functions. But we must recollect that the existence of any one of these causes of disorder must render our bodies more susceptible of the operation of any of the rest;

* I refer your readers to "Surgical Observations on the constitutional Origin and Treatment of Local Diseases, &c. by John Abernethy," &c. London, 1809.

and

and that when a state of weakness and irritability is induced by the joint influence of a great many, we shall be rendered more liable to be affected by peculiarities in the state of the atmosphere than when living natural lives and in strong health. That such varieties of weather actually do become powerful agents in the production of disorders, every day's experience must teach us, but the manner and extent of their operation appears at present less easily demonstrable: it may deprive persons already weak of a portion of their electricity, and thus diminish the energies of the brain; or, the atmospheric electricity being unequally distributed, or propagated downwards in pulsations, the electric fluid may be irregularly distributed in our bodies, and this may cause an irregularity of function. Such a state of the air would be indicated by the irregular action of De Luc's electric column, and the multiform appearances of the *cirrus* cloud occasionally attended by the other modifiations, the appearance of meteors*, &c. And I may remark that, during the prevalence of such kind of weather last autumn, when the action of De Luc's electric column was very irregular, some of my medical friends informed me that bilious disorders with hypochondriacism were remarkably numerous.—I must now conclude with apologizing for detaining you so long on this subject, and with saying that, if the opinions advanced appear novel and useless, I desire the reader not to forget my object, which is to excite the attention of superior minds to my own, to this interesting, and, I believe, important subject.

I remain, sir, yours, &c.

Clapton, July 20, 1811. THOMAS FORSTER.

XV. *Memoir on the Action of Elastic Fluids upon Meat. By M.* HILDEBRAND†.

BEING convinced that experiments and observations upon the spontaneous analysis of organic bodies are very instructive, and throw considerable light upon the nature of those bodies, I have always endeavoured in my course of chemistry to make my pupils understand how the elastic fluids, surrounding bodies subjected to putrefaction, can operate in retarding or hastening that natural process. With this view I have been inuced to extend my researches upon this subject,

* The flaccidity, dryness, closer contact, and diminished size of the hairs on the head, during many disorders, may not be unworthy of notice.

† *Journal de Gehien.*

subject, and I now venture to present a course of experiments which I flatter myself will conduce in some measure to the advancement of science.

I must previously observe, 1st. That I constantly employed the same kind of flesh, viz. beef, in order to be certain that the differences noted could only proceed from the action of the different elastic fluids; that this flesh was taken from the same animal, and even from the same muscle; that it did not contain the least fat, but only the muscular fibres; that the pieces were all of equal size, cut into parallel strips, and proportioned to the size of the vessels. 2d. That I always employed the flesh of an animal that had been killed two hours. 3d. That I preserved only the last portions of disengaged gas, in order to have it free from any mixture of the atmospheric air contained in the receiver, and that I always made use of the gas a little while after it was prepared. The atmospheric air was taken from a spacious open garden. 4th. I placed the vessels in a room inaccessible to the sun, the windows of which fronted the north and were very small, so as to exclude the action of light, which I propose to examine at some other opportunity. The temperature of this room is cool in summer, and in winter it is above the freezing point; if, however, frost was to be apprehended, I removed the vessels into my study, which was by the side of the room.

I employed three methods of subjecting the flesh to gas; and to avoid repetition, I shall designate them as follows: 1st. above water, 2d. above mercury, and 3d. in an empty bottle.

1st. (Experiment over water.) I filled cylindrical receivers which contained from 92 to 98 cubic inches Paris measure, over the pneumatic trough. I then introduced into them pieces of the flesh 3½ inches long, 1 inch broad, and ¾ of an inch thick. To support the flesh, I made use of a brass stand formed of two plates, which crossed each other. The top of this stand was made of the same metal, the plates being joined by a piece of the same, placed perpendicularly; these two upper plates had a point half an inch long, upon which the piece of meat was suspended. When the stand was thus prepared, I passed it under water and introduced it into the receiver. I then passed a plate under water and placed the recipient above, so that I might take it from the trough and place it upon a table. As the water rose, which happened when the temperature was diminished, or when gas was absorbed, I took care to add more, so as to prevent the access of atmospheric air. By

 this

this method the contact of atmospheric air was avoided; but there was this inconvenience, that the whole surface of the flesh was moistened, and that both it and the gas were in contact with the water which surrounded the recipient.

2d. (Experiment made in an empty vessel, that is to say, which contained neither water nor mercury.) I took bottles similar to common wine bottles but with larger necks. I filled them with gas in the pneumatic trough, and introduced the meat, keeping the mouth of the bottle above the water. I then corked it as speedily as possible, taking care to lute the cork with pasted paper when the neck of the bottle was put into water. By turning the bottle, the piece of flesh fell to the bottom, where it generally adhered on account of its moisture. By this method I avoided wetting the meat; and the gas also as well as that, was not in contact with the vapour of the water; but this method has the inconvenience of bringing some atmospheric air in contact with the meat, and a little of it always gets in when the bottle is corked. This method could not be followed when nitrous gas was employed.

3d. (Experiment over mercury.) I filled over mercury small recipients of three or four cubic inches, with gas intended for the experiment. I then introduced into it small pieces of flesh an inch long, half an inch broad, and two inches thick; I passed the meat through the mercury with my fingers, and put it under the receiver; by these means it was preserved from the contact of atmospheric air, and also of the vapour from water, the gas having been obtained over mercury. But I could only operate on a small scale, having but a small quantity of mercury.

It was necessary always to leave a little water or mercury in the recipient, so that the air when it came to dilate might not escape from the vessels. In the experiments over mercury it had this advantage, that the vessels were prevented from being overset.

The first set of experiments began the 25th of March, and ended the 4th of April 1808.

The temperature of the external air was always between the 8th degree of Reaumur and the 5th degree of the same thermometer in the shade. The first extreme was on the 30th of March in the morning, the latter on the 25th of March at noon.

1st. *Oxygen Gas.*

This was obtained from nitrate of potass. The experiments were made over water.

The

The first day, 25th March. The flesh is become sensibly redder, and even of a more beautiful red than with nitrous gas.

2d and 3d days. The red colour has diminished, but it still looks bright. 4th and 5th days, the same.

6th and 7th days. The redness has gradually diminished; the flesh is more moist than that put into nitrous and hydrogen gases.

8th day. The meat becomes quite moist, and begins to appear livid; I can discern upon its surface some isolated drops, semi-spherical and almost transparent.

9th day. The little drops are increased in number, and gradually become opake and whitish, so that the flesh appears as if it was covered with the small-pox.

10th and 11th days. Putrefaction makes sensible progress, the flesh has become flabby, the moisture increases, and the surface dissolves; however, the little drops may still be discovered on the surface of the liquor, which covers the whole of the flesh. On the evening of the eleventh day I took out the meat, removing the recipient above the trough. It gave out a putrid garlic odour, which had some resemblance to that of phosphorated hydrogen gas. The meat continued to putrefy in the atmospheric air, just as if it had been exposed to it from the first.

The volume of the oxygen gas was not diminished, it no longer inflamed a candle, nevertheless one burned in it for an instant with a little brightness.

2d. *Hydrogen Gas.*

This was prepared by dissolving zinc in sulphuric acid diluted with water.

1st day, 25th March. The flesh took on a palish colour at the end of a few hours, afterwards it became brown, like meat that is smoked after having been salted with pickle that did not contain nitre. From the second to the eleventh day no external change was observed in the flesh; it neither became flaccid nor moist; its cohesion seemed even to increase; it appeared to be harder and drier. It lost its red colour more and more; it became more brown than meat exposed to carbonic acid.

I took out the meat the evening of the eleventh day; there was not any sign of putridity, nor had it the least bad smell; at the most, one could only say it had a slight acid odour; exposed to the atmosphere it did not putrefy, but became dried; nevertheless, upon its surface might be observed

served some white spots of mouldiness. The gas inflamed when the recipient was turned up and carried near a candle.

3d. Carbonic Acid Gas prepared by dissolving Chalk in Nitric Acid.

1st day, 25th March. At the end of a few hours the meat had a palish tint, it then became brown, but it continued more pale than in the hydrogen gas. From the second to the eleventh day: in the first days it appeared to become flabby and livid, then no further change could be observed. I could not see that the surface became moist. The carbonic acid gas employed for the experiment was absorbed by the water; more readily it appeared than when it was purer, for I was obliged daily to add more gas to prevent the water touching the meat.

The evening of the eleventh day I took out the meat, which had not the least sign of putridity; it resembled cooked meat, it was flexible, but neither moist nor sticky: it had a slight acidulous odour, something like sour yeast; exposed to the atmosphere it did not putrefy, but dried, and its surface was covered with small white spots.

4. *Nitrous Gas obtained from Copper and Nitric Acid over Water.*

1st day, March 25. The meat became of a more beautiful red than in the atmospheric air, and could not be distinguished from that in the oxygen gas for several hours.

2d and 3d days. There was not any alteration. 4th, 5th, and 6th days, the red colour was a little diminished, but it was still very bright. 7th to the 11th day I could not perceive any change, except that the meat became a little moist, but it was not dissolved upon its surface. Its cohesion even appeared to be increased. On the evening of the eleventh day I took out the meat; it was of a fine red colour, firm, and had not the least smell, not even of nitrous gas. Exposed to the air, it lost its red colour in a few hours, became brown, and dried up more speedily than the meat in the two former experiments. Its surface was not covered with white spots. The gas tried with oxygen gas gave out red vapours, and was greatly diminished in its volume. It did not sensibly differ from common nitrous gas.

Second Set of Experiments from the 5th of April to the 10th of June.

The temperature of the external air was on the morning of of the 18th of April, 2° 5, and on the 17th of May, at noon, 23° 3. The temperature of the room was between 7° and 20° (Reaumur.) The meat employed for these experiments was paler than the former, and appeared to have been taken from a younger ox.

Oxygen obtained from red Oxide of Mercury. The Experiment made over Mercury.

1st day, April 5. The meat had become much redder; 2d and 3d days, no sensible change; 4th and 5th days, the meat was of a paler colour; 6th to 8th day, the colour was quite destroyed, and the meat looked as if it had been washed. 9th day. Small drops appeared upon its surface, as in the first experiment. 18th day. The drops are become opake, and resemble pustules of the small-pox. The meat preserves its consistence without dissolving, although the temperature was higher than in the last experiment.

19th to the 51st day (25th of May). I could observe very evident signs of putrefaction at the surface, the globules had united, and the surface was become black. An accident having occasioned the overthrow of the vessel, the gas escaped, and filled the whole house with such an offensive odour, that I was obliged to make use of the strongest perfumes to overcome it.

Oxygen obtained from Nitrate of Potass. Experiment made in an empty Vessel, close stopped with a Cork Stopper.

1st day to the 3d. The meat did not become more red. 4th day it was paler. 5th day to 51st. I could not observe any globules, the meat gradually grew paler, putrefied, and dissolved at the surface. At last a considerable quantity of fluid was formed. of a bad colour, which ran into the neck of the bottle. The meat was covered with moisture; the odour was less strong than that from the meat putrefied in the oxygen gas, and of a different kind.

Atmospheric Air. Experiment made over Mercury.

1st and 2d days, (5th and 6th of April). No remarkable change. 4th day. The meat had become very pale, much paler than in the oxygen gas.

5th to 51st day. No drops of liquor could be perceived. From the 8th day the meat was moist and dissolved on the surface, but less so than in the oxygen gas; and at the end of the experiment it was not so black as that in the pure oxygen gas. When it was taken out of the recipient, it did

did not smell so strong, and its colour was much redder when cut into.

Pure Hydrogen Gas obtained from the Vapours of Water, passed over red-hot Iron. Over Mercury.

1st day, (April 5). The meat became of a crimson red. 2d to the 51st day. No other change could be observed than that the meat became a little brown, but it had not a livid colour. It is remarkable that this meat preserved its red colour and retained an appearance of freshness, while the pieces put into the oxygen gas and atmospheric air grew pale. When it was taken out of the receiver, it had not the least smell. The gas at the end of the experiment rendered lime water turbid.

Pure Hydrogen Gas, in a Bottle with a Cork Stopper.

From the 1st to the 51st day the meat was not in the least brown, it preserved its colour, and only appeared a little moist. When taken out on the 51st day it had not the least bad odour; its smell something resembled that of smoked meat. The gas tried by nitrous air was not sensibly diminished, it rendered lime-water slightly turbid, and afterwards burned with great vivacity.

[To be continued.]

XVI. *On Heating Buildings by Steam.*

To Mr. Tilloch,

DEAR SIR, HAVING frequently troubled you on the subject of heating buildings by steam, I beg leave to mention that considerable progress has been made in it since the publication of my Essay on Fuel, and since I last wrote to you. A place of worship has been for a considerable time heated by steam on a most simple plan, so as to require little or no attendance, and does not require any water whatever to be added to that first put into the boiler above thrice in a winter. This is *an important fact*, and so is the following.

I have seen another mode, by which a fire of three hours in the morning serves for heating the whole of the rest of the day. This does away the objection to the use of steam as formerly applied for many purposes, such as hot-houses, &c. and is an introduction of a *new principle*, if I may use the expression, from which important practical benefits may arise. I am your most obedient,

ROBERTSON BUCHANAN,
Civil Engineer, Glasgow.

XVII. *Pro-*

XVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 27. The President in the chair. Professor Playfair furnished a lithological survey of Shehalean, one of the Grampian mountains in the north of Scotland, on which the late Dr. Maskelyne made his ingenious experiments to ascertain the attraction of the mountain on a plummet, and also the mean density of the earth. Part of this paper consisted of mathematical tables of a nature not to be read. The professor discovered with some difficulty the parts where Dr. M. had taken his angles from, and then proceeded to examine the rocks which compose the mountain: these he found to be chiefly granular quartz and micaceous schist, with their varieties. Mr. P., after taking a general survey of the position, bearings, surface, elevation and perpendicular sides of the mountain, collected specimens of the different rocks which compose it, and ascertained their specific gravity with great care and minuteness.

July 4.—A paper by Dr. Wells was read on vision. The purport of the author's observations was, that the focal distance of the eye depends chiefly on the contractibility of its muscles, and that the latter is much greater in youth than in persons of more advanced years. In youth the eye is capable of accommodating itself to the light and the distance of external objects, but in old age this contractile power of the muscles ceases, and the focal distance of the eye becomes shorter, and more fixed to a determinate point. Dr. Wells has made some experiments both when young and old, and caused others to make similar ones, with bella donna applied to the eyes: this plant increased the action of the ocular muscles in the young, but not in the old subject. Hence he inferred that short sight is less owing to the prominence or figure of the pupil, than to the flexibility of the muscles which direct it.

The Society then adjourned, during the long vacation, till Thursday the 10th of November next.

XVIII. *Intelligence and Miscellaneous Articles.*

The eleventh Number of Leybourn's Mathematical Repository contains, 1. Solutions to the mathematical questions proposed in Number IX; 2. Solutions to a curious problem in dynamics; 3. Expansion of a formula connected

nected with the inquiries relating to physical astronomy; 4. On the sine and cosine of the multiple arc; on the sine and cosine of an arc in terms of the arc itself, and a new theorem for the elliptic quadrant; 5. On magic squares; 6. An account of an experiment for determining the universal attraction of matter; 7. Observations on polygonal numbers; 8. On the irreducible case of cubic equations; 9. The Senate-house problems given in the university of Cambridge to the candidates for honours during the examination for the degree of B. A. in January 1811; 10. Continuation of Legendre's Memoir on elliptic transcendentals; and 11. A series of new questions to be answered in a subsequent Number.

The Royal College of Surgeons in London have awarded the Jacksonian prize of £10. and an extraordinary premium of £10. to Mr. John Smith Soden, of Coventry, and Mr. James Gillman, of Highgate, both members of that College, for their Dissertations on the *Bite of a rabid Animal*, from the consideration that such two dissertations are highly meritorious productions, and are *equally* worthy of the Jacksonian prize.

Dr. Quadrie, professor of anatomy at the university of Bologna, lately performed, while on his journey to Padua, two operations upon two individuals entirely blind, in the presence of the professors Socrafi and Brera and all their pupils. In these operations he demonstrated the advantages of a method invented by himself for extracting the cataract, without injury either to the transparent cornea or the iris. His method was acknowledged to be superior to that of Wenzel, and to that by simple pressure. His patients recovered their sight. The foreign professors loudly extol the new method, as easy, more practicable, and less hazardous, than that of simple pressure of the cataract. Dr. Quadrie has promised to make his method public.

M. Carnot, in his new Treatise on the Defence of Fortified Places, recommends the besieged to fire howitzers loaded with grape-shot or musket-ball, at an elevation of 45 degrees, when the enemy have made their approaches to within a certain distance, which may be effected without their being exposed, and the shot will do more execution, not being stopped by the enemy's works. M. Carnot's suggestion has been adopted in France, and now forms part of the artillery exercise.

Some

Some time since a labouring man engaged in ploughing in a field at Bignor, near Petworth, in Sussex, found the plough obstructed by a heavy stone, which he obtained assistance and removed; it is of marble, and beneath it a flight of steps of the same, leading to a large arched passage, when they discovered an entire Roman bath, with tessellated pavement in perfect preservation. The bath is of a hexagonal form, surrounded with seats; in the centre is a metallic pipe; the bottom of the bath is about two feet below the pavement, and five feet wide; the tessellated floor represents various figures in dancing attitudes, most beautifully wrought. In digging further, they found a dolphin and various other antiquities of the most costly materials. It is supposed to be the remains of a Roman palace. A Roman road has also been discovered leading through the field, and supposed to extend much further, but it is not at present suffered to be explored. A gentleman in the vicinity has an ancient manuscript which particularly speaks of this place, and many attempts have been made to discover it, before it was so fortunately accomplished by accident. In this manuscript many other curiosities are spoken of, which are expected to be discovered on a further exploration. Numbers of persons have been to examine the place. A very considerable sum has been offered for the field on a speculation, but refused.

M. Henz, an eminent tanner at Srzensk, in Poland, has ascertained that the leaves of the oak may be advantageously substituted for the bark, in tanning leather, provided they are used in the month of September, when they possess the bitter sap which they afterwards lose.

FALL OF A METEORIC STONE IN RUSSIA.

A meteoric stone, of the weight of fifteen pounds, fell to the earth on the 1st of March, in the village of Konleghowsk, dependent on the town of Romea, in the government of Tschernigoff, in Russia, and making part of the domains of Count Golovkin; its fall was preceded by three violent claps of thunder. When it was dug out from the depth of more than three feet, through a thick layer of ice, it still possessed heat: it was remarked, that at the third clap of thunder there was an extraordinary explosion, with a loud hissing noise, and throwing out a great quantity of sparks.

METEORO-

METEOROLOGICAL TABLE, By Mr. Carey, of the Strand, *For July* 1811.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Thermometer. Noon.	Thermometer. 11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
June 27	61	68°	53°	29·88	20	Cloudy
28	63	63	60	·82	0	Rain
29	61	66	60	·85	29	Cloudy
30	60	64	60	·85	36	Cloudy
July 1	62	66	64	·82	27	Cloudy
2	64	73	56	·87	33	Fair
3	60	66	54	·92	0	Rain
4	55	60	54	30·03	0	Rain
5	56	66	55	·10	57	Fair
6	55	68	54	·02	70	Fair
7	54	67	52	29·94	50	Fair
8	54	68	66	·92	62	Fair
9	57	71	66	30·01	46	Fair
10	66	74	62	·03	56	Fair
11	64	79	66	·13	64	Fair
12	66	79	65	·07	71	Fair
13	66	76	66	29·95	56	Fair
14	67	68	60	·85	32	Cloudy
15	63	72	66	·90	37	Showery
16	66	72	60	·91	38	Fair
17	65	72	61	·91	61	Fair
18	64	71	67	·82	47	Fair
19	68	73	68	·83	51	Fair
20	60	57	55	·83	0	Rain
21	57	57	56	·79	0	Rain
22	61	66	56	30·04	29	Fair
23	55	70	61	·04	46	Fair
24	62	72	55	·16	69	Fair
25	60	73	63	·17	60	Fair
26	66	73	67	·25	46	Fair

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

XIX. *On the Electricity of Minerals.* *By M.* Haüy*.

The property which certain natural bodies possess of becoming electrical by the intermedium of heat, furnishes mineralogy with one of the most advantageous characters for ascertaining what they are, and also gives rise to experiments which are interesting, in so far as they serve to manifest a remarkable co-relation between the crystalline forms of the same bodies and the positions of their electrical poles. But these experiments are at the same time delicate, particularly when we employ crystals of borated magnesia, which, with a volume scarcely exceeding two or three millimetres, have eight poles opposite to each other, in pairs; the powers of which have little energy, and reside each in a single point. Previously to the publication of my Treatise on Mineralogy, I was occupied with the construction of an apparatus which should be at the same time convenient, and sufficiently sensible to leave no doubt as to the results of the experiments in question. That which I have described in my Treatise, tome i. and in which I afterwards made a change as subsequently described in a former volume of these Annals, would appear to be perfect in every respect, if its effects were not subject, like those of all electrical machines, to be affected by the state of the atmosphere. I shall briefly mention here, that this apparatus consists of a small needle (Pl. III. fig. 1.) of copper or silver, terminated by two globules, moveable on a pivot, and isolated, to which we give at pleasure the vitreous or resinous electricity, by the action exercised upon it by an idio-electrical body to which friction has communicated the contrary electricity. If this body is a stick of wax, for instance, we present it at a few centimetres distance from the stalk which supports the small needle, at the same time that we keep a finger applied to the foot *a* of this stalk: we afterwards remove the finger, then the stick of wax, and in this case the apparatus is vitreously electrified. Now, when the air is loaded with aqueous vapours, its influence on the metallic needle destroys in an instant the electrical virtues of the latter, or renders it so feeble and fugacious, that the operator is compelled to abandon the experiment, and to wait for dry weather. In a public lecture-room, a numerous audience produces a similar effect to that of dampness. I have on such occasions tried in vain to electrify by friction

* *Ann. du Museum d'Histoire Naturelle*, tome xv. p. 1.

a stick of wax or gum lac, particularly in the great heats of summer.

These inconveniences suggested the idea of employing in the experiments relative to the object in question, only bodies susceptible of being electrified by heat, and to make their mutual capacities subservient to the development of their properties. As the two fluids which compose the natural electric fluid of these bodies, before the experiment, remain engaged in their pores; after being extricated by the effects of heat, they are removed from all external influence, and the electrical state of the bodies is kept up in the midst of the dampest air. I do not know if there is not even something more striking in those experiments which connect the functions of bodies electrical by heat, with those of the magnet, to which they have so great an analogy, either in consequence of their double polar virtue, or by the law to which the distribution of the two fluids is subjected in their interior.

I shall now describe the new apparatus which I employ in the experiments in question, and which was made with much care by M. Tavernier, an eminent watch-maker. It is composed of two principal pieces; the one is a stalk of silver, *a b* (fig. 2.) fixed on a round piece of the same metal, and having at its upper extremity a very sharp-pointed steel needle, *a g*: the other piece consists principally of a rectangular plate of silver, *h k*, turned up at both ends, where holes have been made at *o r*. This silver plate is pierced in the middle by a circular hole, in order to receive a small cover of rock crystal which is held by a circle of silver, and by means of two screws, *s*, *z*.

Towards the extremities of the inferior surface of the plate *h k*, are fixed two silver wires *m i*, *n y*, directed a little obliquely to this surface, and terminated by two globules *l*, *p* of the same metal. Fig. 3. represents this plate seen from below, and fig. 4. represents the stalk, with the steel needle by which it is terminated.

When the apparatus is fixed, as we see in fig. 2, the needle in question performs the office of a pivot, which enters into a small aperture made in the under surface of the rock crystal cover. The two holes *o*, *r*, are destined to receive a tourmaline *t t′*, or any other body of an oblong form, susceptible of being electrized by heat; and such is the sensibility of the apparatus, that a small force which acts by attraction, or by repulsion, on either extremity of the body *t t′*, instantly produces a very perceptible rotatory motion in this body.

To proceed to the experiments: let us suppose in the first place that we wish to determine the positions of the electrical poles of a tourmaline, which is of a thin and oblong form, like all the tourmalines of Spain. After having heated it, we shall place it in the apparatus, and present successively, at a small distance from its two extremities, another body which has been electrized by friction. I prefer employing for this purpose the Saxon or Brazil topazes, because these minerals, as I have remarked in my Treatise on Mineralogy, are extremely sensible to the action of friction in order to excite the electrical virtue in them, so that not only a damp atmosphere does not hinder them from acquiring it, but they preserve it for a considerable time. The kind of electricity in question being of the same nature with that of glass, the pole of the tourmaline which the topaz will repel will be the vitreous pole, and that upon which it will act by attraction will be the resinous pole. It is sufficient to have a tourmaline susceptible by its form of being placed in the apparatus, and the poles of which are known, in order that it may serve as a term of comparison to all the bodies of the same species, or of different species, which share the property in question, whatever in other respects are the forms and dimensions of such bodies. After having heated that which we wish to examine, we bring it successively by its two extremities near both of those of the tourmaline, and the consequence of the result is presented of itself, agreeably to the principle common to electricity and to magnetism, viz. the pole solicited by homogeneous fluids is repelled, and those in which heterogeneous fluids reside are attracted.

The advantages of the apparatus which I have described, are particularly felt in the experiments relative to borated magnesia, which, in order to succeed, require very favourable circumstances, when we use the metallic needle which I first mentioned. It is necessary that we should wait until the action of the tourmaline placed in the apparatus has been diminished, by cooling, to the point of being found in proportion to the feeble virtue of the crystal of borated magnesia; and we must also take care to keep the crystal in such a position, that, the axis which passes by the pole which we present to the tourmaline being perpendicular to the length of the latter, the same pole corresponds to the centre of action of the tourmaline, which we know to be very near the extremity.

This apparatus may be also employed for determining the kind of electricity which a body acquires by means of friction.

tion. If it repels the pole of the tourmaline to which we first present it, this only indicates that the body is itself in the electric state, and also that its electricity is contrary to that of the pole in question. But if the tourmaline was attracted, we could conclude nothing from it, because a body which is even in the natural state, acts always by attraction on an electrized body, whatever be the kind of electricity which this last requires. In this case, therefore, we must afterwards present the body to the other pole of the tourmaline; and if the repulsion succeeds the attraction, we shall have a proof that this body is in a state opposite to that of the pole which has been repelled.

When we employ the electricity acquired by heat, solely as a mineralogical character, the small metallic needle represented (fig. 1.) is sufficient for the experiments relative to this character, without its being even necessary to isolate this needle. We judge that a mineral is endowed with the property in question, according as it attracts the needle to it, or leaves it fixed when placed at a small distance from it.

I recently made use of this method for comparing various minerals, relative to the faculty which they have of preserving for a longer or shorter time the electricity acquired by friction. After having put them in the electrical state, I placed them on any stone, (marble for instance,) so as to make the surface which had been rubbed, opposite to that which lay upon the stone, and from time to time I took them with my fingers or with pincers, by a corner which was far from the electrized part, in order to present them to the small needle. The topaz, of all the minerals which I tried, seemed to preserve electricity longest. A cut piece of the limpid Brazil kind acted upon the needle at the end of 32 hours. In the hyaline corindon, called *oriental sapphire*, the emerald, the spinel, and other stones which are made into trinkets, the duration of the electrical virtue generally exceeded five or six hours: it exceeded 24 hours in an emerald from Peru. But I met with two minerals which differ from the above in a striking manner by a less coercive force with respect to the electrical fluid,—the one is the diamond and the other rock crystal,—and I remarked that their electrical virtue was extinct in 15 or 20 minutes. Some crystals of quartz, however, preserved it for about 40 minutes.

The limpid Brazil topaz, already mentioned, seems to resemble the diamond in the liveliness of its lustre, where it has been cut. It is the same with the hyaline corindon called *white sapphire*. The foregoing results might be em-

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ployed in such cases, at least as auxiliary characters, to assist us in distinguishing substances which are so different in their nature.

Coloured glass possesses but feebly the faculty of preserving electricity; and if there does not exist in this respect any very marked difference between such substances and quartz, we shall at least avoid confounding with the emerald, the topaz, or the sapphire, factitious stones which sometimes present imposing imitations of these gems. I know that the specific gravity, the hardness, and the refraction of these substances present much more palpable means of detection than the above; but we cannot too much multiply indications which may assist us in ascertaining a mineral substance, when the artist has stripped it of the exterior which nature had given it, or rather of that form which cannot be imitated by any other.

XX. *Experiments and Observations on the different Modes in which Death is produced by certain vegetable Poisons. By* B. C. Brodie, *Esq. F.R.S. Communicated by the Society for promoting the Knowledge of Animal Chemistry**.

I. The following experiments were instituted with a view to ascertain, in what manner certain substances act on the animal system, so as to occasion death, independently of mechanical injury. I was led to the inquiry, from the subject of it appearing to be of considerable interest and importance, and from a hope, that, in the present improved state of physiological knowledge, we might be enabled to arrive at some more satisfactory conclusions than had been deduced from any former observations.

The substances which act as poisons when applied to the animal body are very numerous. In the experiments which I have hitherto made, I have employed vegetable poisons only. Of these I have selected such as are very active and certain in producing their effects, believing that, on this account, the exact nature of those effects would be more readily ascertained. The principal objects which I have kept in view have been to determine, on which of the vital organs the poison employed exercises its primary influence, and through what medium that organ becomes affected. I have also endeavoured to ascertain by what means the fatal consequences of some poisons may be prevented.

* From the Philosophical Transactions for 1811, part i.

vented. With some of the conclusions which I have ventured to draw, so far as I know, we were not before acquainted; and others of them, though not entirely new, had not been previously established by satisfactory experiments.

I shall relate first those experiments in which poisons were applied internally, that is, to the mucous membranes of the tongue or alimentary canal, and afterwards those in which poisons were applied to wounded surfaces.

II. *Experiments with Poisons applied to the Tongue or alimentary Canal.*

Experiments with Alcohol.

When spirits are taken into the stomach, in a certain quantity, they produce that kind of delirium which constitutes intoxication: when taken in a larger quantity, it is well known that they destroy life altogether, and that in the course of a very short space of time. Intoxication is a derangement of the functions of the mind, and, as these are in some way connected with those of the brain, it seems probable, that it is by acting on this organ, that spirits when taken into the stomach occasion death. In order to ascertain how far this conclusion is just, I made the following experiments*.

Exp. 1. I poured two drachms of proof spirits down the œsophagus of a cat. Instantly he struggled violently; then lay on one side, perfectly motionless and insensible; the breathing was laboured and stertorous, and the pulsations of the heart were very frequent. He continued in this state for seven or eight minutes; then began to recover; the respirations became easier, and presently he stood up, and was able to walk.

Exp. 2. I injected an ounce and a half of proof spirits into the stomach of a large full-grown rabbit, by means of an elastic gum tube passed down the œsophagus. The same symptoms took place as in the last experiment; but the animal did not begin to recover from the state of insensibility until forty minutes had elapsed from the time of the injection.

Exp. 3. Seven drachms of proof spirits were injected

* I am indebted to Dr E. N. Bancroft for his assistance in many of the experiments which I am about to detail. Mr. W. Brande lent me his assistance in the greater part of those which were made. I have been further assisted by Mr. Broughton, Mr. R. Rawlins, and Mr. R. Gatcombe, and by several other gentlemen.

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into the stomach of a younger rabbit. Two minutes afterwards, he evidently was affected by the spirits, and in three minutes more he lay on one side motionless and insensible. The pupils of the eyes were perfectly dilated; there were occasional slight convulsive motions of the extremities; the respiration was laborious, it was gradually performed at longer and longer intervals, and at the end of an hour and fifteen minutes had entirely ceased. Two minutes after the animal was apparently dead, I opened into the thorax, and found the heart acting with moderate force and frequency, circulating dark-coloured blood. I introduced a tube into the trachea, and produced artificial respiration by inflating the lungs, and found that by these means the action of the heart might be kept up to the natural standard, as in an animal from whom the head is removed.

Exp. 4. I injected into the stomach of a rabbit two ounces of proof spirits. The injection was scarcely completed, when the animal became perfectly insensible. Precisely the same symptoms took place as in the last experiment, and at the end of twenty-seven minutes, from the time of the injection, the rabbit was apparently dead; but on examining the thorax the heart was found still acting, as in the last experiment.

It has been shown by M. Bichat, and the observation has been confirmed by some experiments which I have lately had the honour of communicating to this learned Society, that the brain is not directly necessary to the action of the heart, and that, when the functions of the brain are destroyed, the heart continues to contract for some time afterwards, and then ceases only in consequence of the suspension of respiration, which is under the influence of the brain.

It would appear, from the experiments which I have just detailed, that the symptoms produced by a large quantity of spirits taken into the stomach, arise entirely from disturbance of the functions of the brain. The complete insensibility to external impressions; the dilatation of the pupils of the eyes; and the loss of motion, indicate that the functions of this organ are suspended; respiration, which is under its influence, is ill performed, and at last altogether ceases; while the heart, to the action of which the brain is not directly necessary, continues to contract, circulating dark-coloured blood for some time afterwards.

There is a striking analogy between the symptoms arising from spirits taken internally, and those produced by injuries of the brain.

Concussion of the brain, which may be considered as the slightest degree of injury, occasions a state of mind resembling intoxication, and the resemblance in some instances is so complete, that the most accurate observer cannot form a diagnosis, except from the history of the case. Pressure on the brain, which is a more severe injury than concussion, produces loss of motion, insensibility, dilatation of the pupils; respiration becomes laboured and stertorous, is performed at long intervals, and at last altogether ceases, and the patient dies.

It forms an interesting matter of inquiry, whether spirits when taken into the stomach produce their effects on the brain, by being absorbed into the circulation, or in consequence of the sympathy that exists between these organs by means of the nerves. The following circumstances lead me to conclude that they act in the last of these two ways.

1. In experiments where animals have been killed by the injection of spirits into the stomach, I have found this organ to bear the marks of great inflammation, but never found any preternatural appearances whatever in the brain. 2. The effects of spirits taken into the stomach in the last experiment were so instantaneous, that it appears impossible that absorption should have taken place before they were produced. 3. A person who is intoxicated, frequently becomes suddenly sober after vomiting. 4. In the experiments which I have just related, I mixed tincture of rhubarb with the spirits, knowing from the experiments of Mr. Home, and Mr. William Brande, that this, when absorbed into the circulation, was readily separated from the blood by the kidneys, and that very small quantities might be detected in the urine by the addition of potash; but, though I never failed to find urine in the bladder, I never detected rhubarb in it.

The including the termination of the thoracic duct in a ligature does not prevent spirits, when taken into the stomach, from producing their usual effects on the nervous system; but subsequent observations, which Mr. Home has already communicated to this Society, have shown that no conclusion can be drawn from this experiment.

That a poison may affect a distant organ, through the medium of the nerves, without entering the circulation, is proved by the well-known circumstance of solution of the extract of *belladonna*, when applied to the tunica conjunctiva of the eye, occasioning dilatation of the pupil of the same eye, though no other part of the system is affected.

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It has been formerly supposed by Dr. Mead and other physiologists, that a poison may produce death by acting on the extremities of the nerves of the stomach and intestines, without being absorbed into the circulation. That it should by these means be capable of affecting the brain is not to be wondered at, when we consider the numerous and various sympathies between this organ and the alimentary canal, evidently independent of any other communication than the nerves.

Experiments with the Essential Oil of Bitter Almonds.*

Exp. 5. One drop of the essential oil of bitter almonds was applied to the tongue of a young cat. She was instantly seized with violent convulsions; then lay on one side motionless, insensible, breathing in a hurried manner; the respirations became laboured, took place at longer and longer intervals, and at the end of five minutes, from the application of the poison, had entirely ceased, and the animal was apparently dead; but, on opening the thorax, the heart was found acting regularly eighty times in a minute, circulating dark-coloured blood, and it continued to act for six or seven minutes afterwards.

Exp. 6. I injected into the rectum of a cat half an ounce of water, with two drops of the essential oil. In two minutes afterwards, he was affected with symptoms similar to those which occurred in the last experiment, and at the end of five minutes, from the injection of the poison, he was apparently dead. Two minutes after apparent death, the heart was found acting eighty times in a minute. On dissection, no preternatural appearances were found either in the internal membrane of the rectum, or the brain.

The symptoms produced by this poison, and the circumstance of the heart continuing to contract after apparent death, lead to the conclusion that it occasions death by disturbing the functions of the brain.

While engaged in these last experiments, I dipped the blunt end of a probe into the essential oil, and applied it to my tongue, meaning to taste it, and having no suspicion that so small a quantity could produce any of its specific effects on the nervous system; but scarcely had I applied it, when I experienced a very remarkable and unpleasant sensation, which I referred chiefly to the epigastric region,

* The essential oil of bitter almonds does not appear to differ from the essential oil of laurel. I was furnished with a quantity of it, first by my friend Mr. William Brandé, and afterwards by Mr. Cooke of Southampton-street.

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but the exact nature of which I cannot describe, because I know nothing precisely similar to it. At the same time there was a sense of weakness in my limbs, as if I had not the command of my muscles, and I thought that I was about to fall. However, these sensations were momentary, and I experienced no inconvenience whatever afterwards.

I afterwards applied a more minute quantity of the essential oil to my tongue several times, without experiencing from it any disagreeable effects; but on applying a larger quantity, I was affected with the same momentary sensations as in the former instance, and there was a recurrence of them in three or four seconds after the first attack had subsided.

From the instantaneousness with which the effects are produced; and from its acting more speedily when applied to the tongue than when injected into the intestine, though the latter presents a better absorbing surface, we may conclude that this poison acts on the brain through the medium of the nerves, without being absorbed into the circulation.

Experiment with the Juice of the Leaves of Aconite.

Exp. 7. An ounce of this juice was injected into the rectum of a cat. Three minutes afterwards he voided what appeared to be nearly the whole of the injection; he then stood for some minutes perfectly motionless, with his legs drawn together; at the end of nine minutes, from the time of the injection, he retched and vomited; then attempted to walk, but faltered and fell at every step, as if from giddiness. At the end of thirteen minutes, he lay on one side insensible, motionless, except some slight convulsive motions of the limbs. The respiration became slow and laboured; and at forty-seven minutes from the time of the injection, he was apparently dead. One minute and a half afterwards, the heart was found contracting regularly one hundred times in a minute.

It appears from this experiment, that the juice of aconite, when injected into the intestine, occasions death by destroying the functions of the brain. From the analogy of other poisons, it is rendered probable that it acts on the brain through the medium of the nerves, without being absorbed into the circulation. This opinion is confirmed by the following circumstance: if a small quantity of the leaf of aconite is chewed, it occasions a remarkable sense of numbness of the lips and gums, which does not subside for two or three hours.

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Experiments with the Infusion of Tobacco.

Exp. 8. Four ounces of infusion of tobacco were injected into the rectum of a dog. Four minutes afterwards he retched, but did not vomit; he then became faint, and lay motionless on one side; at the end of nine minutes from the time of the injection, the heart could not be felt; he gasped for breath at long invervals; and in another minute there was no appearance whatever of life. I immediately laid open the cavities of the thorax and abdomen. The heart was much distended, and had entirely ceased to contract; there was no peristaltic motion of the intestines.

Exp. 9. An ounce of very strong infusion of tobacco was injected into the rectum of a cat. Symptoms were produced similar to those which occurred in the last experiment, and the animal died at the end of seven minutes from the time of the injection. On opening the thorax immediately after death, the heart was found extremely distended, and to have entirely ceased acting, with the exception of a slight tremulous motion of the auricles.

Exp. 10. Three ounces of infusion of tobacco were injected into the rectum of a dog. He was affected with symptoms similar to those in the former experiments, and died at the end of ten minutes. On opening the thorax immediately after death, I found the heart much distended, and to have entirely ceased contracting.

Exp. 11. Three ounces of infusion of tobacco were injected into the rectum of a dog. Immediately there took place tremulous contractions of the voluntary muscles. Five minutes afterwards the injection was repeated in the same quantity. The dog then was sick, and threw up some of the infusion, with other matter, from the stomach; he became faint, and died ten minutes after the second injection. Immediately after respiration had ceased, I opened the thorax, and found the heart extremely distended, and without any evident contraction, except of the appendix of the right auricle, which every now and then contracted in a slight degree. I divided the pericardium on the right side. In consequence of the extreme distension of the heart, this could not be done without irritating the fibres with the point of the scalpel. Immediately both auricles and ventricles began to contract with considerable force, so as to restore the circulation. Artificial respiration was produced, and the circulation was kept up for more than half an hour, beyond which time the experiment was not continued.

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We may conclude from these experiments, that the effect of the infusion of tobacco, when injected into the intestine of a living animal, is to destroy the action of the heart, stopping the circulation and producing syncope. It appeared to me that the action of the heart ceased even before the animal had ceased to respire; and this was confirmed by another experiment, in which, in a dog killed by the infusion of tobacco, I found the cavities of the left side of the heart to contain scarlet blood, while in those of the right side the blood was dark-coloured. This poison therefore differs materially from alcohol, the essential oil of almonds, and the juice of aconite, which have no direct influence on the action of the heart. The infusion of tobacco renders the heart insensible to the stimulus of the blood, but it does not altogether destroy the power of muscular contraction, since the heart resumed its action in one instance on the division of the pericardium, and I have found that the voluntary muscles of an animal killed by this poison, are as readily stimulated to contract by the influence of the Voltaic battery, as if it had been killed in any other manner. At the same time, however, that the infusion of tobacco destroys the action of the heart, it appears to destroy also the functions of the brain, since these did not return in the last experiment; although the circulation was restored, and kept up by artificial respiration.

Since there is no direct communication between the intestinal canal and the heart, I was at first induced to suppose that the latter becomes affected in consequence of the infusion being conveyed into the blood by absorption. Some circumstances in the following experiment have since led me to doubt whether this is the case.

Exp. 12. In a dog, whose head was removed, I kept up the circulation by means of artificial respiration, in the manner already described in the account of some experiments which I lately communicated to this Society. I then injected into the stomach and intestines nine ounces of infusion of tobacco. At the time of the injection, the body of the animal lay perfectly quiet and motionless on the table; the heart acted regularly one hundred times in a minute. Ten minutes afterwards the pulse rose to one hundred and forty in a minute; the peristaltic motion of the intestines was much increased, and the voluntary muscles in every part of the body were thrown into repeated and violent spasmodic action. The joints of the extremities were alternately bent and extended; the muscles of the spine,

spine, abdomen, and tail alternately relaxed and contracted, so as to turn the whole animal from one side to the other. I have observed, in other instances, spasmodic actions of the muscles, where the circulation was kept up by artificial respiration, after the removal of the head, but not at all to be compared, either in strength or frequency, with those which took place on this occasion. I made pressure on the abdominal aorta for more than a minute, so as to obstruct the circulation of the blood in the lower extremities; but the muscular contractions were not lessened in consequence. Half an hour after the injection of the infusion, the artificial respiration was discontinued. The heart continued to act, circulating dark-coloured blood; the muscular contractions continued, but gradually diminished in strength and frequency. I tied a ligature round the vessels at the base of the heart, so as to stop the circulation; nevertheless the muscular contractions still continued, though less frequent and forcible than before, and some minutes elapsed before they entirely ceased.

In this experiment, the disposition to contraction in the muscles was very much increased, instead of being diminished, as in those just related. If the infusion of tobacco influences the heart from being absorbed into the blood, and thus coming into actual contact with its fibres, there is no evident reason why the removal of the brain, and the employment of artificial respiration, should occasion so material a difference in its effects. If the contractions of the voluntary muscles had depended on the infusion circulating with the blood, it is reasonable to suppose that the pressure on the aorta would have occasioned some diminution of them, and that the complete obstruction of the circulation would have caused them to cease altogether.

From these considerations, I am induced, on the whole, to believe that the infusion of tobacco, when injected into the intestines, influences the heart through the medium of the nervous system; but I have not been able to devise any experiment, by which the truth or fallacy of this opinion might be put beyond the reach of doubt.

It appears remarkable, that the brain and nervous system, although not necessary to the action of the heart, should, when under the influence of the infusion of tobacco, be capable of influencing this organ so as to stop its action; but this is analogous to what we see occur in consequence of violent emotions of the mind. Those states of the nervous system, which accompany the passions of joy, fear,

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or anger, when existing in a moderate degree, render the heart more sensible to the stimulus of the blood, and increase the frequency of its contractions; while, when the same passions exist in a greater degree, the heart is rendered altogether insensible to the stimulus of the blood, and syncope ensues.

[To be continued.]

XXI. *History of fatal Effects from the accidental Use of White Lead; in a Letter to the President.* By JOHN DEERING, *Surgeon, F.M.S.; with additional Remarks by* WILLIAM SHEARMAN, *M.D.F.M.S.**

AT the sitting of January 30, 1809, a verbal communication was made to the Society, by the author of the following memoir, of some extraordinary symptoms, followed by the death of several individuals of a family whom he had attended. It appearing to the members present highly probable that these unfortunate events originated from the poison of lead†, a committee was deputed to investigate and to endeavour to detect the real cause of the fatality; which the following relation fully and satisfactorily explains.

Aldersgate Street, Oct. 4, 1809.

If the following narrative do not convey any important medical information, it may not be wholly uninstructive, as it relates to a domestic calamity, occasioned by a circumstance which at the time was wholly unsuspected; and it may at least inculcate the necessity of a closer investigation of symptoms from causes not fully ascertained, and at the same time evince the fallacy of hasty prognostics.

On the 21st of October last, I was desired to visit Mrs. R., the wife of a respectable tradesman in Aldersgate-street, who complained of violent pain in the scrobiculus cordis, with great soreness of the epigastric region when pressed upon. She had vomited a considerable quantity of bilious matter, and at the same time her bowels were constipated: the pulse was calm and regular, the tongue clean and moist, and there was no symptom of fever present. She immediately took a cathartic, which operated, and an opiate in the evening. The following morning the patient appeared

* From the Transactions of the Medical Society of London, vol. i. part i.

† On the following evening Dr. Shearman delivered the annexed communication, which served to confirm the probability of these suspicions; although Mr. D. had been hitherto unsuccessful in detecting the precise origin of the exciting cause.

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relieved; in the evening, however, the pains and vomiting recurred, and these symptoms continued for some successive days, in so distressing a degree, that it was deemed advisable to consult the family physician, which was done on November 4, 1808. At this time these symptoms continued as already intimated, without any appearance of fever, and hence the physician was induced to consider the affections as of a rheumatic and spasmodic nature.

In a few days, in consequence of the amendment of the patient, he discontinued his visits. In about a week after this period, a boy in the same family, nearly sixteen years of age, was seized with symptoms exactly similar to those of the preceding case, and similar remedies afforded only partial relief, till at length he was removed into the country, and thereby recovered his health.

A week after the attack of this youth, the eldest child, a boy six years old, was also seized with analogous symptoms, and, the mother having relapsed into her former state, the physician was again consulted on the 19th of November. At this time three other persons in the family laboured under similar affections, and suspicions were now entertained that some poisonous substance might have caused this general indisposition of the family; but after minute investigation no one circumstance was discovered to confirm this suspicion, or to elucidate the source of so extensive a calamity.

The sickness and pain continued unabated in Mrs. R.; but the son, after the period of a fortnight, was deemed in a state of convalescence by his physician, who discontinued his attendance; he was, however, soon after seized with convulsions, and expired within a few hours. Unexpected and severe as this shock was, Mrs. R. afterwards gradually grew a little better. She had hitherto continued to suckle her child, which, it being fifteen months old, she was advised to wean: to this she reluctantly consented. In about ten days afterwards the child became somewhat costive, without any other apparent indisposition; but at this period it was seized with vomiting and convulsions, and suddenly expired. The unhappy parent now experienced a return of her complaints, and, under a persuasion of the inefficacy of professional aid, she was prevailed upon to consult an empiric, whose attendance, though continued to the end of the year, proved unavailing; and on the 3d of January, 1809, she had the advice of Mr. Chevalier, an experienced surgeon, who considered the patient's complaint to be chronic rheumatism; and by the use of clysters of warm water,

water, oily mucilaginous medicines, fomentations, and vesicatories, she appeared to experience more relief than at any period since the first attack; but, although the vomiting and sickness were less violent and frequent, the pain and soreness of the abdomen, first complained of, never entirely subsided: she was, however, able to sit up and amuse herself with a little needlework, and even to go about the domestic concerns of the family, and Mr. Chevalier had proposed to pay his final visit on the 21st. On the morning of this day she rose at ten o'clock, and within the space of an hour afterwards, whilst standing near the desk of drawers, she suddenly exclaimed, "I am dying!" She was seized with convulsions, which continued till five o'clock in the afternoon, when she expired.

On the subsequent day, Mr. Chevalier, whose anatomical skill is well known, examined the body by dissection. Neither the thoracic and abdominal viscera, nor the brain, upon the most minute examination, exhibited the least appearance of disease; in short, not the least trace could be discovered of any morbid affection.

With respect to the three other persons already mentioned to have been indisposed, the servant maid, one of them, was conveyed to her friends, and recovered. A sister-in-law of Mrs. R. also recovered; but the third, who was her mother-in-law, died, after lingering under disease till March.

These circumstances having been cursorily communicated to the Medical Society, Dr. Adams, Dr. Hamilton, and Mr. Lawrence, were requested to visit the house of this unfortunate family, and to endeavour to ascertain the cause of the calamity. Every culinary article and the whole premises were accurately examined, but without its leading to any discovery. It appeared, indeed, that Mr. R., the husband of the deceased lady, had purchased a cask of sugar at a sale, a considerable part of which had been disposed of to some friends in the country, who had used it without inconvenience, and hence no suspicion was entertained of this article having produced the fatality in Mr. R.'s family.

In this state of uncertainty, Dr. Laird, another member of the Medical Society, visited the house; and, on examining the cask which had contained the sugar, he observed a white powder adhering to its inner surface, and which, on being heated by the blow-pipe on charcoal, afforded globules of lead in the metallic state.

The mystery was thus at length developed. The sugar had been injudiciously put into a cask which had previously contained white lead. That part of the sugar which was

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sent into the country had probably been taken out of the middle of the cask, and had never come in contact with the lead; whilst that which was used by the family, having been taken from the side, was impregnated with this metal, and doubtless was the source of the fatal events described.

Of nine persons in this family, who were more or less indisposed, four died, and the effects of the poison appear to have been nearly in the ratio of their respective ages.

The infant, fifteen months old, was attacked and expired within the space of twenty-four hours; the child six years of age survived a fortnight; Mrs. R., aged forty, lingered three months before the fatal event took place; and the mother-in-law, aged sixty-seven, died four months after the attack.

The symptoms in each were very similar. The vomiting, pain in the stomach, and costiveness, marked the attack of the disease; and the soreness of the epigastric region in those who recovered was not removed by medicine, but seemed rather gradually to wear away by time or change of air. The matter vomited was usually of a dark yellow colour, though sometimes green; the fæces were in general dark-coloured; but in the case of Mrs. R. they were completely white during the space of twenty-four hours only.

There was a considerable sameness in the medical treatment. The opiates which were given afforded no mitigation of the symptoms, unless joined with cathartics, and aided by fomentations, &c. The countenances of all the patients exhibited a pale, sickly, wan aspect. The pulse in each was slow and regular, rather indeed sluggish, and generally below the natural state; but in no instance was there any symptom of paralysis.

J. Deering.

Further Observations on the same Subject. By William Shearman, *M.D.F.M.S.*

The circumstance related in the preceding communication of several persons in the same family being attacked with similar symptoms, differing only in degree, and resembling in appearance those of the colica pictonum, the exciting cause of which could not be discovered after the most accurate research, brings to my recollection an occurrence which happened within my observation several years ago, where this disease raged with different degrees of violence among a great number of people, produced in all of them by the same unsuspected cause, and which, in its incipient and milder state, from its general prevalence, was not recognised either by the other practitioners of the town where I then resided, or myself, to be the genuine painters' colic.

This town, a sea-port in Essex, contained between three and four thousand inhabitants, and at the time I speak of, very many people, chiefly adults, and a greater proportion of them men, complained of occasional violent colic pains, chiefly occurring after meals, attended with an obstinate costiveness; and although these symptoms were for a time relieved by the use of purgatives and other means, they almost universally recurred. The progress of the disease, even in those cases where it attained its utmost violence, was in almost every instance so insidious and so slow, as to leave us unapprehensive of its true character; which, however, was at length brought to light in the following manner:

An infant, under twelve months, at the breast, who had been subject to complaints arising from acidity of the food, was tormented with most excruciating pain, apparently in the bowels, attended by a very great degree of constipation, and accompanied with violent straining efforts at evacuation, resembling tenesmus. The sufferings of this poor little child were in the highest degree distressing, and it obtained but temporary relief from the warm bath, laxative injections, those of an anodyne quality, the throwing up into the rectum warm oil, opiates and purgatives combined, or from any treatment whatever that could be suggested. The seeing so unusually severe a case, suggested to my mind the probability that some improper substances had been exhibited to the little patient, and I was earnest in my inquiries to this point. All my endeavours only ascertained that the nurse had occasionally given the child a tea spoonful or two of ardent spirit in its food; a practice, which, although I much reprobated, I knew to be too common among nurses, solely to account for this violent disease. My patient at length fell a victim; and a very short time after, the father of the child regretting to me the mismanagement of its nurse in giving it spirits, observed, that he himself was occasionally tormented with pains in his bowels, which he was inclined to attribute to drinking a single glass of Hollands and water every night. This induced a suspicion in my mind; and upon dropping into a small quantity of the spirits a single drop of the volatile tincture of sulphur of the old London Pharmacopœia, it assumed a very dark colour, affording a certain evidence of its

its containing a metallic poison. This Hollands geneva had been bought at the king's excise warehouse in the town, where many hundred gallons were annually sold, that had been seized by the excise officers from persons attempting to smuggle it into this country. The gentleman, grieved at the loss of his child, which he could no longer fail to attribute to its true source, brought up the chief managing officer before the magistrates; when he confessed that the whole of the quantity of Hollands sold at the last sale had been impregnated with sugar of lead, for the purpose of depriving the spirit of the colour which it always obtained by being kept for some time in the tubs in which it was brought over sea by the smugglers, and the loss of which colour enhanced its price by three or four shillings a gallon. This circumstance afforded an easy explication of the cause of the malady which had so generally prevailed; and henceforth none other than coloured Hollands were exposed to sale at the excise warehouse, as had been the custom previous to this scientific attempt of the above officer, at once to increase the king's revenue and his own.

This recital strongly illustrates the obscurity in which the occasional causes of disease may sometimes be involved; and, as a proof of the difficulty of raising suspicion of the deleterious quality of substances, I may mention, that among those who died on this occasion was a dissenting clergyman, about sixty years of age, a man of good sense and observation, of temperate habits (if the daily custom of taking a glass of spirits and water after supper is not to be considered a deviation from the rules of temperance), whose wife carried on the business of a druggist; and it may be supposed they were both acquainted with the noxious qualities of the preparations of lead: yet it appeared that the sugar of lead with which this spirit was impregnated had been bought at their house by the exciseman himself, and in quantities of 28lbs. at a time; but it did not occur to either of them, or to his medical attendant, that the disorder was connected with the drinking of the Hollands. It is to be remembered, that in the early stages we have no certain diagnostic signs by which the colica pictonum can be distinguished from the other species of colic; it is only by its ultimate effects, or by a knowledge of its exciting causes, that we can confidently pronounce concerning the existence of the disease.

W. SHEARMAN.

 XXII. *Me-*

XXII. *Memoir on the Existence of a Combination of Tannin and a vegetable Matter in some Vegetables.* By *Messrs.* Fourcroy *and* Vauquelin *.

§ I. *Subject of our Inquiries.*

It was natural to suppose that when there were formed, either successively or simultaneously, tannin and animal substances in vegetables, these two compounds would unite when they met: nevertheless, although the knowledge we have acquired on the subject of tannin and the animal substance give great probability to this opinion, no chemist has yet announced the existence of this species of combination in plants.

Upon analysing several vegetable matters more or less different from each other, and particularly the Indian chesnut, garden beans, lentils, &c., we discovered the compound in question, and we shall now give the result of our experiments.

The facts which we are about to describe seem interesting, because they afford an explanation of a great number of phænomena observed in the analysis of vegetables, as well as in their employment in dyeing or in other arts,—phænomena which the chemists have not yet been able to account for.

§ II. *Examination of the Skin of Garden Beans.*

It was in the skin which covers the cotyledon of the bean that we first ascertained the combination of tannin with an animal matter.

When macerated in tepid water for 24 hours, this tunic communicated to the water the property of reddening turnsole tincture, that of precipitating the solution of sulphate of iron blue, the solution of glue in yellowish-white, lime water in red flakes like oxide of iron, the acetate of lead in yellowish-white, and at the same time the property of experiencing no effect from the infusion of gall-nuts.

The characters of this water prove that it contains a free acid and tannin. We must here remark, that pure tannin precipitates iron brown, and that when it is joined with an acid it precipitates it blue.

The skins of garden beans submitted four different times to the action of large quantities of boiling water always communicated to it the above properties, but in a remarkably decreasing ratio.

* *Annales du Museum d'Hist. Nat.* tome xv. p. 77.

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When they no longer furnish any thing to the water, they preserve the property of becoming instantly of a deep black by the application of a little sulphate of iron: even when reduced to pulp and washed with boiling water, they still become black when in contact with this salt.

§ III. *First Result of the foregoing Trials: ulterior Experiments on the same Bodies.*

These experiments began to make us suspect that the tannin to which the effects above described are manifestly owing, was combined in the pellicles of the garden beans with some substance which opposed its solubility in water.

In order to ascertain, if possible, the nature of this substance, we put into a slight solution of potash a portion of pounded pellicles, and heated the mixture gently. The liquor soon became of a purple-red colour, as well as the substance of the pellicles. When filtered, and mixed to saturation with the acetic acid, this liquor precipitated a reddish matter in the form of flakes, having a gelatinous appearance; and it preserved but a very feeble colour itself.

The alkaline lixivium, thus cleared by the acetic acid of the substance which it had taken up from the pellicles of garden beans, did not give a blue colour to the solution of sulphate of iron; the mixture merely assumed a slight brownish colour; but the matter precipitated, on the contrary, became intensely black with this metallic solution, so that the tannin was really dissolved by the potash with the matter to which it was united, and afterwards precipitated with this same substance by the acetic acid, the action of which is here confined to the saturation of the potash. The pellicles of the beans, when cleansed by repeated washings, and distilled in a slow fire, furnished a liquor slightly acid, but from which caustic potash extricated a great quantity of ammonia: the produce of the distillation, before being thus mixed with the potash, gave a blue precipitate with sulphate of iron.

From these last experiments, it appears no longer doubtful to us, that the skins of garden beans actually contain a combination of tannin and an animal substance: we are even inclined to think that the greatest part of the parenchyme of those skins is formed of this combination.

Their charcoal yielded upon incineration a small quantity of ashes formed of carbonate of lime, phosphate with the same base, and oxidated iron.

The envelopes of the lentils presented precisely the same properties

properties and the same results with those of garden beans; we shall therefore dispense with any further details on this head.

§ IV. *Examination of the Leaves of the Indian Chesnut-tree.*

The leaves of the chesnut-tree, when deprived by alcohol of all which was soluble in it, having been afterwards subjected to the action of boiling water, communicated to it a light-brown colour, viscosity, and the property of frothing on agitation.

This liquor, when evaporated to dryness, left a small quantity of brownish matter, which was attached to the capsule in a thin shining layer like a gum, which burned with a crackling noise, exhaling a fetid vapour sensibly ammoniacal. Its solution in water precipitated iron black, and the acetate of lead yellow, but produced no effect in glue, nor in the infusion of gall-nuts.

We are of opinion that this substance is also a combination of animal matter and of tannin insoluble in alcohol, and by no means a gum, as the appearances denote; and this combination, as is the case with the pellicles of garden beans and lentils, is accompanied by a superabundance of tannin, which alcohol takes up. Thus, when we treat these substances directly by water, the free acid and tannin favour the solubility of those combinations saturated with the animal matter and tannin, which for the greater part remain insoluble, in the case in which we first treat these vegetable matters with alcohol.

The leaves of the chesnut-tree, when successively freed by alcohol and by water of every thing which is soluble in these two menstrua, and when dried and afterwards distilled, furnished an ammoniacal vapour so strong as to be scarcely supportable, and a very alkaline liquor. This last, when saturated by the muriatic acid, precipitated the solution of sulphate of iron in blackish blue; which proves that there still remained in these leaves a certain quantity of the combination of animal matter and tannin, which neither the alcohol nor the water could dissolve.

§ V. *Attempts made to imitate the vegetable Compound above described.*

Although we were well convinced, by the properties which we have detailed, and by various other experiments on the leaves of the chesnut-tree, that the matter in question is a true combination of animal principle and tannin, we we were nevertheless at a loss how to account for its solution in water; this combination being in fact but very little so of itself.

Supposing that the acids, which frequently exist in the plants, and the tannin itself when it is in excess, could favour this solution, we thought it right to make some experiments to verify this conjecture: after having saturated therefore a solution of the tannin of gall-nuts with animal glue dissolved in water, we treated the precipitate, when well washed, with acetic acid on the one hand, and with phosphoric acid on the other: these two acids produced, by means of a slight heat, the complete solution of the *tannate of gelatine*, or *tannated gelatine*.

The following are the properties exhibited to us by the solution made with the acetic acid: 1. If we raise the temperature to the boiling point, it becomes turbid and white like milk, but precipitates nothing: 2d. Neither the solution of gelatine nor that of tannin produces any change in it: 3d. It precipitates iron black, and the acetate of lead yellow: 4th. Alcohol very much dephlegmated precipitates the *tannate of gelatine* from its acid solution in white flakes, which become brown when they unite.

This last experiment shows, that when we treat with alcohol parts of vegetables which contain at the same time acids soluble in this agent, and *tannate of gelatine* or albumen, the first are taken up, and the other becomes insoluble in the water, if there is not in the vegetable matter some other acid insoluble in alcohol. Thus, when we treat these kinds of plants directly by water, we obtain, as we have said above, much more of the combination of tannin and animal matter existing in the solution.

It will be found from what precedes, that there is the most remarkable analogy between the properties of tannin and animal gelatine, and those of the natural combination which we have discovered in several astringent vegetables: only there is more tannin in the natural combination: the artificial contains more animal matter, and yields more ammonia upon distillation.

§ VI. *View as to the Existence of this Compound in many Vegetables, and as to its Uses.*

Although we have only examined the combination in question in a small number of vegetables, we have reason to think that it is very common among all of them. It is that which sometimes makes the vegetable infusions turbid, or is separated from them in the form of pellicles of various

 thickness,

thickness, when they are boiled or evaporated. It is to this that the sediments are owing which are formed in some infusions when they cool, and which are dissolved afterwards with more or less difficulty. It is this substance also, perhaps, which, as well as some other combinations of different vegetable principles with which it may be mixed, has been taken for more than half a century for a peculiar principle, and which has been denominated the extract of plants. This is certainly the case with the astringent plants, and particularly the roots, wood, bark, &c. which have this character.

It would be very interesting to examine with care, and with the views above pointed out, the extracts which are prepared by the apothecaries, and to inquire if the name of *extractive matter*, adopted since 1787, in order to designate homogeneous principle in plants, ought to be retained in the present state of science.

While we expect that something more will be done on this subject, we beg leave to assure our readers, that the vegetable substances employed as body-colours in dyeing, and in giving a brownness to common cloths, contain a combination of tannin and animal matter: of this number are chiefly the bark of the alder tree, of the ash, the green shell of walnuts, &c.: to these we may add the Indian chesnut-tree, since the compound of tannin contained in its leaves unites very easily with wool, silk, and even cotton; and the colours thus given appear very solid.

We are inclined to think, therefore, that the theory of dyeing may acquire some improvements from a precise knowledge of a compound hitherto unknown in plants, and which acts a peculiar part in the production of colours applied without previous preparation upon cloths.

It results, for instance, from our inquiries, that, in order to fix the colouring matter of woods and barks upon cloths of vegetable origin, it would perhaps be advantageous to prepare them first with animal liquors, in order to precipitate more abundantly the tannin and the tannated substance, which it renders more soluble: there is even reason to believe that this process is already in use in some manufactories.

Might we not also be permitted to ascribe to the same combination a physiological use with respect to seeds, and to acknowledge in the chemical composition of their envelopes an anxiety on the part of nature to preserve them by covering them with an insoluble and imputrescible substance? What we have discovered as belonging to the skins of of garden beans and lentils, will unquestionably be found in a great variety of other vegetables when subjected to the same examination. Those which do not present a similar nature in their envelopes, sometimes exhibit ligneous or horny envelopes, or dry pellicles clothed or penetrated with a waxy substance, or with bitter and aromatic oils, in which the naturalist will recognise a similar defensive and preservative property.

XXIII. *On a Case of nervous Affection cured by Pressure of the Carotids; with some physiological Remarks.* By C. H. Parry, *M.D.F.R.S.**

Observing that the Royal Society, of which I have the honour to be a member, occasionally receives communications illustrative of the laws of animal life, which are indeed the most important branch of physics, I take the liberty of calling their attention to a case, confirming a principle which I long ago published, and which, I believe, had never till then been remarked by pathologists.

About the year 1786, I began to attend a young lady, who laboured under repeated and violent attacks, either of head-ache, vertigo, mania, dyspnœa, convulsions, or other symptoms usually denominated nervous. This case I described at large to the Medical Society of London, who published it in their Memoirs, in the year 1788. Long meditation on the circumstances of the case led me to conclude, that all the symptoms arose from a violent impulse of blood into the vessels of the brain; whence I inferred, that as the chief canals conveying this blood were the carotid arteries, it might perhaps be possible to intercept a considerable part of it so impelled, and thus remove those symptoms which were the supposed effect of that inordinate influx. With this view, I compressed with my thumb one or both carotids, and uniformly found all the symptoms removed by that process. Those circumstances of rapidity or intensity of thought, which constituted delirium, immediately ceased, and gave place to other trains of a healthy kind; head-ache and vertigo were removed, and a stop was put to convulsions, which the united strength of three or four attendants had before been insufficient to counteract.

That this extraordinary effect was not that of mere pressure, operating as a sort of counteracting stimulus, was evi-

* From the Philosophical Transactions for 1811, part i.

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dent; for the salutary effect was exactly proportioned to the actual pressure of the carotid itself, and did not take place at all, if, in consequence of a wrong direction either to the right or left, the carotid escaped the effects of the operation.

This view of the order of phænomena was, in reality, very conformable to the known laws of the animal œconomy. It is admitted, that a certain momentum of the circulating blood in the brain is necessary to the due performance of the functions of that organ. Reduce the momentum, and you not only impair those functions, but, if the reduction go to a certain degree, you bring on syncope, in which they are for a time suspended. On the other hand, in nervous affections, the sensibility and other functions of the brain are unduly increased; and what can be more natural than to attribute this effect to the contrary cause, or excessive momentum in the vessels of the brain? If, however, this analogical reasoning has any force in ascertaining the principle, I must acknowledge that it did not occur to me till twenty years afterwards, when a great number of direct experiments had appeared to me clearly to demonstrate the fact.

From various cases of this kind, I beg leave to select one which occurred to me in the month of January 1805.

Mrs. T. aged 51, two years and a half beyond a certain critical period of female life, a widow, mother of two children, thin, and of a middle size, had been habitually free from gout, rheumatism, hæmorrhoids, eruptions, and all other disorders, except those usually called nervous, and occasional colds, one of which, about two years and a half before, had been accompanied with considerable cough, and had still left some shortness of breathing, affecting her only when she used strong muscular exertion, as in walking up stairs, or up hill.

In February 1803, after sitting for a considerable time in a room without a fire, in very severe weather, she was so much chilled as to feel, according to her own expression, "as if her blood within was cold." In order to warm herself, she walked briskly for a considerable time about the house, but ineffectually. The coldness continued for several hours, during which she was seized with a numbness or sleepiness of her left side, together with a momentary deafness, but no privation or hebetude of the other senses, or pain or giddiness of the head. After the deafness had subsided, she became preternaturally sensible to sound

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in the ear of the affected side, and felt a sort of rushing or tingling in the fingers of the left hand, which led her to conclude that "the blood went too forcibly there."

Though the coldness went off, what she called numbness still continued, but without the least diminution of the power of motion in the side affected. In about six weeks, the numbness extended itself to the right side.

Among various ineffectual remedies for these complaints, blisters were applied to the back, and the inside of the left arm above the elbow. The former drew well. The latter inflamed without discharging; so that a poultice of bread and milk was put on the blistered part. After this period, the muscles of the humerus began to feel as if contracted and stiff; and these sensations gradually spread themselves to the neck and head, and all across the body, so as to make it uncomfortable for her to lie on either side, though there was no inability of motion.

She now began to be affected with violent occasional flushings of her face and head, which occurred even while her feet and legs were cold, together with a rushing noise in the back of the head, especially in hot weather, or from any of those causes which usually produce the feelings of heat.

It is difficult to give intelligible names to sensations of a new and uncommon kind. That which this lady denominated numbness, diminished neither the motion nor the sensibility of the parts affected. It was more a perception of tightness and constriction, in which the susceptibility of feeling in the parts was in fact increased; and the skin of the extremities was so tender, that the cold air produced a sense of uneasiness, the finest flannel or worsted felt disagreeably coarse, and the attempt to stick a pin with her fingers caused intolerable pain.

In the month of September 1803, not long after the application of the blisters, she experienced, in certain parts of the left arm and thigh, that sensation of twitching which is vulgarly called the "life blood," and which soon extended itself to the right side. Shortly afterwards, she began to perceive an actual vibration or starting up of certain portions of the flexor muscles of the fore-arm, and of the deltoid on the left side; not so, however, as to move the arm or hand.

This disorder had continued with little variation to the period of my first visit. The vibrations constantly existed while the arm was in the common posture, the fore-arm and

and hand leaning on the lap. If the arm were stretched strongly downwards, the vibration of the flexors ceased, but those of the deltoid continued. The arm being strongly extended forwards, all ceased; but returned as soon as the muscles were relaxed. The vibrations were of different degrees of frequency, and at pretty regular intervals, usually about 80 in a minute. They were increased in frequency and force by any thing which agitated or heated the patient, and were always worse after dinner than after breakfast. The pulse in the radial artery was 80 in a minute, and rather hard. That in the carotids was very full and strong; and each carotid appeared to be unusually dilated for about half an inch in length, the adjacent portions above and below being much smaller, and of the natural size. I much regret that I find in my notes of this case, no inquiry whether there was any coincidence between the systoles of the heart and the muscular vibrations. The patient's feet were usually cold, and her head and face hot. The feeling in her limbs was much as I have above described, except that the sensibility was somewhat less acute than it had been, and she complained of a tightness all over her head, as if it had been bound with a close night-cap. Her sleep was usually sound on first going to bed, but afterwards, for the most part, interrupted by dreaming. Bowels generally costive: appetite moderate: no flatulency or indigestion: tongue slightly furred, without thirst: urine variable, but generally pale.

The late Mr. George Crook, surgeon, was present while I made these examinations; and when we afterwards conversed together, I remarked to him, that if my theory of the usual cause of spasmodic or nervous affections were well founded, I should probably be able to suppress or restrain these muscular vibrations of the left arm, by compressing the carotid artery on the opposite or right side; while little effect might perhaps be produced by compressing the carotid of the side affected. The event was exactly conformable to my expectation. Strong pressure on the right carotid uniformly stopped all the vibrations, while that on the left had no apparent influence. I may add that these experiments were afterwards, at my request, repeated on this lady in London by Dr. Baillie, and, as he informed me in a letter, with a similar result.

It is perfectly well known to many of the learned members of this society, that irritations of the brain, when of moderate force, usually exhibit their effects on the nerves or muscles

muscles of the opposite side of the body; and in the case before us, it is difficult to understand how the suspension of these automatic motions could have been produced by this pressure of the opposite carotid, in any other way than by the interruption of the excessive flow of blood through a vessel morbidly dilated; in consequence of which interruption, the undue irritation of the brain was removed, and the muscular fibres permitted to resume their usual state of rest.

From these and many other similar facts, I am disposed to conclude, that irritation of the brain, from undue impulse of blood, is the common though not the only cause of spasmodic and nervous affections; and I can with the most precise regard to truth add, that a mode of practice conformable to this principle has enabled me, during more than twenty years, to cure a vast number of such maladies which had resisted the usual means.

An investigation of all the modifications of the principle itself, and of its numerous relations to therapeutics, would be inconsistent with the views of the Royal Society, and must be reserved for another place.

Bath, Dec. 8, 1810.

XXIV. *Memoir on the Action of Elastic Fluids upon Meat. By M.* HILDEBRAND.

[Concluded from p. 76.]

Pure Carbonic Acid Gas obtained by the Calcination of Chalk. Over Mercury.

1st day.—THE meat became of a crimson red, similar to that in the hydrogen gas. 2d to the 11th day there was not any sensible change; the meat had the appearance of being quite fresh. 15th to 22d day, it became paler. 51st day, the meat has become uniformly pale, and has the appearance of cooked meat, and something of the same consistence; it has not the least smell nor any mark of putridity, it is neither moist nor sticky. The gas was absorbed by lime, except a small residue which did not amount to more than 0·01. If this experiment is repeated in vessels stopped with cork, and some meat is shut up in one bottle while the gas is hot, and in another not until after the gas is become cold; it will be found that the meat put into the cold gas will be in good preservation on the 60th day, but will have acquired a disagreeable odour, whereas that shut

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up in the hot gas putrefies on the 30th day, and is entirely spoiled by the 60th.

Nitrous Gas. Above Mercury.

1st day. The meat appeared much redder. 51st day. The meat had preserved its fine colour, and was very firm; the liquor which trickled from it had a fine red colour, and had deposited a small quantity of whitish matter, resembling fat, although the meat did not contain any. 67th day, June 10. The meat still kept its bright colour, therefore I had not taken it out, in order to see how long time was required for its decomposition.

Third Set.

The temperature was the same as in the last set of experiments.

Oxygen Gas placed over Water, the Recipient containing 28½ Cubic Inches.

1st day. The meat became of a beautiful red colour. 2d, 3d, and 4th days, the meat preserved its colour, and did not begin to putrefy. 6th day, I could distinguish small transparent drops: these increased in size and number the 7th day; on the 8th they were turbid, and appeared red. 9th day. Putrefaction is apparent on the whole surface, which begins to liquefy. The gas is much diminished in bulk. There can be no doubt but that the increase of temperature is the cause of the more speedy putrefaction.

10th day. The gas when measured by the gasometer had diminished 7 cubic inches. Having subjected it to the action of lime water, it diminished 6½ inches more. Supposing that the 7 cubic inches absorbed by the water were carbonic acid gas, we see that there were 13½ cubic inches of oxygen consumed, which must have formed 18·75 cubic inches of carbonic acid gas. Having examined the remaining 15 cubic inches with nitrous gas, I found that they consisted of 5·4 of azotic gas and 9·6 of oxygen; the 28½ cubic inches of oxygen then were thus accounted for:

13·5 in the carbonic acid.
5·4 in the azotic gas.
9·6 oxygen remaining.

28·5

Atmospheric Air.

The meat putrefied and was decomposed on the 48th day; the water rose up considerably, and absorbed 21 cubic inches

inches out of the 96 which the receiver contained. The experiment being interrupted, I had no opportunity of continuing my observations.

Pure Hydrogen Gas.

1st day. The meat became of a red poppy colour. 4th day. No alteration, except that the meat appeared to be dried up. 6th day. Some mouldiness might be observed, which was increased on the 7th day. From the 8th to the 41st day no change could be perceived, except that about the 20th day the mouldiness disappeared. The flesh resembled meat that had been salted without nitre, and afterwards smoked; there was not the least bad scent. The gas did not render lime water turbid; it burned with considerable force.

It will be seen by this recital how necessary it is to repeat these experiments separately, in order to obtain correct results. The following conclusions may be drawn from them:

1. That hydrogen preserves and even increases the cohesion of dead flesh by drying it; that, on the contrary, oxygen diminishes this cohesion by rendering the meat moist and flaccid. It is remarkable that hydrogen preserves the cohesion of the fibres, even above water, when the gas is loaded with humidity.

2. That the meat undergoes alteration, and becomes dissolved much sooner in oxygen when it contains azote, as in atmospheric air, and in the gas obtained from nitrate of potass, than when the gas is quite pure.

3. That nitrous gas strongly resists putrefaction, holding the next place to hydrogen, and after it carbonic acid gas.

4. That meat becomes putrid less readily in oxygen gas than in atmospheric air; but that when putrefaction does once take place, it goes on more rapidly than in atmospheric air, and the vapour arising from it is much more offensive.

5. That the colour of meat becomes brown in hydrogen, and grows brighter in oxygen and azotic gas.

6. That hydrogen gas, nitrous and carbonic acid gases, do not appear to suffer any change by being enclosed over meat.

7. That oxygen gas, either pure or combined with azote, is converted into carbonic acid gas.

8. That one part of the oxygen gas still retains its properties as in other combustions.

9. That during the putrefaction of meat in oxygen gas, azote

azote is obtained, and that this azote is either disengaged from the meat, or the oxygen is converted into azote.

10. When the meat begins to corrupt in the hydrogen, there is disengaged from it carbonic acid; but when putrefaction did not take place, none of it was formed.

11. That upon the meat in oxygen gas are formed small drops of water, which resemble the pustules of the small-pox.

My next researches shall be directed to ascertain all these facts I have announced, and especially to satisfy myself, if the carbonic acid gas found in the hydrogen exists in the meat; to determine the influence of light, and the shining properties of putrid meat.

XXV. *Letter from Dr.* Hutton *on the Calculations for ascertaining the Mean Density of the Earth.*

To Mr. Tilloch.

Sir, Accustomed, as I constantly am, to peruse with much pleasure and profit, the numerous valuable philosophical dissertations that adorn your Magazine, I am truly sorry to have occasion to make a remark on a paragraph in your Number for the month of June last, which has not done justice to my labours, and which has doubtless been admitted unawares into your work; as I cannot for a moment suppose it possible, that either yourself or Mr. Davy would intentionally write or say one word to do injustice or to give pain to me or to any one else. The paragraph alluded to is in your excellent account of Mr. Davy's very ingenious Lectures on Geology, given at the Royal Institution, being in page 469 of your last volume; and runs thus: "But what are the agents concerned in these great and awful elevations? The discoveries of Mr. Davy prove that the earths and alkalies consist of metals united to oxygen, or pure air; and these metals are highly inflammable, some of them so much so as to burn even in contact with water. The mean density of the earth, as determined by Mr. Cavendish and Dr. Maskelyne, would lead to the conclusion that the interior consists principally of metallic matter, which may be alloys of the metals of the earths and alkalies with the common metals:—and such an assumption, says Mr. Davy, would offer a ready explanation of subterranean heat and volcanic explosions; for, supposing water from the sea or lakes to act upon these inflammable masses, elastic matters

matters would be rapidly disengaged, the surface would be broken, air would act upon the metals, inflammation would take place, and the result would be lava, the metals of the earths combined with oxygen."

Now, Mr. T., will it not be understood by every reader of this paragraph, who may not have been otherwise informed on these matters, that Mr. C. and Dr. M. have been the only, or the first, or the principal determiners of the mean density of the whole earth, and the authors of the idea of immense quantities of metallic matter about the central parts of it? I cannot suppose that the paragraph has been so worded by suppressing his name, purposely to throw the slightest disparagement on the labours of the person who first and chiefly computed that density, and suggested in consequence that idea of the metallic interior parts: yet, it seems strange that the name of Dr. Hutton should be omitted in the mention of a circumstance in which he was the chief efficacious person, especially when other names, of inferior concernment in the business, are so pointedly announced. I trust, therefore, that Mr. T. will have the goodness to allow me the favour of doing myself barely justice, in thus publicly stating the claim to which my labours have entitled me, as will appear in the following short history of this business.

About the year 1774, there was much conversation among some of the most scientific members of the Royal Society, about the universal attraction of all matter, and in devising some general and familiar proofs of it. It was then concluded that it would be a very decisive, and, indeed, palpable proof, if it could be experimentally shown that any hill attracted a plummet, drawing it sensibly aside from the perpendicular direction, towards itself. In pursuing this idea, it was soon perceived, that as any hill, especially in this country, is very small in comparison with the whole earth, the effect of its attraction, in drawing the plummet aside, must be extremely small, probably but a very few seconds of a degree. Besides the magnitude of a hill, it was considered that the effect would be increased by the circumstances of its shape; such as, that it should be of a form very long and narrow, the sides very steep and lofty; on all which accounts it might be expected to produce the greater effect on the plummet placed near the middle of a side of the mountain. The trial having been resolved on, several persons were directed to search through the island, and to make inquiries for hills having properties nearest approaching to those above mentioned. After several reports to the Royal Society, Mr. Smeaton announced that he had discovered the mountain Schehallien, one of the Grampian hills in the north of Scotland, possessing the desired properties in a very eminent degree; being a very lofty and narrow ridge, very steep, extending a great length east and west, and very narrow from north to south.

This hill was in consequence deemed sufficiently convenient for making the experiment; and a person, who had been an assistant to Dr. Maskelyne at the Royal Observatory, was engaged by the Society, and sent down to Scotland to take the necessary measures about the hill, to ascertain its shape and magnitude by horizontal measurements, and by vertical sections in a great many directions and situations; and lastly, by placing a proper instrument and plummet against the middle of the sides of the hill, to observe, by zenith distances, the deviation of the plumb-line towards the hill. Before the survey and observations were quite completed,—at the request of the Society, Dr. Maskelyne himself went down to Scotland, to see how the business was carried on; and brought back the account of the survey, with the report that, having tried the plummet on the opposite sides of the hill, each side attracted it between 5 and 6 seconds from the perpendicular, and, in fact, that the sum of the two opposite attractions was just equal to $11\frac{6}{10}$ seconds.

Thus, then, the original question was satisfactorily answered in the affirmative, viz. that the hill, a mass of dense rocks, did sensibly attract the plummet, and draw it aside from the perpendicular direction of the earth's gravitation, and that by a certain quantity.

The next consideration was, whether and how these observations and measurements could be employed, in comparison with the magnitude and effects of the whole globe of the earth, to determine its mean density, in comparison with that of the mountain. This indeed was the grand question, a point of the highest importance to natural philosophy, of novel and of the most delicate and intricate consideration, as well as a work of immense labour. Here were to be calculated mathematically the exact magnitude of the hill, its shape and form in every respect, the position and situation of all its parts, the various elevations and depressions, and the attraction on the plummets, by every point and particle in the hill, as well as of the neighbouring mountains on every side of it. Then there was to be calculated, in like manner, the attraction of the whole magnitude and mass of the earth, on the same plummets. Lastly, the

the proportion of these two computed attractions was to be compared with that of the observed effects on the plummets, viz. the lateral deviation by the hill in comparison with the perpendicular direction of gravity, which comparison of the computed and observed effects would give the ratio of the densities, namely, of the hill and the earth.

The magnitude and novelty of these nice calculations, the requisite portion of science and ingenuity for making them with effect, were such as appalled every mind, and every one shrank from the task; when, at the request of the President and Council of the Society, I undertook the performance; and after incessant labour, during the course of a year, produced the result of the whole, to the entire satisfaction of all the Society. The account of these calculations was published in the Philosophical Transactions for the year 1778, and in volume xiv. of my Abridgement of these Transactions; and, though in a very condensed form, occupied no less than a hundred quarto pages in that work, containing only the results of many thousands of intricate calculations. The conclusion from all which was, that the mean density of the whole mass of the earth is nearly double that of the mountain, being to the former in the proportion of 9 to 5; whence it appears that the density of the earth is about five times that of water, considering the specific gravity of the rock of the hill as between 2·7 and 2·8, as it really is; and hence also I inferred, as a probable deduction, the necessity of the interior of the earth consisting, in a great measure, of metallic matter. Thus, then, this grand desideratum was minutely determined, for the first time, by the English nation, and on a large scale of measurement and calculation.

Besides the above computation, no other experiments have been made for the same purpose, except an attempt made many years afterwards by Mr. Cavendish, and published in the Philosophical Transactions for the year 1798. This method was by means of two balls of lead fixed to the ends of a straight bar, which was suspended horizontally at its middle point by a fine wire. Then another ball being held in a position opposite to one of the former, its minute attraction was supposed to cause, after a long time acting, a very small torsion or twisting of the wire, suppose about a quarter round; from which twisting Mr. C. deduced nearly the same conclusion as by the former grand experiment, viz. that the density of the earth is about five times the density of water.

Thus then, it appears, sir, that Dr. Maskelyne never determined the mean density of the earth: that Mr Cavendish's late attempt can be considered only as a pretty and amusing little experiment, tending to corroborate the result of the calculations made on the large experiment, by, sir,

Your obliged humble servant,

CHARLES HUTTON.

London, August 14, 1811.

XXVI. *Description of a new Method of applying the Filtering Stone for purifying Water. By Mr.* WILLIAM MOULT*.

SIR, IF you think the following information, relative to a new method of filtering water, is deserving of the attention of the Society of Arts, &c. I wish you would lay it before them. My objections to the old method of filtering by putting water into the filtering-stone are, that the dirt falls to the bottom, and fills up or chokes the pores of the filtering-stone, so that the stone requires frequently to be cleaned with a brush and sponge to allow the water to pass, after which the water passes through the stone in a muddy state for two or three days; it likewise requires to be frequently filled, and as it empties, less water comes in contact with the stone, and therefore a smaller quantity only, in such a state, can pass through. Likewise a filtering-stone used in the common way soon becomes useless, from the filth insinuating itself into the internal parts of the stone, out of the reach of the brush.

In the method I propose and practise, the filtering-stone is placed within the water to be purified, which presses upon the outside of the filter, and the stone does not require to be supported in a frame, as it needs only to stand within the water cistern; it will thus filter, in an equal time, double the quantity of water procured in the common mode; it fills itself, and requires no cleaning. I have upon this plan used one for more than three years with great success.

I am, sir,

Your humble servant,

No. 37, Bedford-square, April 18, 1810. WILLIAM MOULT.

To C. Taylor, M.D. Sec.

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* for 1810.——The Society voted their silver medal to Mr. William Moult for this communication.

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Reference to the Drawing of Mr. Moult's Filtering Apparatus, Fig. 1. Pl. IV.

AA is the cistern containing the water to be filtered; the filtering-stone B is suspended in the cistern by a ring around the inside of it, which catches the projecting part of the stone; the water in the cistern filters through into the stone. D is a syphon which conveys the filtered water from the inside of the stone into a cistern E, which is the reservoir for clean water. *d* a cock to draw it off as it is wanted. By this mode of filtration the impurities of the water are deposited in the bottom of the cistern A, instead of being left in the bottom of the stone as in the usual mode.

XXVII. *Method of raising a loaded Cart when the Horse in the Shafts has fallen.* By Mr. BENJAMIN SMITH*.

SIR, I HAVE taken the liberty of sending you a model with a brief explanation of the utility of my invention, in order that it may be laid before the Society instituted for the Encouragement of Arts, &c. to whose comprehensive judgement and abilities I with great deference submit it for their determination, whether they think it likely to be attended with the success and utility which I flatter myself it deserves. From the simplicity of the construction and the trivial expense attending it, I presume there will be no bar to its universal adoption. I respectfully submit it to the discernment and decision of the Society, who will, I am convinced, give it all the merit and approbation it may deserve.

The reason which prompted me to undertake this business is, the having seen a horse which had fallen down under the immense weight of a heavy loaded cart, where it lay for a considerable time in that painful and dangerous situation, which naturally excited compassion even in the most obdurate heart. Every person frequenting the streets of this metropolis must have witnessed similar scenes; and indeed it surprises me that long before now some expedients have not been publicly suggested to remove the mischief arising from such occurrences, considering the great encouragement that is given in this enlightened age to all useful improvements.

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1810.—The Society voted fifteen guineas to Mr. Benjamin Smith for this communication.

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Having conversed on this subject with persons who possess considerable knowledge of horses, and who constantly employ these noble animals, I find that horses remaining so long as they usually do in such improper positions, and from being often dragged a considerable distance by fruitless endeavours to raise them, are much endangered in their health and lives, and that their situation upon the stones is more prejudicial than the injury received by the fall.

I flatter myself that my method will be found to raise the whole weight of the cart, and a considerable part of that of the horse, in the short space of three or four minutes from the moment of the accident, by means simple and useful, and within the reach of the meanest capacity to execute; and that the whole apparatus will not cost above fifty shillings, and will last many years. Requesting your kind attention,

I am, sir,

Your most obedient servant,

No. 11, Turnham-Place, Curtain-Road, Shoreditch, London, Dec. 13, 1809. BENJAMIN SMITH.

To C. Taylor, M.D. Sec.

Advantages derivable from this Invention.

1.—The invention is of itself so simple, and the operation so conspicuous at the first view, that the whole process may be easily comprehended and executed.

2.—The apparatus may be fitted with little difficulty to any cart now in use for heavy loads, such as bricks, coals, corn, or the like.

3.—The chains which lead from the uprights at the back part of the cart to the fore part of it on each side are for the purpose of taking the purchase therefrom, and making the back part of the cart act as a lever at the time the horses are drawing behind, which without fail, with the strength of one, two or three horses fastened there to raise the one which is down in the shafts, will instantly assist him to get upon his feet.

4.—The number of horses to draw a cart are usually in proportion to the weight contained therein; therefore supposing three horses are employed to draw it, and the shaft-horse falls, the carman has only to unhook the two leaders and then hook them to the short chain at each side of the back of the cart, and with their strength the fallen horse will be so relieved from the weight as to raise himself without further assistance.

5.—The

5.—The same principle may be applied in different ways from what I have shown in the model; for instance, another mode may be adopted by framing the tail-board of the cart strong enough to bear the purchase, and with the use of the two side-chains above mentioned it may be made to answer the purpose.

Another plan, though more expensive, is by obtaining two wrought-iron uprights to be fixed as substitutes for the truss-staffs at the back part of the cart, with a hole in the top of each to receive an iron rod, which is occasionally to be introduced, reaching from one side of the cart to the other, connecting the two uprights together; when in action the two side-chains to be used as in other cases.

Reference to the Drawing of Mr. Smith's Method of raising up a Horse when fallen down in the Shafts of a loaded Cart, Fig. 2. Pl. IV.

A is the wheel, and B the shafts of a cart, such as is used in London; *c* the side-rails; at the end of the body an iron stancheon or truss-staff, *a*, is fixed by a hinge at the lower end, and at the upper end it is supported by a chain *b*, extended from the fore part of the body of the cart: this diagonal chain forms a firm support to the stancheon. This is all the addition made to the common cart, and is used in the event of the shaft-horse falling, by hooking the traces of the other horses to a chain *d*, also fixed to the stancheon: the power of these horses, applied at this height above the fulcrum, will have a great purchase to elevate the shafts and set the fallen horse at liberty, as is evident from an inspection of the figure. The stancheon moves on a joint on its lower end, and the oblique chain unhooks at *b*; the end can be connected with a short piece of chain *e* fastened to the last of the side-rails; the stancheon now takes the position of the dotted lines *f*, and the short chain which hangs down perpendicular from the end of it, may be taken hold of by any number of men, to weigh upon and raise the cart in cases where the horses cannot conveniently be applied; the men will in this manner have much greater effect than merely (as is the common practice) weighing on the hind part of the cart.

When the chain is completely detached, and the stancheon suffered to hang down perpendicularly, it forms a prop to support the cart steady whilst it is unloaded. It should be observed, that though only one stancheon appears in the figure, there are in fact two, one being placed on each side of the cart.

XXVIII. *Method of Ventilating Mines or Hospitals, by extracting the foul Air from them. By Mr.* John Taylor, *of Holwell-House, near Tavistock**.

Sir, I send you herewith a drawing and description of a machine of my invention for the ventilation of mines, with a view to their being laid before the Society for the Encouragement of Arts, &c. and hope they will meet with their approbation.

I am, sir,
Your obedient servant,
John Taylor.

Holwell, April 9, 1810.

To C. Taylor, M.D. Sec.

On the Ventilation of Mines, with the Description of a new Machine for that Purpose. See Pl. IV. Fig. 3.

Next in importance to the means employed for draining underground works from water, may be reckoned those which are intended to afford a supply of pure air, sufficient to enable the workmen to continue their operations with ease and safety to themselves, and to keep up, undiminished, the artificial light upon which they depend. It is well known, indeed, to all who are practically engaged in concerns of this kind, that men are frequently obliged to persevere in their labour, where a candle will scarcely burn, and where not only their own health materially suffers in the end, but their employers are put to considerable additional expense by the unavoidable hindrance and the waste of candles and other materials.

I mean to confine the following remarks to such mines as are worked upon metalliferous veins, according to the practice of this district, and that of the great seat of mining in the neighbouring county of Cornwall, from which indeed ours is borrowed. We find then that a single shaft, not communicating by levels to another, can hardly be sunk to any considerable depth, nor can a level (or, as the foreign miners call it, a gallery) be driven horizontally to any great distance, without some contrivance being had recourse to for procuring currents of air to make up the deficiency of oxygen, which is so rapidly consumed by respiration and combustion in situations like these, where otherwise the whole remains in nearly a stagnant condition.

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* for 1810.——The Society's silver medal was voted to Mr. John Taylor for this communication.

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We are here unacquainted with the rapid production of those gases which occasionally in the collieries are the cause of such dreadful effects; such as hydrogen gas, or the fire-damp; carbonic acid, or the choke-damp; the inconvenience we experience takes place gradually as we recede from the openings to the atmosphere, and seems to arise solely from the causes which I have before assigned, though it is found to come on more rapidly in certain situations than in others.

The most obvious remedy, and that which is most frequently resorted to, is the opening a communication either to some other part of the mine, or to the surface itself; and as soon as this is done, the ventilation is found to be complete, by the currents which immediately take place, often with considerable force, from the different degrees of temperature in the subterranean and upper atmospheres; and these currents may be observed to change their directions as the temperatures alternate.

The great objection to this mode of curing the evil is, the enormous expense with which it it most commonly attended. In driving a long level, or tunnel, for instance, it may happen to be at a great depth under the surface, and the intervening rock of great hardness; in such a case every shaft which must be sunk upon it for air alone, where not required (as often they might not) to draw up the waste, would cost several hundred pounds; or in sinking a shaft it may be necessary, at an expense not much less, to drive a level to it from some other for this purpose alone.

To avoid this, recourse has been had to dividing the shaft or level into two distinct parts, communicating near the part intended to be ventilated, so that a current may be produced in opposite directions on each side the partition; and this, where room is to be spared for it, is often effectual to a certain extent. It is found, however, to have its limits at no very great distance, and the current at best is but a feeble one, from the nearly equal states of heat in the air on each side. The only scheme besides these, that I know of, has hitherto been to force down a volume of purer air, through a system of pipes placed for the purpose, and a variety of contrivances have been devised for effecting this; most of them are so old that they may be found described in Agricola's work *De Re Metallica*. The most common are by bellows worked by hand; by boxes or cylinders of various forms placed on the surface with a large opening against the wind, and a smaller one communicating with the air-pipes by a cylinder and piston working in it, which when driven by a sufficient force has great power. But the cheapest

cheapest and most effectual scheme for this purpose, where circumstances will admit of its being applied, is one which I adopted some time since in the tunnel of the Tavistock canal. It is by applying the fall of a stream of water for this purpose, and it has been long known that a blast of considerable strength may be obtained in this manner, which has the advantage of being constant and self-acting. The stream being turned down a perpendicular column of pipes, dashes in at a vessel so contrived as to let off the water one way, with an opening at another part for the air, which being pressed into it by the falling water, may be conveyed in any direction, and will pass through air-pipes with a strong current, which will be found efficacious in ventilating mines in many instances, as it has likewise, in some cases, been sufficient for urging the intensity of fires for the purposes of the forge. It is easily procured where a sufficient fall is to be had; and the perpendicular column can be so fixed as that the water from the bottom may pass off, while the air is forced into a pipe branching from the air-vessel, and which is to be continued to the part of the mine where the supply of fresh air is required.

I have found, however, that the forcing into vitiated air a mixture of that which is purer, even when the best means are used, though a measure which affords relief, is not in bad cases a complete remedy; and, where the operation depends on manual labour, or any means that are not unremitted in their action, it becomes quite ineffectual. The foul air, charged with the smoke of gunpowder used in blasting, and which it strongly retains, is certainly ameliorated by the mixture of pure air, but is not removed. While the blast continues, some of it is driven into the other parts of the mine; but when the influx of pure air ceases, it returns again: or if during the influx of pure air a fresh volume of smoke be produced by explosions which are constantly taking place, it is not until some time afterwards that it becomes sufficiently attenuated for the workmen to resume their stations with comfort.

A consideration of these circumstances led me to think that the usual operation of all ventilating engines ought to be reversed, to afford all the advantages that could be desired; that, instead of using the machines which serve as condensers, exhausters should be adopted; and thus, instead of forcing pure air into that in a vitiated state, a complete remedy could only be had by pumping out all that was impure as fast as it became so.

Many modes of doing this suggested themselves to me, by by the alteration of the machines commonly applied, and by producing an ascending stream of air through pipes by a furnace constructed for the purpose. The latter mode would, however, have been here expensive in fuel as well as in attendance; and the others required power to overcome the friction of pistons, and so on, or considerable accuracy in construction.

I at last erected the machine, of which the annexed is a drawing; which, while it is so simple in construction, and requires so small an expense of power, is so complete in its operation, and its parts are so little liable to be injured by wear, that, as far as I can imagine, nothing more can be desired where such an one is applied. This engine bears considerable resemblance to Mr. Pepys's gazometer, though this did not occur to me until after it was put to work. It will readily be understood by an inspection of the drawing, (Pl. IV. fig. 3,) where the shaft of the mine is represented at A; and it may here be observed, that the machine may be as well placed at the bottom of the shaft as at the top, and that in either case it is proper to fix it upon a floor, which may prevent the return of the foul air into the mine, after being discharged from the exhauster: this floor may be furnished with a trap door, to be opened occasionally for the passage of buckets through it.

B, the air-pipe from the mine passing through the bottom of the fixed vessel or cylinder C, which is formed of timber and bound with iron hoops; this is filled with water nearly to the top of the pipe B, on which is fixed a valve opening upwards at D.

E, the air- or exhausting-cylinder made of cast-iron, open at the bottom and suspended over the air-pipe, immersed some way in the water. It is furnished with a wooden top, in which is an opening fitted with a valve likewise opening upwards at F.

The exhausting-cylinder has its motion up and down given to it by the bob G, connected to any engine by the horizontal rod H, and the weight of the cylinder is balanced, if necessary, by the counterpoise I.

The action is obvious.—When the exhausting-cylinder is raised, a vacuum would be produced, or rather the water would likewise be raised in it, were it not for the stream of air from the mine rushing through the pipe and valve D. As soon as the cylinder begins to descend, this valve closes, and prevents the return of the air which is discharged through the valve F.

The quantity of air exhausted is calculated of course from the

the area of the bore of the cylinder, and the length of the stroke.

The dimensions which I have found sufficient for large works are as follow:

The bore of the exhausting cylinder two feet.

The length six feet, so as to afford a stroke of four feet.

The pipes which conduct the air to such an engine ought not to be less than six inch bore.

The best rate of working is from two to three strokes a minute; but if required to go much faster, it will be proper to adapt a capacious air-vessel to the pipes near the machine, which will equalize the current pressing through them.

Such an engine discharges more than two hundred gallons of air in a minute; and I have found that a stream of water supplied by an inch and a half bore falling twelve feet, is sufficient to keep it regularly working.

A small engine to pump out two gallons at a stroke, which would be sufficient in many cases, could be worked by a power equal to raising a very few pounds weight, as the whole machine may be put into complete equilibrium before it begins to work, and there is hardly any other friction to overcome but that of the air passing through the pipes.

The end of the tunnel of the Tavistock canal, which it was my object to ventilate, was driven into the hill to a distance of near three hundred yards from any opening to the surface; and being at a depth of one hundred and twenty yards, and all in hard schistus rock, air-shafts would have been attended with an enormous expense; so that the tunnel being a long one, it was most desirable to sink as few as possible, and of course at considerable distances from each other. Thus a ventilating machine was required, which should act with sufficient force through a length of near half a mile; and on the side of the hill where it first became necessary to apply it, no larger stream of water to give it motion could be relied on, than such an one as I have mentioned after the description of the engine, and even that flowed at a distance from the shaft where the engine was to be fixed; which made a considerable length of connexion-rods necessary.

Within a very short time after the engine began to work, the superiority of its action over those formerly employed was abundantly evident. The whole extent of the tunnel, which had been uninterruptedly clouded with smoke for some months before, and which the air that was forced in never could drive out, now became speedily so clear, that the day-light and even objects at its mouth were distinctly

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seen from its furthest end. After blowing up the rock, the miners could instantly return to the place where they were employed, unimpeded by the smoke, of which no appearance would remain underground in a very few minutes, while it might be seen to be discharged in gusts, from the valve at the top of the shaft. The constant current into the pipe, at the same time effectually prevented the accumulation of air unfit for respiration. The influx of air, from the level into the mouth of the pipe, rushes with such force as instantly to extinguish the flame of a large candle; and any substance applied, so as to stop the orifice, is held tight by the outward pressure.

It is now more than two years since the machine was erected, and it has been uninterruptedly at work ever since, and without repair. The length of the tunnel has been nearly doubled, and the pipes of course in the same proportion, and no want of ventilation is yet perceptible.

Two similar engines have been since constructed for other parts of the same tunnel, and have in every respect answered the purpose for which they were designed.

The original one is worked by the small stream of water before mentioned, by means of a light overshot-wheel twelve feet in diameter, and about six inches in breast.—The two others are attached to the great overshot-wheel which pumps the water from the shafts which are sinking upon the line; and as their friction is comparatively nothing, this may be done in any case, with so little waste of power for this purpose as not to be an object of consideration, even if the power be derived from more expensive means.

The size of the exhauster may always be proportioned to the demand for air; and by a due consideration of this circumstance, this engine may be effectually adapted not only to mines and collieries, but also to manufactories, workhouses, hospitals, prisons, ships, and so on.

Thus, if it were required to ventilate a shaft of a mine, or a single level, which is most frequently the case, where three men are at work at one time, and we allow that those three men vitiate each twenty-seven and a half cubic inches of air per minute, (as determined by the experiments of Messrs. Allen and Pepys,) and allowing further that their candles vitiate as much as the men, there will be six times twenty-seven and a half cubic inches of air to be drawn out in a minute, equal to one hundred and sixty-five.

Now a cylinder five inches in diameter, working with a stroke at nine inches, will effect this by one stroke in a minute;

nute; though it would certainly be advisable to make it larger.

Not being practically acquainted with collieries, or mines that suffer from peculiar gases that are produced in them, I cannot state, from actual experiment, what effect this machine might have in relieving them; but it must appear, I conceive, evident to every person at all acquainted with the first principles of pneumatics, that it must do all that can be wished, as it is obvious that such a machine must in a given time pump out the whole volume of air contained in a given space, and thus change an impure atmosphere for a better one. And in constructing the machine it is only necessary to estimate the volume of gas produced in a certain time, or the capacity of the whole space to be ventilated. It is easy to judge how much more this must do for such cases as these, than such schemes as have lately been proposed of exciting jets of water, or slaking lime, both of which projects, likewise, must fail when applied; as one of them has, I believe, when applied to the case of hydrogen gas. But with such a machine as this, if the dreadful effects of explosions of this air are to be counteracted, it may be done by one of sufficient size to draw off this air as fast as it is generated; and by carrying the pipes into the elevated parts of the mine, where from its lightness it would collect. If, on the other hand, it is desired to free any subterraneous work from the carbonic acid gas, it may as certainly be done by suffering the pipe to terminate in the lower parts, where this air would be directed by its gravity.

In workhouses, hospitals, manufactories, &c. it is always easy to calculate the quantity of air contained in any room, or number of rooms, and easy to estimate how often it is desirable to change this in a certain number of hours, and to adjust the size and velocity of the engine accordingly. Where this change of foul air for pure is to take place in the night, means for working the machine may be provided by pumping up a quantity of water into a reservoir of sufficient height to admit of its flowing out during the night in a small stream, with sufficient fall, so as to give motion to the engine; or by winding up a weight of sufficient size, or by many other means which are easily devised.

If, for instance, a room in which fifty persons slept was eighty feet long, twenty wide, and ten high, it would contain 16,000 cubic feet of air, and if this was to be removed twice in eight hours, it would require a cylinder of thirty inches diameter, working with a four-feet stroke four times

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in a minute, to do it; or nearly that. Such a cylinder could be worked by the descent of ten gallons of water ten feet in a minute; or, for the whole time, by eighty hogsheads falling the same height.

But this is a vast deal more than could be required, as the fifty people would in eight hours vitiate only three thousand gallons of air, which could be removed by one hundred and fifty strokes of a cylinder, twelve inches diameter, with a four-feet stroke, which would not require an expenditure of more than one thousand five hundred gallons of water properly applied, or about twenty-eight hogsheads.

Holwell, near Tavistock, Feb. 7, 1810. JOHN TAYLOR.

XXIX. *Description of an improved Micrometer.* By E. WALKER, *Esq.*

To Mr. Tilloch.

SIR, AN exact method of taking small angles is a subject of the greatest importance in various branches of practical astronomy and philosophy. For this purpose the micrometer was invented, which has been constructed of various forms and on different principles; but even the best of them are very complex in their construction, and consequently too expensive to be of general utility.

To obviate these inconveniences Mr. Tiberius Cavallo invented the telescopic mother-of-pearl micrometer, "which consists of a small semitransparent slip of mother-of-pearl about the 20th part of an inch broad, and of the thickness of common writing-paper, divided into a number of equal parts by parallel lines." A full description of this micrometer and the uses to which it may be applied have been published in the 81st volume of the Philosophical Transactions for the year 1791, and in a separate pamphlet published in 1793.

This simple instrument possesses many valuable properties; and when the extremities of the object to be measured fall *exactly* upon two lines of the scale, it is very accurate: but as the fractional parts of a division on the scale cannot be known but by estimation, it falls far short of that accuracy which is obtained by other micrometers.

The micrometer that I have contrived is constructed at a small expense, and yet it is very accurate. It consists of a number of parallel lines drawn upon a piece of plane glass with

with the point of a small diamond, at the distance of $\frac{1}{100}$dth part of an inch from one another. This micrometer is placed in the focus of the eye-glass of a telescope or microscope, just where the image is formed when the instrument is adjusted to the object.

The telescope to which I have adapted this micrometer is a 30-inch achromatic with a compound eye-piece, consisting of a short tube containing two glasses*. Now it is a property of telescopes of this construction, that as the eye-piece is drawn out the magnifying power of the telescope is increased, and consequently the image of the object upon the micrometer is enlarged. By this means the fractional parts of a division on the scale may be determined in a very easy and expeditious manner, and to a very great degree of exactness.

The value of the divisions of the micrometer in the telescope which I use, magnifying 23 times, was ascertained by observations on the sun, in the following manner.

When the eye-tube of the telescope was drawn out $\frac{1}{10}$th of an inch, the solar image extended over 19 divisions of the micrometer. Then the diameter of the sun on the day of observation, being taken out of the Nautical Almanack, and divided by 19, gave 99″·6, which is the value of one division of the micrometer. After this value had been determined by a great number of observations, I drew out the eye-piece and re-adjusted the telescope until the image of the sun extended over 20 divisions on the micrometer; and on measuring the eye-piece I found that it had been drawn out $\frac{327}{1000}$dth parts of an inch further than before. Then $\frac{99''\cdot6}{327} =$ 304″·5 = one inch of the eye-piece. Consequently,

$\frac{1}{10}$th of an inch on the eye-piece	is =	30″·45	On the micrometer.
$\frac{1}{20}$th of ditto	is =	15″·22	
$\frac{1}{50}$th of ditto	is =	6″·09	
$\frac{1}{100}$dth of ditto	is =	3″· 0	

A micrometer, with lines drawn at the distance of $\frac{1}{100}$dth of an inch from one another, was adapted to a $3\frac{1}{2}$ feet achromatic telescope magnifying from 45 to 60 times, with the same glasses. When the eye-piece was drawn out $\frac{4}{10}$ths of an inch a remote land object extended over 23 divisions of the micrometer, and when it was drawn out $\frac{575}{1000}$dth parts of an inch further than before, the same object extended over 24 divisions of the micrometer. Each division of the scale, in this

* Called the Huygenian eye-piece.

telescope,

scope, subtends an angle of 30 seconds, which being divided by 575 the quotient is 52″, the value of one inch of the eye-tube. Consequently,

$\frac{1}{10}$th of an inch on the eye-tube	is =	5″·2	On the micrometer.
$\frac{1}{20}$th of ditto	is =	2″·6	
$\frac{1}{50}$th of ditto	is =	1″	
$\frac{1}{100}$dth of ditto	is =	0″.5	

The fractional parts of an inch on the eye-tube were obtained very exactly, by means of a pair of fine pointed compasses and a diagonal scale.—But it may be alleged, that the telescope cannot be adjusted a number of times to the same object by means of the eye-piece, so that it shall be drawn out precisely to the same point. Errors in these adjustments will frequently take place, but they may be corrected. The astronomer takes a mean of a number of his observations; and errors arising from adjusting the telescope to the object may be corrected in the same manner.

The following observations, which were taken of the sun's diameter with my 30-inch telescope, will show the limits of these errors.

The telescope was adjusted six different times to the sun, so that his image extended over 20 divisions on the micrometer, and the lengths of the eye-tube drawn out each time, in parts of an inch, were 0·42...0·42...0·44...0·42... 0·42...0·43; the mean of these different measures is 0·425, which differs only 0·015 from the extreme: and other sets of observations were taken with the same precision. But, to obtain correct observations, it is necessary that the telescope should be a good one, and mounted upon a proper stand.

The angular distance between two contiguous objects, at a remote distance, may be determined thus:

Direct the telescope to the objects, and if the angular points do not fall upon two lines of the micrometer scale, which will but seldom happen, draw out the eye-tube till they do. Then multiply the value of one of the divisions by the number of divisions, from which product subtract the number of seconds indicated by the eye-tube, and the remainder will be the angle sought.

Example 1.—Suppose the image of the full moon extended over 18 divisions of the micrometer, when the eye-tube was drawn out $\frac{15}{100}$dth parts of an inch: What was her apparent diameter?

Each division contains 99·6 seconds; but when this value was determined the eye-tube was drawn out $\frac{1}{10}$th part of an inch, which must always be deducted from the whole length of the tube drawn out. In this example $\frac{15}{100} - \frac{1}{10} = \frac{5}{100} = \frac{1}{20}$, which by the table is = 15·22 seconds.

Then $\overline{99''\cdot 6 \times 18} - 15''\cdot 22 = 1777''\cdot 58 = 29'\cdot 37''\cdot 58 =$ the apparent diameter of the moon at that time.

Example 2.—Suppose the distance between two stars was observed to extend over more than 15 divisions of the micrometer, but not 16, the eye-tube being drawn out $\frac{1}{10}$th of an inch; but on drawing out the eye-tube $\frac{37}{100}$dth parts of an inch, their distance extended over 16 divisions: What was the apparent angle subtended by those stars?

In this example $\frac{37}{100} - \frac{1}{10} = \frac{27}{100}$, which are $= 81''\cdot 9$. Then $\overline{99''\cdot 6 \times 16} - 81''\cdot 9 = 1511''\cdot 7 = 25'\ 11''\cdot 7$, which is the apparent angular distance between the two objects.

This micrometer is represented in fig. 5, Pl. III. The middle part of the scale is divided into 20 equal parts by parallel lines drawn at the distance of $\frac{1}{100}$dth part of an inch from each other, and the large divisions on the sides are each $= \frac{10}{100}$dth parts of an inch. These lines are drawn as fine as possible to appear distinct.

Angles may be taken in any direction by this micrometer, as it is easily turned round upon its axis; and as it is fixed against the eye-stop only by a ring of wire, in the same manner that glasses are fixed in ordinary instruments, it may be taken out and put in again with as little trouble as any other glass in the telescope.

I am, sir,

Your obedient servant,

Lynn, August 14, 1811. E. WALKER.

XXX. *Observations on some of the Strata in the Neighbourhood of London, and on the Fossil Remains contained in them.* *By* JAMES PARKINSON, *Esq. Member of the Geological Society**.

THE study of fossil organized remains has hitherto been directed too exclusively to the consideration of the specimens themselves; and hence has been considered rather as an appendix to botany and zoology, than as (what it really is) a very important branch of geological inquiry.

From a comparison of fossil remains with those living or extant beings to which they bear the closest analogy, great resem-

* From the Transactions of the Geological Society, vol. i.

resemblances and striking differences are at the same time perceivable. In some instances the generic characters materially differ, but in most they very closely correspond; whilst the specific characters are very rarely found to agree, except when the fossil appears to have existed at, comparatively, a late period. Of man, who constitutes a genus by himself, not a single decided remain has been found in a fossil state.

Chemical analysis has been called in to the aid of the naturalist, in order to account for the perfect state of preservation observable in remains organized with the most exquisite delicacy, and which there is every reason for supposing to have been readily decomposable in their recent state. From this investigation we learn the manner in which these memorials of the old world, so interesting and so frail, have been preserved. Some have been impregnated with calcareous matter, others with siliceous, and others with iron or copper pyrites.

But these facts, however important and interesting, cannot, when considered by themselves, add much to our knowledge respecting the formation and structure of the earth. To derive any information of consequence from them, on these subjects, it is necessary that their examination should be connected with that of the several strata in which they are found*.

Already have these examinations, thus carried on, taught us the following highly instructive facts. That exactly similar fossils are found in distant parts of the same stratum, not only where it traverses this island, but where it appears again on the opposite coast: that, in strata of considerable comparative depth, fossils are found, which are not dis-

* This mode of conducting our inquiries was long since recommended by Mr. W. Smith, who first noticed that *certain fossils are peculiar to, and are only found lodged in, particular strata*; and who first ascertained the constancy in *the order of superposition*, and *the continuity, of the strata of this island*. It will appear from the following quotation, that these observations have lately also occurred to Messrs. Cuvier and Brongniart, whilst examining into the nature of the strata of the neighbourhood of Paris. " Cette constance dans l'ordre de superposition des couches les plus minces, et sur une étendue de 12 myriamètres au moins, est, selon nous, un des faits les plus remarquables que nous ayons constatés dans la suite de nos recherches. Il doit en résulter pour les arts et pour la géologie des conséquences d'autant plus intéressantes qu'elles sont plus sûres.

" Le moyen que nous avons employé pour reconnoître au milieu d'un si grand nombre de lits calcaires, un lit déjà observé dans un canton très éloigné, est pris de la nature des fossiles renfermés dans chaque couche: ces fossiles sont toujours généralement les mêmes dans les couches correspondantes, et présentent des différences d'espèces assez notables d'un système des couches à un autre système. C'est un signe de reconnoissance qui jusqu'à présent ne nous a pas trompé."—*Annales du Muséum d'Hist. Nat.* tome xi. p. 307.

covered in any of the superincumbent beds: that some fossils, which abound in the lower, are found in diminishing numbers through several of the superincumbent, and are entirely wanting in the uppermost strata: that some fossils, occurring in considerable numbers in one stratum, become very rare in the adjacent portion of the next superincumbent stratum, and afterwards are lost: that fossils of one particular genus, which exist abundantly in the lower strata, and occur in several of the superincumbent ones, are not found in the three highest strata; whilst one species of that genus, but which has not been found in a fossil state, exists in our present seas: and lastly, that most of the remains which are abundant in the superior strata, are not at all found in the lower. These general facts lead us to hope, that geology may derive considerable assistance from an examination of fossils, made in connexion with that of the strata to which they belong.

The following is an attempt to investigate on this plan some of the upper strata in the vicinity of the metropolis with their contained fossils; and, although by no means complete, it will, it is hoped, induce others, who possess superior abilities and opportunities, not only to re-examine more correctly these strata, but to extend their researches to the subjacent strata.

The whole of this island displays evident marks of its stratification having, since its completion, suffered considerable disturbance from some prodigious and mysterious power. By this power all the known strata, to the greatest depths that have been explored, have been more or less broken and displaced; and in some parts have been so lifted, that some of the lowest of these have been raised to the surface; whilst portions of others, to a very considerable depth and extent, have been entirely carried away*. From these circumstances great difficulties and confusion frequently arise in examining the superior strata: the counties however immediately surrounding the metropolis, as well as that on which it stands, having suffered least disturbance, are those in which an investigation of these strata may be carried on with the smallest chance of mistake.

Real alluvial fossils, washed out of lifted or original superior strata by strong currents, and which in other parts

* See several essays on this subject in the Philosophical Magazine, by Mr. Farey, and the Report on Derbyshire, vol. i. p. 105.

Also A Letter on the Alterations which have taken place in the Structure of Rocks, on the Surface of the Basaltic Country in the Counties of Derry and Antrim, by William Richardson, D.D. Phil. Trans. 1808.

are

are very abundant, are rarely seen in the counties adjacent to the metropolis. This remark is rendered necessary, since those widely extended beds of sand and gravel, with sandy clay, sometimes intermixed and sometimes interposed, and which have been generally hitherto considered as alluvial beds, are here assumed to be the last or newest strata of this island, slowly deposited by a pre-existent ocean: with the strata, therefore, of this formation, these remarks commence.

Beds of Sand and Gravel.—The sands of this formation vary in colour from white, which is most rare, through different shades of yellow up to orange red: the colour proceeding partly from a ferruginous stain on the surface of the particles of sand, and partly from the intermixture of yellow oxide of iron. Particles of those sands, which are disposed in distinct seams or beds, when examined by the microscope, are found to be transparent, most of them angular, but some a little rounded, with all their surfaces smooth, having no appearance of fracture, and resembling, in every respect, an uniform crystalline deposition. Those sands on the contrary, which blended with broken and unbroken pebbles form gravel, appear, when thus examined, to be mostly opake, to be variously coloured, and to be marked with conchoidal depressions and eminences, the result of fracture.

The pebbles of this formation appear to be of four kinds; 1st. Various pieces of jasper, gritstone, white semi transparent quartz, and other rocks. These have acquired, in general, smooth surfaces and roundish forms, evidently from attrition, and exhibit no traces of organization, except when, as is very rarely the case, the substance of the pebble is jasperized wood. The white quartz pebbles, like quartz crystals, on being rubbed together, emit a strong white lambent light, with a red fiery streak on the line of collision, and an odour which much resembles that of the electric aura.

2d. Oval or roundish, and rather flat siliceous pebbles, generally surrounded by a crust or coat differing in colour and degree of transparency from the internal substance, which also varies in different specimens, in these respects, as well as in the disposition of the parts of which the substance is composed. In some this is spotted, or clouded, in very beautiful forms; in others it is marked by concentric striæ, as if the result of the successive application of distinct laminæ: the prevailing colours in most of these pebbles being different shades of yellow. In several the traces of marine remains are observable: these are, in some the casts of *anomiæ*, and the impressions of the spines and plates of *echini;* and in others, which generally possess a degree of transparency, the remains of *alcyonia*. The impressions, though frequently on the surface of the pebble, seldom, if ever, appear to be in the least rubbed down; thus seeming to prove decidedly, that these pebbles have not been rounded by rolling, but that they owe their figures to the circumstances under which they were originally formed: it is apprehended therefore, that these pebbles have each been produced by a distinct chemical formation, which, it may be safely concluded from the remains of marine animals so frequently found in them, took place at the bottom of the sea, while these animals were yet living.

The formation of these fossils at the bottom of a former sea, and perhaps on the identical spots in which they are now frequently found, is more plainly evinced by pebbles agreeing in some peculiar characters being found together in particular spots. Thus those in the county of Essex, ten miles northward of London, contain a much greater proportion of argil and iron than those met with in many other places; hence their colours are darker, and the delineations which their sections display are very strong and decided, sometimes closely agreeing with those seen in the Egyptian pebbles*. Passing on into Hertfordshire, pebbles of a very different character are found: their crust is nearly black, and their section displays delicate tints of blue, red, and yellow, disposed on a dead-white ground in very beautiful forms. In another part of the same county occurs the pebble of the pudding-stone, which also presents peculiar characters of colour, &c.

3d. Large tuberous, or rather ramose, irregularly formed flints, somewhat resembling in figure the flints which are found in chalk, materially differing however from them, not only in the colour of their external coat, which is of various shades of brown, but also in that of their substance, which is seldom black, but exhibits shades of yellow or brown, in which red likewise is sometimes perceptible. The traces of organic structure, particularly of the *alcyonium*, occasionally seen in these stones, determine them also to have been formed at the bottom of the sea.

4th. Pebbles, owing their form to an investment and

* The gravel pebbles of Epping Forest are of this description; and on most of the grounds leading down from the forest to the hamlet of Sewardstone and to the town of Waltham, white, opake, and partly decomposed pebbles are frequently seen, in which the argil and iron have been removed, and the silex only has remained.

impregnation with silex of various marine animals of unknown genera, but bearing a close affinity to the *alcyonia.* These stones display, in general, not only the external form but the internal structure also of these animals. The congregation of many pebbles of this genus, and indeed of the same species, in particular tracts, warrants the conclusion, that these animal substances were thus changed, whilst inhabiting that bottom of a former ocean, which now forms the stratum the contents of which are here sketched. Pebbles of this description are most frequently found in the gravel pits of Hackney, Islington, &c.

Among the traces of organization discoverable in this stratum are casts of *echini*, which are frequently found among the gravel, and which have generally been supposed to have been washed out of the chalk. But these casts have their origin plainly stamped on them. Their substance is covered with iron; they are almost always of a rude and distorted form, and I apprehend that they are never found with any part of the crust of the animal converted into spar, adherent to them, as is commonly the case with the casts of echini found in chalk.

A sufficient proof, that these several strata of gravel, sand, &c. have been deposited by a former ocean, is to be found in a circumstance which does not appear to have been hitherto sufficiently adverted to. This circumstance is the existence of fossil shells belonging to, and accompanying, the superior part of these strata in particular spots; their absence in other parts being, perhaps, attributable to the removal of the upper beds.

These fossil shells are still found disposed over a very considerable extent. Their nearest situation to the metropolis is at Walton Nase, a point of land about sixteen miles S. E. of Colchester. Here a cliff rises more than fifty feet above high-water mark and the adjacent marshes. It is formed of about two feet of vegetable mould, twenty or thirty feet of shells, mixed with sand and gravel, and from ten to fifteen feet of blue clay. The bed of shells is here exposed for about three hundred paces in length, and about a hundred feet in breadth.

Immediately beyond the Nase the shore suddenly recedes and forms a kind of estuary, terminated towards the east by the projecting cliff of Harwich, which is capped in a similar manner with beds of these shells. The height of this cliff is from forty to fifty feet, about twenty-two feet of the lower part of which is the upper part of the blue clay stratum: " above which," as Mr. Dale observes, " to within two feet of the surface, are divers strata of sand and gravel mixed with fragments of shells, and small pebbles; and it is in some of these last-mentioned strata that the fossil shells are imbedded. These fossils lie promiscuously together, bivalve and turbinate, neither do the strata in which they lie observe any order, being sometimes higher and sometimes lower in the cliff; with strata of sand, gravel, and fragments of shells between. Nor do the shells always lie separate or distinct in the strata, but are sometimes found in lumps or masses, something friable, cemented together with sand and fragments, of a ferruginous or rusty colour, of which all these strata are *."

The coast of Essex is here separated from that of Suffolk by the river Stour, by which the continuity of this stratum is necessarily interrupted. It however occurs again on the opposite side of the river, and through Suffolk and great part of Norfolk the same bed of shells is found on digging; thus appearing to extend over a tract of at least forty miles in length.

These shells are in general found in the same confused mixture as is described by Mr. Dale; but they are also sometimes so disposed, that patches of particular genera and species appear to be now occupying the very spots where they had lived. This seems particularly the case with the small *pectens*, the *mactræ*, and the *left-turned whelk*.

From the excellent state of preservation in which many of these shells have been found, it has been thought that they could hardly be regarded as fossil. Many acknowledged fossil shells, however, have undergone much less changes than those of this stratum; the original coloured markings are entirely discharged, and the external surfaces are deeply penetrated with a strong ferruginous stain; the inner surfaces also are considerably changed, their resplendence being superseded, to a considerable depth, by a dead whiteness, the consequence of the decomposition of this part of the shell.

Like the fossils of most other strata, this assemblage of shells manifests a peculiar distinctive character. A few shells only, which may be placed among those which are supposed to be lost, or among those which are the inhabitants of distant seas, are here discoverable; the greater number appearing not to differ specifically, as far as their altered state will allow of determining, from the recent shells of the neighbouring sea.

* Appendix by Samuel Dale to the History and Antiquities of Harwich and Dovercourt by Silas Taylor, 1732.

Among

Among those of which no recent analogue is known, appears to be the *terebratula*, figured in Dale's History and Antiquities of Harwich, &c. tab. xi. fig. 9, p. 294, and described, Phil. Trans. No. 291, p. 1578. Mr. Dale describes this shell as *Concha longa fossilis fasciata,* and remarks that he has not observed "either in Aldrovandus, Rondeletius, Belonius, Gesner, Johnson, Lister, or Bonanus, any shell that resembles this our fossil, unless it is one of those figured by Lachmund, p. 43, No. 6 and 7, the inward part resembling our fossil." The shells figured by Lachmund are undoubtedly *terebratulæ,* but they manifest no particular agreement with this fossil.

This shell appears to be figured by Lister, *Histor. Conchyl. tab.* 211, *fig.* 45, and is assumed by Gmelin as *Anomia spondylodes.* The other shells, fig. 46, of the same plate, referred to by Gmelin as *Anomia psittacea,* appear to be mutilated specimens of the same shell. This opinion is corroborated by the tint given by the accurate artists to the whole of the shells contained in this plate, agreeing with the dark colour of the Essex fossil; and by the circumstance of their being generally found in the mutilated state in which they are here figured by Lister. Besides, neither of Lister's specimens at all agrees with the pellucid shell, with a triangular foramen, of *Anomia psittacea,* but they all agree with the oval antiquated shell, with an obtuse canaliculated beak, of *Anomia spondylodes.*

In consequence of this agreement, it seems proper to consider this fossil shell as forming the species *Anomia spondylodes.* But as the channelled beak is not natural to it, but is the consequence of injury; and as this part, in its natural state, is pierced with a large round foramen, a correspondent change should be made in the description, and it may be placed under the more appropriate genus of terebratula, as *Terebratula spondylodes,* with an oval antiquated shell, the beak pierced by a large round foramen.

This shell is, in general, about an inch and a half long, thick, nearly oval, roughly striated transversely, and has its large foramen defined by a distinct border. It appears to differ from every known recent or fossil *terebratula.*

Another of the probably lost shells of this stratum is the fossil *oyster,* figured Organic Remains, &c. vol. iii. pl. xiv. fig. 3, and which is there conjectured to be the same oyster as that which is described by Lamarck as *Ostrea deformis.*

The *volute,* Organic Remains, vol. iii. pl. v. fig. 13, is another shell belonging to this stratum, of which it is believed that no recent analogue has been yet found. This

ovate

ovate and rather fusiform shell appears to have been smooth; and at its full size about four inches in length: the columella has four folds, and the shell is formed by about six spiral turns, the last of which makes two thirds of the shell, dilating at about its centre, and contracting nearly equally upwards and downwards. The specimens yet seen give no opportunity of judging of the lip, or of the termination of the spire.

The Essex *reversed whelk*, as it has been termed, *Murex contrarius*, Linn. *Hist. Conch.* of Lister, tab. 950, fig. 44. b. c. which is here very abundant, does not appear to be known in any other stratum of the island. The fossil shell, with the whirls in the ordinary direction, is sometimes found in this stratum*.

It has been said that the recent analogues of both these shells are found in the adjoining sea. A recent shell is indeed found, which very nearly agrees with the ordinarily turned shell in its general characters: but there appears no authority for supposing that the analogue of the left-turned variety has been discovered there.

Among those recent shells, the resemblance of which to the fossil ones of this stratum is such as appears to render a comparison by an experienced conchologist necessary, may be enumerated:

Patella ungarica, Patella militaris, Patella sinensis, (*Calypiræa*, Lam.) *Patella fissura*, (*Emarginula*, Lam.) one or two species of *Patellæ*, with a perforation in the apex, (*Fissurella*, Lam.) *Nerita glaucina, Nerita canrena*, (*Natica*, Lam.) *Turbo terebra*, (*Turritella*, Lam.) *Murex corneus, Murex erinaceus, Strombus pes pelicani, Cypræa pediculus*, with no sulcus along the back, *Pholas crispatus*, in fragments, *Solen ensis*, and *Solen siliqua*, in fragments, *Cardium edule, Cardium aculeatum?* bearing the size and form of this shell, but having from thirty-four to thirty-six ribs, with no depressed line down their middle, nor vestiges of spines; *Mactra solida, Venus exoleta, Venus scotica? Venericardia senilis*, Lam. *Arca glycemeris, Arca nucleus*.

Besides these remains of marine animals, the fossil hollow tubercles, having lost the spines, of the *thornback* are here found; also fragments of the *fossil palate*, (*Scopula littoralis* of Lhwydd) and fossil remains of *sponge* and *alcyonia*, particularly a very fair specimen of the *reticulated alcyonium*. Org. Rem. vol. ii. pl. ix. fig. 9.

* It is erroneously stated, Organic Remains, vol iii. p. 66, that this shell has not been yet mentioned, as found in this stratum; since it is so particularised by Dale.

In

In this bed, among the gravel and the shells, are frequently found fragments of *fossil bone*, which possess some striking peculiarities. They are seldom more than half an inch in thickness, two inches in width, and twelve in length; always having this flat form, and generally marked with small dents or depressions. Their colour, which is brown, light or dark, and sometimes inclining to a greenish tint, is evidently derived from an impregnation with iron. From this impregnation they have also received a great increase of weight and solidity; from having been rolled they have acquired a considerable polish; and on being struck by any hard body they give a shrill ringing sound. These fragments, washed out of the stratum in which they had been imbedded, are found on the beach at Walton, but occur in much greater quantity at Harwich.

Of the flat rounded pieces described above, no conjecture can be formed as to the particular bone or particular animal to which they belonged. But within these few years an Essex gentleman found, on the beach at Harwich, a tooth which was supposed to have belonged to the *mammoth*. This fossil was kindly obtained, at my request, for the purpose of being exhibited to the members of the Geological Society, by my late friend Dr. Menish; and certainly it appeared to be part of a tooth of that animal. It had been broken and rounded by rolling, but its characters were still capable of being ascertained. It possessed, in the softer parts, the colour and appearance of the Essex mineralised bones so distinctly, as to leave not a doubt of its having been imbedded in this stratum; whilst in the enamel it manifested decided characters of the tooth of some species of the mammoth, or *mastodon* of Cuvier.

The actual limit of this stratum has not been ascertained; it is however known to extend through Essex, Middlesex, part of Kent, and Surry, and through Hertfordshire, Buckinghamshire, and indeed much further both to the northward and westward. In many parts its continuity has been interrupted, apparently by partial abruptions of it, together even with a portion of the stratum on which it rests. The shells of this stratum have hitherto been discovered only in the parts already noticed.

Blue Clay Stratum.—This, the next subjacent bed, is formed of a ferruginous clay exceeding two hundred feet in thickness. Its colour for a few feet in the upper part is a yellowish-brown, but through the whole of its remaining depth is of a dark-blueish gray, verging on black. It is not only characterized by these circumstances, but by the numerous

numerous *septaria* which are dispersed through it, and by the peculiar fossils which it contains.

The difference of colour observed between its superior and inferior part, and which has generally been supposed to be owing to a difference in the degree of oxidation of the iron present in it, appears to be the result of a difference in the quantity of it, occasioned by the washing away of this metal in the upper part by the water which percolates through it, and which runs off laterally by the numerous drains made near the surface. The dark-red colour of tiles made from the blue clay, the reddish-yellow colour of the *place* bricks made of the yellowish-brown clay, and the bright-yellow hue of the *washed malms*, those bricks which are formed of the yellow clay which has been exposed to repeated washings, are thus accounted for.

The septaria lie horizontally, and are disposed at unequal distances from each other in seemingly regular layers; and, as has been just observed of the stratum itself, they become of a paler colour, and it may be added suffer decomposition, when placed so high in the stratum as to be exposed to the action of percolating water. They frequently include portions of wood pierced by the *Teredines*, *Nautili*, and other shells; and it is a fact that may be worthy of being attended to, whilst inquiring into their formation, that the septa of calcareous spar frequently intersect the substances enclosed in the septaria.

This stratum is to be found not only wherever the preceding deposition extends, but in other parts also where that has been removed. The cliffs of this clay, at Shepey, extend about six miles in length; the more elevated parts, which are about ninety feet in height, being about four miles in length, and declining gradually as they terminate towards the east and west.

The fossils of this stratum have been already carefully particularised. A catalogue of those found at Shepey was added by Mr. Jacobs to his *Plantæ Favershamienses*; and an account of several of the fossil fruits found at Shepey was published by Dr. Parsons in the fiftieth volume of the Philosophical Transactions. The fossils of Hampshire have been scientifically described by Dr. Solander, in the *Fossilia Hantoniensia* of Mr. Brander, where the fossils themselves are very exactly figured.

It was not supposed, even after the publication of these accounts, that the fossils of Shepey and those of Hampshire were of the same stratum. Among the Hampshire fossils no mention is made of *crabs*, *lobsters*, *tortoises*, *nautili*, nor

nor of the heads or bodies of *fishes* so abundant at Shepey; whilst the *Murex pyrus*, *Murex longævus*, *Strombus amplus*, &c. of the Hampshire cliff had never, perhaps, been enumerated among the Shepey fossils.

The identity of the stratum at Shepey and in Hampshire has, within a few years, been decided by digging into this same stratum at Kew, where several of the fossils, which had hitherto been supposed peculiar to Shepey, were found in the same pit with those which had been considered as peculiar to Hampshire.

In the present year, on cutting through a mound of this stratum which forms Highgate-hill, this identity has been still further manifested by the discovery of great numbers of those fossils mingled together which had been generally distinguished into Hampshire and Shepey fossils; as *crabs*, *nautili*, &c. like those of Shepey, together with several shells which had been generally regarded as peculiar to Hampshire, and in particular that uncommon alated shell, *Strombus amplus*, Solander. (*Rostellaria macroptera*, Lamarck.)

In examining this stratum, the curious fact that certain organic remains are peculiar to particular depositions, is first observed. Very few indeed of the fossil shells of the gravel strata are to be found in the bed of blue clay. In the gravel strata, by far the greater number of the shells bear a close agreement with those which now exist in not very distant seas; but in this clay stratum, "very few of the shells are known to be natives of our own, or indeed any of the European shores, but the far greater part of them, upon a comparison with the recent, are wholly unknown to us*."

But although this clay stratum contains fossils of a much older date than those of the gravel stratum, it possesses other marks which agree with its position in showing that it is of comparatively modern formation. It includes none of the remains of any of the lost fossils, such as the *Cornu ammonis*, *Encrinites*, &c. Mr. Jacobs indeed speaks of one imperfect specimen of *Belemnites* and of *Astroitæ* having been found, but at the same time as being very uncommon. Mr. Brander however does not appear to have met with any of these older fossils; nor have any of them been discovered either at Kew or at Highgate. Hence it seems reasonable to conclude, that the single imperfect belemnite and the few astroitæ were not inhabitants of the sea at the period when this stratum was deposited, but were washed

* *Fossilia Hantoniensia*, p. 5.

out of some of the more ancient strata, and lodged by accident in the bed where they were found*.

The quantity of fruit or ligneous seed-vessels and berries, which has been found in this stratum at Shepey, is prodigious. Mr. Francis Crow, of Feversham, has procured from this fertile spot a very large collection; and by carefully comparing each individual specimen by their internal as well as their external appearance, he has been enabled to select seven hundred specimens, none of which are duplicates, and very few agree with any known seed-vessels. These vegetable remains have also been found on the opposite Essex shore, but in very small numbers. They have also been met with in that part of the stratum which has been examined at Kew. At Highgate and at Shepey a resinous matter, highly inflammable, of a darkish-brown colour, and yielding, on friction, a peculiar odour, has also been found. This substance has been conjectured to exist in an unaltered state, and this indeed seems to be the fact from its resinous fracture; but it must be observed, on the other hand, that pieces of it occur which are penetrated by iron pyrites.

This stratum is also rendered exceedingly interesting by its surface appearing to have been the residence of land animals, not a single vestige of which seems to have been found in any of the numerous subjacent strata of the British series. Mr. Jacobs relates that the remains of an *elephant* were found at Shepey. The remains of the *elephant*, *stag*, and *hippopotamus* have also been dug up at Kew. At Walton in Essex, not only the remains of the *elephant*, *stag*, and *hippopotamus* have been discovered, but also remains of the *rhinoceros*, and of the *Irish fossil elk*. Org. Rem. vol. iii. p. 366.

It has been generally supposed that these remains were contained within the stratum of blue clay; but the circumstances under which they are found seem rather to warrant the conclusion, that they were deposited on the surface of those low spots where abruptions of the superior part of this stratum had taken place. Thus the remains of the elephant mentioned by Mr. Jacobs were not in the cliff, but in a low situation at a distance from it: so

* It appears to be necessary to guard against two sources of error whilst appropriating fossils to their respective strata: one is the circumstance here alluded to, where the fossils of a preexistent stratum have been washed out by the waters while depositing a more recent stratum: the other is where, at the line of junction of two strata, the animals of the one are found within the borders of the other stratum; a circumstance by no means difficult to be conceived or explained.

also

also the remains of land animals in Essex occur a little below the surface, in a line with the marshes, which are a very few feet above high-water mark. By a communication of the late Mr. William Trimmer of Kew, it appeared that he found, under the sandy gravel, a bed of earth, highly calcareous, from one foot to nine feet in thickness; beneath this a bed of gravel a few feet thick, containing water, and then the main stratum of blue clay. At the bottom of the sandy gravel, he observed that the bones of the *hippopotamus*, *deer*, and *elephant* were met with; but not in those parts of the field to which the calcareous bed did not extend. Here also a considerable number of small and apparently fresh-water shells, and at the bottom snail-shells, were found. Does it not seem that the first appearance, or creation, of land-animals was on the dry land of this stratum, and that they were overwhelmed in these spots by that sea which deposited the present superincumbent strata of gravel?

STRATA INTERPOSED BETWEEN THE CLAY AND THE CHALK.

It is almost impossible to speak with precision of the subjacent strata, which are situated between the clay and the chalk, since very considerable variations occur as to their thickness, and indeed as to the form in which their constituent parts are disposed; and since there exist but few sections, at least in the neighbourhood of the metropolis, which present a view of the strata composing this formation. They are included in the following account by Mr. Farey: "A sand stratum, of very variable thickness, next succeeds, and lays immediately upon the chalk, in most instances, as between Greenwich and Woolwich, on the banks of the Thames; which has often been called the *Blackheath sand*: it frequently has a bed of cherty sandstone in it, called the gray-weathers *."

On the upper part of a mound at New Charlton some traces of the lowest part of the blue clay appear, covered by not more than a foot of vegetable earth. This layer of clay does not seem to exceed two feet in thickness, which, indeed, it possesses only on the top of some of those mounds, which occur so frequently as to render the surface in this district very irregular. In this clay, oysters of different forms are found; some approaching to the recent species, and others longer and somewhat vaulted: but they are in general so tender as to render it very difficult to obtain a

* Report on Derbyshire, &c. vol. i. p. 111.

tolerable

tolerable specimen. With these also occur numerous *Cerithia, Turritellæ* and *Cythereæ,* Lam. all of which are in a similar state with the oysters, and appear to be shells strictly belonging to the subjacent stratum, but which, having lain uppermost, became involved in the first or lowest deposition of the blue clay.

Immediately beneath the clay there is found a line of about three or four inches of the preceding shells imbedded in a mass of calcareous matter, the result of their disintegration. Beneath this are numerous alternating layers of shells, marl, and pebbles, for about twelve or fifteen feet. The shells are those which have been already mentioned; but are very rarely to be met with whole, and when entire are so brittle as to be extricated with much difficulty. In some of these layers scarcely any thing but the mere fragments of shells is to be found, and in others a calcareous powder only is left.

The pebbles are almost all of a roundish oval form, many of them being striped, but differing from those of the superior gravel stratum, in being seldom broken, in there being few large ramose masses, and in their not bearing any marks or traces of organization. Many of these pebbles are passing into a state of decomposition, whence they have in some degree the appearance of having been subjected to the action of fire: small fragments of shells are every where dispersed amongst them.

Beneath the pebbles is a stratum of light fawn-coloured sand of about ten feet in depth, and immediately under this is the stratum of white sand, which is about five-and-thirty feet deep, and is here seen resting immediately on the chalk.

At Plumstead, about a mile distant in a south-eastern direction, there is a pit, in which the shells, about two years ago, were to be obtained in a much better state of preservation than at New Charlton; but this seam of shells, as the pit has been dug further in, has by degrees become so narrow as to be now nearly lost. In this pit, not only the shells already mentioned were found, but many tolerably perfect specimens of *Calyptræa trochiformis,* Lam. *Trochus apertus,* Brander. *Arcæ glycemeres, Arcæ Naticæ,* and many minutes shells in good preservation. All these shells appear to have entirely lost their animal matter, and not having become imbued with any connecting impregnation, they are extremely brittle. On examination with a lens, it also appears that in most of the specimens nothing of their original surface remains, it having been every where indented with impressions of the surrounding minute sand, made

made whilst the shells were in a softened state. This circumstance is particularly evinced in the *Cyclades*, in which a particular character in the hinge was thus concealed: in a mass of these shells from the Isle of Wight, it appears that the lateral teeth are crenulated, somewhat similar to those of the *Mactra solida* in the gravel stratum; but in the *Cyclades* of Plumstead this was not discoverable, from the injuries which their surface had sustained from the sand.

The fossils of this stratum evidently agree with those found by Lamarck and M. De France above the chalk at Grignon, Courtagnon, &c. and they have been just shown, incidentally, to exist in the Isle of Wight. In an eastern and southern direction from London this stratum with its fossils is frequently discovered.

On the heath near Crayford, about four miles eastward of Charlton, long vaulted oysters are found similar to those already mentioned. About two miles further, in the parish of Stone, is *Cockle-shell-bank*, so called, as Mr. Thorpe, the author of *Custumale Roffense*, says, p. 254 of that work, "from the great number of small shells there observable." These are the *Cyclades* already spoken of, and which Mr. John Latham, author of *The general Synopsis of Birds*, thought bore some resemblance to *Tellina cornea* Linn. *Histor. Conchyl.* of Lister, tab. 159, fig. 14. Mr. Latham here also met with a species of *Cerithium*, and another of *Turritella*. Fragments of these shells are also frequently turned up with the plough in that neighbourhood. They have likewise been found at Dartford, at Bexley, and at Bromley, to the southward.

Mr. Thorpe also relates that in the parish of Stone there was a large mass of stone, of some hundreds weight, full of shells, which was brought from a field, and used as a bridge or stepway over a drain in the farm-yard. (*Custumale Roffense*, p. 255.)

In several spots in the neighbourhood of Bromley, stone is found near the surface, formed of oyster-shells still adhering to the pebbles to which they were attached, and which are similar to those which have been just described as occurring at Plumstead and at Charlton; the whole being formed by a calcareous cement into a coarse shelly limestone containing numerous pebbles. The only quarry of this stone which has been yet worked is in the grounds of Claude Scott, Esq. The opening hitherto made is but small; it is however sufficient to show that the stratum here worked has suffered some degree of displacement, as it dips with an angle of about forty-five degrees.

At Feversham, over the chalk, Mr. Francis Crow has discovered a bed of dark-brown sand, slightly agglutinated by a siliceous cement, and intermixed with a small portion of clay. In this stratum, which has been hitherto but little explored, he has found in a siliceous state specimens of *Strombus pes pelicani* and a species of *Cucullæa*, nearly resembling those which are met with in the Black-down whetstone pits.

Patches of plastic clay are frequently found over the chalk: some of these are yellow, and employed for the common sorts of pottery; but others are white, or grayish-white, and are used for finer purposes. The coarser clay is very frequently met with, nor are the finer kinds of very rare occurrence. In the Isle of Wight two species of plastic white clay are worked for the purpose of making tobacco-pipes. A similar clay, which is used for making gallipots, is dug from the banks of the Medway. A fine, light ash-coloured nearly white clay, which is employed in pottery-works, is also dug at Cheam near Epsom in Surry.

The UPPER OR FLINTY CHALK, which is the next older stratum, is extremely thick, forming stupendous cliffs upwards of six hundred and fifty feet high, on the south-eastern coast of the island. It extends nearly through almost all that part of the island which lies south of a line supposed to be drawn from Dorchester in the county of Dorset to Flamborough-head in Yorkshire.

In this stratum there is a great quantity of flint, chiefly in irregularly formed nodules, disposed in layers, which preserve a parallelism with each other and with continuous seams of flint, sometimes not exceeding half an inch in thickness. The chalk contains a fine sand, which may be separated by washing*.

The fossils of this stratum are for the most part peculiar to it; very few of them being found in any other. They also appear to agree very closely with those species found in the chalk of France, by Messrs. De France, Cuvier and Brongniart. The number of fossils noticed by these gentlemen amounts to fifty; but they have as yet only particularised a part of them. These are here compared with what appeared to be the correspondent fossils in the English part of this stratum; and some others are also pointed out, which these gentlemen have not yet mentioned as being found in the neighbourhood of Paris.

* The chalk in the neighbourhood of Paris contains, according to M. Bouillon La Grange, magnesia 0.11, and silex 0.19.

In

In the French stratum there occur,

Two *Lituolites*. No species of this genus is noticed as having been seen in our English chalk. But research has not been made with the necessary precision.

Three *Vermiculites*. The fossil figured Org. Rem. vol. iii. pl. vii. fig. 11, was considered as a vermiculite, until by removal of the chalk and opening different specimens it was found to be a chambered and an adherent shell. Should these gentlemen not have perceived these circumstances in the specimens they met with, they would certainly regard this fossil as a vermiculite. It must also be observed, that from the different forms in which the spiral part is disposed, its division into two or three species might be authorised.

Belemnites. These, according to M. De France, are different from those which accompany the *ammonites* of the compact limestone. The *belemnites* of our chalk are smaller than those of the limestone, besides which they are different in form, being narrower and more elongated. But M. De France may also have confounded with them the spines of the *echinus*, which so closely resemble the *belemnite*: if that gentleman should not have met with perfect specimens, he might not be able to remark the difference between these two fossils. The characters which he has noticed are however sufficient to lead to the belief of a correspondence between the French and English fossils.

Fragments of a thick shell of a fibrous structure.—The doubts expressed respecting the nature of this shell, and the observations made with regard to it, offer another strong point of agreement between the shells of the two strata. The shell here alluded to is most probably that represented Org. Rem. vol. iii. pl. v. fig. 3; the structure of which agrees exactly with that mentioned as found in the French stratum of chalk. That shell is however described as being of a tubular form; it is therefore right to observe, that fossil *pinnæ* do sometimes possess this peculiar structure.

A *Muscle*. No instance appears in which any shell of this genus has been found in our chalk.

Two *Oysters*. The Kentish chalk-pits yield at least three species of this genus. One of them bearing very much the form and appearance of *Ostrea edulis*, but being only about a fourth of its size; one smaller, the serrated edge of which places it in the family of *Cristæ galli;* and the third still smaller, not half an inch in length, crenulated on each side of the hinge.

A species of *Pecten*. There are two or three small species

 of

of *pecten* in the English chalk ; besides a shell, with long slender spines, which may be safely classed with the pecten.

A *Crania* (*Anomia craniolaris* Linn. *Crania personata* Lam.) This fossil is not known in the English chalk ; nor indeed could it be easily ascertained, unless the inferior valve happened to be well displayed.

Three *Terebratulæ*. *T. sulcata* and a terebratula agreeing with *Anomia terebratula* Linn. are frequently found in our chalk ; and sometimes another species, hardly half an inch in length, with remarkably acute and well defined ribs.

A *Spirorbis*. Traces of these shells are frequently found on the surface of the *echinitæ*.

Ananchitæ (*Echinus ovatus*). The crustaceous covering of which, it is remarked by MM. Cuvier and Brongniart, remains calcareous, and has assumed a sparry texture, whilst the middle alone is changed into silex. No actual change has however taken place, as far as respects the flinty part of the fossil, the flint having merely filled up the hollow of the sparry crustaceous covering. This fossil is frequently found in the English chalk.

Porpitæ. These also occur in the English chalk.

Five or six different fossil bodies called by the French oryctologists *Polypiers*, one appearing to belong to the genus *Caryophyllæa*. Several of these bodies, from the English chalk, have been figured in the Org. Rem. vol. ii. pl. xiii. fig. 70 to 79.

Another is supposed to belong to the genus *Millepora*. This is generally brown, and is in the state of oxidized iron, as resulting from the decomposition of pyrites. These fossils exist in the Wiltshire soft chalk.

Lastly, *Shark's teeth*. These also occur frequently in the English stratum.

Messrs. Cuvier and Brongniart state, that there are many more fossils in the chalk stratum of France than those which have been just referred to. This is also the case with the fossils of the English chalk ; since the following may be enumerated as occurring in this stratum. *Rugous palates*, and, though rarely, the *scales* and *vertebræ of fishes*. Three or four species of *stellæ marinæ*. A *long saccular bivalve*, with an uncommonly thin shell, of which so little has been hitherto saved, as not to give a chance of gaining a knowledge of its general form, or the structure of its hinge. *A bivalve*, which approaches to a circular form, but is so thin as to afford but little hope of discovering its genus.

genus. *A bivalve, nearly circular, the margin turning upwards so as to give it a patella or disk form, with numerous long processes passing from the margin and external surface, and fixing it to other bodies. A small pecten with sharp angulated ribs*, not exceeding a quarter of an inch in length. *A bivalve*, not an eighth of an inch in length, finely striated longitudinally, bearing a bright polish, and seemingly possessing its original light brown colour. *Plates of the tortoise echinite*, and several remains apparently of other species of this genus.

When to these are added the remains of various *echini*, such as *conulites*, *cassidites*, and *spatangites*, and the different *spines* of *echini* which are found in this stratum; and when it is also considered that the present account is drawn up almost entirely from the productions of chalk cliffs, of not more than two miles in length, it will not be difficult to conceive, that the number of these fossils is not less in the English than in the French chalk.

The state in which these fossils are found, plainly evinces that the matrix in which they are imbedded was formed by a gradual deposition, which entombed these animals whilst living in their native beds. The fine and delicate spinous projections of the shells are unbroken, and the spines are still found adhering to the crustaceous coverings of the *echini*; neither of which circumstances could have occurred had these bodies been suddenly and rudely overwhelmed by these investing depositions, or had they been brought hither from distant spots.

It may be said that the specimens possessing the characters here alluded to are rare. With respect to the spinous shells, however, they certainly occur often, although it is almost impossible to extricate them unbroken from their surrounding chalk; and the rarity of the specimens of *echinites* with their attached spines, depends in a great measure on the mode in which these specimens are obtained. The specimens seen in cabinets are seldom found by the naturalist himself, but are preserved by the work people, who break the chalk when any uncommon appearances catch their eye. But it frequently happens that these marks are not seen until the piece is broken by their tool, and with it, perhaps, the entire animal.

The perfect state of the surfaces of the chalk fossils proves also that this deposition proceeded from the surrounding fluid, and that it was not derived from the immediate action of any chemical agent on the shells and other calcareous coverings of the animals living at the bottom of the sea.

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In the fossil animal bodies found in chalk, not the least diminution of the sharpness of their ridges or points is observable, nor is the least dulness of the delicate lines and embossments of the crusts, or of the spines of the *echini*, to be detected.

That the deposition of chalk and of flint was sometimes alternate, and even, as it is expressed by Messrs. Cuvier and Brongniart, *periodical*, appears from the seams or strata of flinty nodules, and particularly from the widely extended flat or tabular flinty depositions interposed between the chalk.

But that the chalk was permeated by the silex at some distance of time after the deposition of the former, seems also to be proved by the state of the fossils of this stratum. There does not appear to be a single instance in which the animal remains are impregnated with silex. On the contrary, the substance of all these fossils has become calcareous spar, and their cavities have been filled with flint; thus plainly evincing that sufficient time must have elapsed for the crystallization of the calcareous spar, previously to the infiltration of the flint.

It may not be improper to remark, that in no instance does the flint, although in contact with the calcareous spar, appear to have become mixed with it. The reverse of this is the case with the chalk, since this latter may be seen in almost every degree of union with the flint; from being blended with its substance, to being merely united with its surface, and forming the white coat of the flint. It has been, without doubt, from certain appearances resulting from this union, that M. Carrosi and others have been led to believe in the change of lime to flint.

There can be hardly any hesitation in agreeing with Mr. Jameson, that the most probable explanation of the formation of imbedded flint is that which was first proposed by Werner, "that during the deposition of chalk, air was evolved, which, in endeavouring to escape, formed irregular cavities, that were afterwards filled up, by infiltration, with flint*." The decomposition of the softer parts of the animals, which were thus entombed, may be considered as a very probable source of a part of those gaseous matters which formed these cavities: and the connexion of the animal remains with these nodules of flint is easily explained by supposing the shells, crusts of the *echini*, &c. to have projected into these cavities, or to have been adherent to

* System of Mineralogy by Prof. Jameson, vol. i. p. 172.

their

their sides, at the period at which this infiltration took place.

That the separation and deposition of the matter forming these siliceous nodules have been the work of crystallization, is rendered evident by the cavities left either in these nodules, or in the fossils, being generally lined with quartz crystals.

Whilst endeavouring thus to explain the formation of these flinty nodules, and the filling up of the cavities of the fossils with flint, a difficulty arises from observing these bodies, insulated as it were in their bed of chalk; it not being easy to conceive, how so copious an infiltration should have taken place into these cavities, whilst the surrounding chalk should only have received a slight intermixture of siliceous grains.

Something analogous is however observable in the formation of the calcareous stalactite; since in those caverns in which these concretions have been forming for a very long period, the infiltration by which they are formed is found to continue to the present day; proving that the interstices of the superincumbent stone have not yet been filled by the concreting of the earthy particles held in solution in the percolating fluid, by the crystallization of which these bodies have been formed, and are now augmenting.

The Oberstein nodules of agate appear to have been formed under somewhat similar circumstances; since it is in general evident from their external surfaces, that they also have had very little adherence to their matrices: which would hardly have been the case had these been highly impregnated with silex.

The HARD CHALK lies immediately beneath the soft chalk. In this stratum there are no flint nodules. "Its beds," according to Mr. Farey, "increase in hardness, until near the bottom where a whitish freestone is dug, at Totternhoe in Bedfordshire, and at numerous other places: that brought from Ryegate and other quarries, of this stratum, south of London, is used as a fire stone*."

It has been generally supposed that these two strata of chalk are of one formation: but not only the absence of the flints but the characters of their fossils prove them to be of distinct formations. No fossils indeed are marked by more decidedly peculiar characters than those of this stratum; since hardly a single fossil has been found in it, which has been met with in the soft chalk, or any other stratum.

* Report on Derbyshire, &c. p. 112.

It is in this chalk that the genus *Ammonites* is first met with; or, in other words, it appears that the water which formed this stratum was that in which this genus last existed, no traces of it having been seen in the soft chalk or in the other superior strata. The chief, and perhaps the only circular species of this genus which has been found in this stratum, is of a large size, with nodular projections on its sides, towards the back, which is generally flat. This fossil appears to be of a different species from any of those that are found in the subjacent strata.

It is very remarkable that in this stratum, the last in which the genus *ammonites* is met with, so remarkable a deviation from the original form of the genus should occur, as almost to claim its being considered as the characteristic of another genus. In the fossil here referred to, which possesses all the other characters of *ammonites*, the spiral coil is disposed in a form rather approaching to that of the oval than the circle*.

In another fossil of this stratum, a still more extraordinary deviation exists. This fossil possesses the concamerations and the foliaceous sutures of the cornu ammonis; but, instead of being spirally coiled, it has its ends turned towards each other, somewhat in the form of a canoe. This peculiar form has led to the placing of this fossil under a separate genus, which has been named *Scaphites*†.

Of the extent of this stratum no correct account has been given; but there is sufficient reason for believing that it accompanies the other chalk in its range through this island. It also appears that its peculiar fossils exist in it at very considerable distances. Thus the *oval ammonite*, which is found in the Sussex hills, likewise occurs in the hard chalk of Wiltshire; and the *scaphites*, another inhabitant of the Sussex hills, has also been discovered in Dorsetshire.

On comparing the preceding sketch with the Essay on the Mineralogical Geography of the Neighbourhood of Paris, by Messrs. Cuvier and Brongniart, some important variations will be perceived between the strata found above the chalk in this island and in France. In France, the strata above the chalk differ both in number and quality from those which have been hitherto observed in a similar situation in England. In France, too, several strata of sand and sandstone exist above the strata of the gravel formation, which in this island appear to be highest.

* Organic Remains, vol. iii. pl. ix. fig. 6. † Ibid. vol. iii. pl. x. fig. 10 & 11.

The

The first of these differences appears to result chiefly from the existence of numerous beds or patches, the formation of which must have depended on certain local circumstances, such as the existence of fresh or salt water lakes, at the period of the drying up of a former ocean; the different chemical combinations which might thence have taken place, &c. But the occurrence of such variations can hardly be considered as interrupting the continuity of the stratification.

Indeed, when it is considered that in France much more frequent opportunities are afforded of examining the stratification immediately above the chalk than in England, it will not be regarded as improbable, that several of these beds or patches may exist here, the discovery of which would render the accordance of the two series of strata much more close.

Even from the examinations which have been already made, the identity of the French and English chalk is established. The British strata above the chalk are also found to contain patches of plastic clay, of most of the varieties mentioned in the French strata, as well as patches of coarse limestone, with its accompanying sand and its peculiar fossil shells, such as are found to exist in the corresponding French strata.

The other difference, the existence, in France, of beds of sand and of sandstone above those of gravel, which are the highest strata of this island, is very remarkable. May it not be attributable to the abruption, from this island, of the superior strata or beds of this formation, by that catastrophe, instances of the astonishing force of which have been already noticed?

XXXI. *Report of the National Vaccine Establishment.*

The Board of the National Vaccine Establishment having learned that great interest has been excited in the public mind, by the occurrence of small-pox after vaccination, in the families of the Earl of Grosvenor and of Sir Henry Martin, Bart. have thought it their duty to lay the following cases before the public, accompanied with some observations, and a statement how far, in their opinion, these cases affected the general advantages of vaccination.

The case of the Hon. Robert Grosvenor, third son of the Earl of Grosvenor, was procured through the favour of Sir Henry

Henry Halford and Sir Walter Farquhar, the physicians who attended the young gentleman during his illness; and the case of the son of Sir Henry Martin was obtained through the favour of Dr. Heberden. Both of these cases were also visited by the Director of the Vaccine Establishment.

I. *The Case of the Hon. Robert Grosvenor.*

On Sunday, May 26, 1811, the Hon. Robert Grosvenor, who was recovering from the hooping-cough, became much indisposed, and threw up his dinner. Fever followed, and he complained most particularly of excruciating pain in his back. He dwelt on this symptom until Thursday, when he became delirious, and there were observed on his face about twenty spots.

He had been vaccinated by Dr. Jenner, in his infancy, about ten years ago, and the mark left in his arm indicated a perfect disease.

On Friday morning, the eruption had not increased materially in point of number; but the appearance of the spots, and the previous symptoms, suggested strongly a suspicion that the disorder was the small-pox.

Sir H. Halford had occasion to go to Windsor in the afternoon of Friday, and did not see Mr. Robert Grosvenor until the Monday following (June 2d); but he learned from Sir W. Farquhar, who attended him most carefully during Sir Henry's absence, (and subsequently,) that the eruption had increased prodigiously in the course of Friday; that on the evening of that day Mr. Robert Grosvenor began to make bloody water, and that he continued to do so until Monday morning.

On the tenth day of the disease the pustules began to dry upon the face, which was swollen to a considerable degree, but not to the extent of closing his eyes, and was attended by a salivation, which lasted several days. Petechiæ had occurred in the interstices of several of the spots, particularly on the limbs, and there was that particular smell from the whole frame which is remarkable in bad cases of confluent small-pox.

It was obvious that the first symptoms of which Mr. Grosvenor complained, were such as indicated a violent disease about to follow; and Sir Henry confesses that he entertained a most unfavourable opinion of the issue of such a malady, when it was fully formed; having never seen an instance of recovery under so heavy an eruption attended by such circumstances. It seemed, however, that the latter stages of the disease were passed through more rapidly in this

this case than usual; and it may be a question whether this extraordinary circumstance, as well as the ultimate recovery of Mr. Grosvenor, were not influenced by previous vaccination.

HENRY HALFORD.
WALTER FARQUHAR.

In addition to the preceding account, the Board have authority to state, that during the illness of Mr. Grosvenor, the other children of the Earl of Grosvenor, who had been previously vaccinated, were exposed to the contagion of the small-pox under which their brother was suffering, and were also submitted to small-pox inoculation without effect.

II. *The Case of the Son of Sir Henry Martin.*

Sir Henry Martin's son, aged eleven years, was vaccinated by Mr. Tegart in the year 1801, and exhibited all the usual marks of that disorder in a complete and satisfactory manner. He still retains on his arm the characteristic scar.

This boy was taken ill on Saturday the 22d day of June, 1811; at the period of the attack he was recovering from hooping-cough.

23d. Continued to be feverish.

24th. Mr. Tegart was sent to.

25th. The fever increased, and at night he became delirious.

26th. An eruption was perceived chiefly about the mouth, at the same time his eyes and throat were slightly inflamed. The fever continued.

27th, or 2d day of the eruption, the pustules increased, so as to afford suspicion of the chicken-pox.

3d day of the eruption, the pustules increased, the fever decreased.

4th. At the close of the fourth day Dr. Heberden first saw this boy, with a distinct eruption of the most perfect kind of small-pox, all pretty uniform in size, well filled with a fluid already beginning to grow yellow, and surrounded by a rose-coloured margin precisely like small-pox of the fifth day. There were about one hundred pustules on the face, and perhaps twice as many on the limbs, but the trunk was almost free: the features were swollen, but not very much so. The skin was hot, and the pulse quick.

5th day. The pustules were more uniform, and yellow, and the patient complained of soreness; but he was cooler, and his pulse was quieter.

6th day. The fever had entirely subsided, and the pock began to turn.

8th.

8th. The pustules were dried, and continued to fall off from the face. The boy continued quite well.

Pall Mall, July 4, 1811. W. HEBERDEN.

With a view of obtaining the most accurate knowledge of the early symptoms of this case, which did not come under the immediate observation of Dr. Heberden, the Board have procured, through the favour of Mr. Tegart, of Pall Mall, an account of the commencement and course of the disorder, which corroborates the above statement. And from the same source they have been informed, that Miss Martin and a nurserymaid of Sir H. Martin's family, who had both been vaccinated, were inoculated with matter taken from Master Martin on the fifth day of the eruption, and were exposed to the contagion of the small-pox during the course of his disorder, without effect.

The Board are of opinion, that the case of the Hon. Robert Grosvenor was a case of confluent small pox. That the attack and progress of the disorder were attended by symptoms which almost invariably announce a fatal termination. But they observe, that the swelling of the face, which is generally so excessive as to close the eyes, and is considered as a favourable symptom, was slighter than usual; that on the tenth day the pustules began to dry upon the face; and that from that time the disease passed with unusual rapidity through the period when life is generally esteemed to be in the greatest hazard.

Those who are acquainted with the nature of the confluent small-pox, are aware that this peculiarity cannot be attributed to the effect of medical treatment.

The case of the son of Sir Henry Martin exhibits a mild form of distinct small-pox occurring after vaccination.

In most cases of small-pox which have succeeded to vaccination, the pustules have been observed to dry more rapidly, and the disorder has concluded at an earlier period than usual.

If allowance be made for the relative periods in which the confluent and distinct small-pox complete their course, the rapid progress towards recovery through the latter stage of confluent small-pox, as exhibited in the case of Mr. Grosvenor, may be compared with the rapid desiccation of the pustules in the distinct and peculiarly mild form of the disorder which is considered as small-pox modified by vaccination. Both forms of the disorder proceed in the usual course, the one attended with violent, the other with mild symptoms, till they arrive near to the height; when they

appear

appear to receive a check, and the recovery is unusually rapid.

From this correspondence of circumstances, the Board are induced to infer that in the case of Mr. Grosvenor, which has been more violent than any yet submitted to them, the progress of the disease, through its latter stage, and the consequent abatement of symptoms, were influenced by an antivariolous effect produced upon the constitution by the vaccine process.

The occurrence of small-pox after vaccination has been foreseen and pointed out in the Report on Vaccination made to Parliament, by the College of Physicians, in the year 1807, to which the Board are desirous of calling the attention of the public; wherein it is stated that,

"The security derived from vaccination against the small-pox, if not absolutely perfect, is as nearly so as can perhaps be expected from any human discovery; for amongst several hundred thousand cases, with the results of which the College have been made acquainted, the number of alleged failures has been surprisingly small, so much so as to form certainly no reasonable objection to the general adoption of vaccination; for it appears that there are not nearly so many failures in a given number of vaccinated persons, as there are deaths in an equal number of persons inoculated for the small-pox. Nothing can more clearly demonstrate the superiority of vaccination over the inoculation of the small-pox than this consideration; and it is a most important fact, which has been confirmed in the course of this inquiry, that in almost every case in which the small-pox has succeeded vaccination, whether by inoculation or by casual infection, the disease has varied much from its ordinary course; it has neither been the same in violence nor in the duration of its symptoms; but has, with very few exceptions, been remarkably mild, as if the small-pox had been deprived by the previous vaccine disease of its usual malignity."—Vide Report of the College of Physicians.

The peculiarities of certain constitutions with regard to eruptive fevers, form a curious subject of medical history. Some individuals have been more than once affected with scarlet fever and measles, others have been through life exposed to the contagion of these diseases without effect; many have resisted the inoculation and contagion of small-pox for several years, and have afterwards become susceptible of the disorder, and some have been twice affected with small-pox.

Among such infinite varieties of temperament it will not

appear

appear extraordinary, that vaccination, though so generally successful, should sometimes fail of rendering the human constitution unsusceptible of small-pox, especially since it has been found that in several instances small-pox has occurred to individuals over whom the small-pox inoculation had appeared to have produced its full influence. Three instances of this kind have taken place within the last month, and in another instance the natural small-pox has occurred a second time.

XXXII. *Intelligence and Miscellaneous Articles.*

A NEW COMET.

The last French papers contain the following account given by the astronomer Bouvard, and dated from the Imperial Observatory, August 21:—"The comet discovered at Viviers, on the 25th of last March, by M. de Flauguergues, and seen till the end of May, when it ceased to be visible, in consequence of its proximity to the Sun, has again appeared this morning in the constellation of the Little Lion. Its motion, almost entirely in declination, carries it towards the north, close to the constellation of the Great Bear, where it will then be visible every night, even to the naked eye. This morning, between three and four o'clock, I discovered this comet very near the horizon; its position was nearly that laid down in the elements calculated by M. Burckhardt; and according to my observations, I have determined it as follows:—Right ascension, 147 deg. 18 min.; North declination, 32 deg. 53 min."

The following is a letter from Dr. Olbers, of Bremen, to Prof. Boden, on the same subject: — "The comet which M. Flauguergues discovered on the 25th of March at Viviers, and M. Pons on the 11th of April at Marseilles, and which was seen on the 20th of May at Paris, will re-appear before the end of August. It will be much more visible then than in the spring. Its greatest brilliancy will be in October, and it may be visible still in December. In September and October it does not quit with us the southern part of the heaven."

LECTURES.

Dr. Clutterbuck will begin his Autumn Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, on Monday, Oct. 7th, at Ten o'clock in the Morning, at his House, No. 1, Crescent, New Bridge-street, where they will be continued daily at the same Hour: viz.

Theory

Theory and Practice, on Mondays, Wednesdays, and Fridays; Materia Medica and Chemistry, on Tuesdays, Thursdays, and Saturdays.

Clinical Lectures will be given occasionally during the Winter, on the most remarkable cases that occur in the practice of the General Dispensary; and Gentlemen preparing to pass the Army and Navy Medical Boards will be admitted to private Examinations previously, if desired. A Syllabus of the Course, with the Terms of Admission, may be had on application as above.

Dr. Clarke's and Mr. Clarke's Lectures on Midwifery, and the Diseases of Women and Children.

Dr. Clarke and Mr. Clarke will begin the Winter Course of their Lectures on Friday, October 4th.

The Lectures are read every Day at the House of Mr. Clarke, No. 10, Upper John-street, Golden-square, from a Quarter past Ten o'clock in the Morning till a Quarter past Eleven, for the convenience of Students attending the Hospitals.

The Students will be provided with Cases when properly qualified.

For Particulars apply to Dr. Clarke, No. 1, New Burlington-street; or to Mr. Clarke, No. 10, Upper John-street, Golden-square.

Medical and Chemical Lectures, St. George's Hospital, and George Street, Hanover Square.

These Medical Lectures will recommence, as usual, in the First Week of October, at Eight o'clock in the Morning, and the Chemical at a Quarter after Nine o'clock, at No. 9, George-street, Hanover-square.

Clinical Lectures are given on the Cases of Patients registered in St. George's Hospital, every Saturday Morning at Nine o'clock, by George Pearson, M.D. F.R.S. Senior Physician to St. George's Hospital, &c. &c.

The Terms of the Lectures, and of Physicians' Pupils, with other Particulars, may be known from the Proposals to be had at St. George's Hospital and in George-street.

St. Thomas's and Guy's Hospitals.

The Winter Courses of Lectures at these adjoining Hospitals will commence the First Week of October, viz.

At St. Thomas's. Anatomy, and the Operations of Surgery,

gery, by Mr. Cline and Mr. Ashley Cooper.—Principles and Practice of Surgery, by Mr. A. Cooper.

At Guy's. Practice of Medicine, by Dr. Babington and Dr. Curry.—Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen.—Experimental Philosophy, by Mr. Allen.—Theory of Medicine, and Materia Medica, by Dr. Curry and Dr. Cholmeley.—Midwifery, and Diseases of Women and Children, by Dr. Haighton.—Physiology, or Laws of the Animal Œconomy, by Dr. Haighton.—Structure and Diseases of the Teeth, by Mr. Fox.

N. B. These several Lectures are so arranged that no two of them interfere in the hours of attendance; and the whole is calculated to form a complete Course of Medical and Chirurgical Instruction. Terms and other Particulars may be learnt at the respective Hospitals.

LIST OF PATENTS FOR NEW INVENTIONS.

To Timothy Sheldrake, of the Strand, Westminster, in the county of Middlesex, mechanic, for certain wheels, which, when combined together, will constitute a moving power of great force, by the application of which to many engines, machines, and machinery, that are now set in motion by steam, wind, water, or animal power, the effect of the said known powers will be greatly increased, and the labour of men or horses that are now employed on the said engines, machines, or machinery, will be diminished; which wheels may be introduced, in part or in the whole, into many engines, machines, or machinery, for whatever uses they may be employed, instead of the wheels and pinions by which such engines, machines, or machinery, are generally kept in motion; and which wheels, when so introduced, will work with much less friction, and much greater velocity, than those which are usually employed.—June 15, 1811.

To Charles Hamond, of Milk-street, Cheapside, London, gent., for his improved machine for sawing, cutting and planing wood.—June 27.

To Thomas Attwood, of Birmingham, esq., and Benj. Cook of the same place, gilt toy makers, for their new method of combining and connecting together different kinds of metals, and of combining and connecting metals and wood together, in such way as to make the combination thereof, whether the same be of metals or of metals and wood, have one appearance or representation only.—June 27.

To Sir Howard Douglas, of High Wycombe, in the county of Bucks. bart. for his improved reflecting circle or semicircle.—July 2.

XXXIII. *Account of the Pitch Lake of the Island of Trinidad.* By Nicholas Nugent, *M.D. Honorary Member of the Geological Society**.

Being desirous to visit the celebrated lake of pitch, previously to my departure from the Island of Trinidad, I embarked with that intention in the month of October, 1807, in a small vessel at Port Spain. After a pleasant sail of about thirty miles down the Gulf of Paria, we arrived at the point la Braye, so called by the French from its characteristic feature. It is a considerable headland, about eighty feet above the level of the sea, and perhaps two miles long and two broad. We landed on the southern side of the point, at the plantation of Mr. Vessigny: as the boat drew near the shore, I was struck with the appearance of a rocky bluff or small promontory of a reddish-brown colour, very different from the pitch which I had expected to find on the whole shore. Upon examining this spot, I found it composed of a substance corresponding to the porcelain jasper of mineralogists, generally of a red colour where it had been exposed to the weather, but of light slate-blue in the interior; it is a very hard stone with a conchoidal fracture, some degree of lustre, and is perfectly opake even at the edges; in some places, from the action of the air, it was of a reddish- or yellowish-brown, and an earthy appearance. I wished to have devoted more time to the investigation of what in the language of the Wernerian school is termed the geognostic relations of this spot, but my companions were anxious to proceed. We ascended the hill, which was entirely composed of this rock, to the plantation, where we procured a negro guide, who conducted us through a wood about three quarters of a mile. We now perceived a strong sulphureous and pitchy smell, like that of burning coal, and soon after had a view of the lake, which at first sight appeared to be an expanse of still water, frequently interrupted by clumps of dwarf trees or islets of rushes and shrubs: but on a nearer approach we found it to be in reality an extensive plain of mineral pitch, with frequent crevices and chasms filled with water. The singularity of the scene was altogether so great, that it was some time before I could recover from my surprise so as to investigate it minutely. The surface of the lake is of the colour of ashes, and at this season was not polished or smooth

* From Transactions of the Geological Society, vol. i.

so as to be slippery; the hardness or consistence was such as to bear any weight; and it was not adhesive, though it partially received the impression of the foot; it bore us without any tremulous motion whatever, and several head of cattle were browsing on it in perfect security. In the dry season, however, the surface is much more yielding, and must be in a state approaching to fluidity, as is shown by pieces of recent wood and other substances being enveloped in it. Even large branches of trees which were a foot above the level, had in some way become enveloped in the bituminous matter. The interstices or chasms are very numerous, ramifying and joining in every direction, and in the wet season, being filled with water, present the only obstacle to walking over the surface: these cavities are generally deep in proportion to their width, some being only a few inches in depth, others several feet, and many almost unfathomable: the water in them is good, and uncontaminated by the pitch; the people of the neighbourhood derive their supply from this source, and refresh themselves by bathing in it; fish are caught in it, and particularly a very good species of mullet. The arrangement of the chasms is very singular: the sides, which of course are formed of the pitch, are invariably shelving from the surface, so as nearly to meet at the bottom, but then they bulge out towards each other with a considerable degree of convexity. This may be supposed to arise from the tendency in the pitch slowly to coalesce, whenever softened by the intensity of the sun's rays. These crevices are known occasionally to close up entirely, and we saw many marks or seams from this cause. How these crevices originate it may not be so easy to explain. One of our party suggested that the whole mass of pitch might be supported by the water which made its way through accidental rents; but in the solid state it is of greater specific gravity than water, for several bits thrown into one of the pools immediately sank*. The lake (I call it so, because I think the common name appropriate enough) contains many islets covered with long grass and shrubs, which are the haunts of birds of the most exquisite plumage, as the pools are of snipe and plover. Alligators are also said to abound here; but it was not our

* Pieces of asphaltum are, I believe, frequently found floating on the Dead Sea in Palestine; but this arises probably from the extraordinary specific gravity of the waters of that lake, which Dr. Marcet found to be 1·211. Mr. Hatchett states the specific gravity of ordinary asphaltum to vary from 1·023 to 1·165, but in two varieties of that of Trinidad it was as great as 1·336 and 1·744, which led Mr. Hatchett to form a conjecture which I shall afterwards notice.

lot

lot to encounter any of these animals. It is not easy to state precisely the extent of this great collection of pitch; the line between it and the neighbouring soil is not always well defined, and indeed it appears to form the substratum of the surrounding tract of land. We may say, however, that it is bounded on the north and west sides by the sea, on the south by the rocky eminence of porcelain jasper before mentioned, and on the east by the usual argillaceous soil of the country; the main body may perhaps be estimated at three miles in circumference; the depth cannot be ascertained, and no subjacent rock or soil can be discovered. Where the bitumen is slightly covered by soil, there are plantations of cassava, plantains and pine-apples, the last of which grow with luxuriance and attain to great perfection. There are three or four French and one English sugar estates in the immediate neighbourhood: our opinion of the soil did not, however, coincide with that of Mr. Anderson, who in the account he gave some years ago thought it very fertile. It is worthy of remark, that the main body of the pitch, which may properly be called the lake, is situated higher than the adjoining land, and that you descend by a gentle slope to the sea, where the pitch is much contaminated by the sand of the beach. During the dry season, as I have before remarked, this pitch is much softened, so that different bodies have been known slowly to sink into it: if a quantity be cut out, the cavity left will be shortly filled up; and I have heard it related, that when the Spaniards undertook formerly to prepare the pitch for œconomical purposes, and had imprudently erected their cauldrons on the very lake, they completely sank in the course of a night, so as to defeat their intentions. Numberless proofs are given of its being at times in this softened state: the negro houses of the vicinage, for instance, built by driving posts in the earth, frequently are twisted or sunk on one side. In many places it seems to have actually overflown like lava, and presents the wrinkled appearance which a sluggish substance would exhibit in motion.

This substance is generally thought to be the asphaltum of naturalists: in different spots, however, it presents different appearances. In some parts it is black, with a splintery conchoidal fracture, of considerable specific gravity, with little or no lustre, resembling particular kinds of coal, and so hard as to require a severe blow of the hammer to detach or break it; in other parts, it is so much softer, as to allow one to cut out a piece in any form with a spade or hatchet, and in the interior is vesicular and oily: this is the character of by far the greater portion of the whole mass; in one place, it bubbles up in a perfectly fluid state, so that you may take it up in a cup; and I am informed that in one of the neighbouring plantations there is a spot where it is of a bright colour, shining, transparent and brittle, like bottle-glass or resin. The odour in all these instances is strong, and like that of a combination of pitch and sulphur. No sulphur, however, is any where to be perceived; but from the strong exhalation of that substance and the affinity which is known to exist between the fluid bitumens and it, much is, no doubt, contained in a state of combination: a bit of the pitch held in the candle melts like sealing-wax and burns with a light flame, which is extinguished whenever it is removed, and on cooling the bitumen hardens again. From this property it is sufficiently evident that this substance may be converted to many useful purposes, and accordingly it is universally used in the country wherever pitch is required; and the reports of the naval officers who have tried it are favourable to its more general adoption: it is requisite merely to prepare it with a proportion of oil, tallow, or common tar, to give it a sufficient degree of fluidity. In this point of view, this lake is of vast national importance, and more especially to a great maritime power. It is indeed singular that the attention of government should not have been more forcibly directed to a subject of such magnitude: the attempts that have hitherto been made to render it extensively useful have for the most part been only feeble and injudicious, and have consequently proved abortive. This vast collection of bitumen might in all probability afford an inexhaustible supply of an essential article of naval stores, and being situated on the margin of the sea could be wrought and shipped with little inconvenience or expense*. It would however be great injustice to sir Alexander Cochrane not to state explicitly that he has at various times, during his long and active command on the Leeward Island station, taken considerable pains to insure a proper and fair trial of this mineral production for the highly important uses of which it is generally believed to be capable. But whether it has arisen from certain perverse occurrences or from the prejudice of the mechanical superintendants of the colonial dock-yards, or really, as some have pretended, from an absolute unfitness of the substance in question; the views of

* This island contains also a great quantity of valuable timber, and several plants which yield excellent hemp.

the

the gallant admiral have, I believe, been invariably thwarted, or his exertions rendered altogether fruitless. I was at Antigua in 1809, when a transport arrived laden with this pitch for the use of the dock-yard at English Harbour: it had evidently been hastily collected with little care or zeal from the beach, and was of course much contaminated with sand and other foreign substances. The best way would probably be to have it properly prepared on the spot, and brought to the state in which it may be serviceable, previously to its exportation. I have frequently seen it used to pay the bottoms of small vessels, for which it is particularly well adapted, as it preserves them from the numerous tribe of worms so abundant in tropical countries*. There seems indeed no reason why it should not when duly prepared and attenuated be applicable to all the purposes of the petroleum of Zante, a well-known article of commerce in the Adriatic, or that of the district in Burmah, where 400,000 hogsheads are said to be collected annually †.

It is observed by Capt. Mallet, in his Short Topographical Sketch of the island, that "near Cape la Brea (la Braye) a little to the south-west, is a gulf or vortex, which in stormy weather gushes out, raising the water five or six feet, and covers the surface for a considerable space with petroleum or tar:" and he adds, that "on the east coast in the Bay of Mayaro, there is another gulf or vortex similar to the former, which in the months of March and June produces a detonation like thunder, having some flame with a thick black smoke, which vanishes away immediately: in about twenty-four hours afterwards is found along the shore of the bay a quantity of bitumen or pitch, about three or four inches thick, which is employed with success." Captain Mallet likewise quotes Gumilla, as stating in his Description of the Orinoco, that about seventy years ago "a spot of land on the western coast of this island, near half way between the capital, an Indian village sank suddenly, and was immediately replaced by a small lake of pitch, to the great terror of the inhabitants."

I have had no opportunity of ascertaining personally whether these statements are accurate, though sufficiently probable from what is known to occur in other parts of the world;

* The different kinds of bitumen have always been found particularly obnoxious to the class of insects. There can be little doubt but that they formed ingredients in the Egyptian compost for embalming bodies, and the Arabians are said to avail themselves of them in preserving the trappings of their horses. Vide Jameson's Mineralogy.

† Vide Aikin's Dictionary of Chemistry, quoted from Captain Cox in the Asiatic Researches.

world; but I have been informed by several persons that the sea in the neighbourhood of La Braye is occasionally covered with a fluid bitumen, and in the south-eastern part of the island there is certainly a similar collection of this bitumen, though of less extent, and many small detached spots of it are to be met with in the woods: it is even said that an evident line of communication may thus be traced between the two great receptacles. There is every probability, that in all these cases the pitch was originally fluid, and has since become inspissated by exposure to the air, as happens in the Dead Sea and other parts of the East.

It is for geologists to explain the origin of this singular phænomenon, and each sect will doubtless give a solution of the difficulty according to its peculiar tenets. To frame any very satisfactory hypothesis on the subject, would require a more exact investigation of the neighbouring country, and particularly to the southward and eastward, which I had not an opportunity of visiting. And it must be remembered that geological inquiries are not conducted here with that facility which they are in some other parts of the world: the soil is almost universally covered with the thickest and most luxuriant vegetation, and the stranger is soon exhausted and overcome by the scorching rays of a vertical sun. Immediately to the southward, the face of the country as seen from la Braye, is a good deal broken and rugged, which Mr. Anderson attributes to some convulsion of nature from subterranean fires, in which idea he is confirmed by having found in the neighbouring woods several hot springs. He is indeed of opinion that this tract has experienced the effects of the volcanic power, which, as he supposes, elevated the great mountains on the main and the northern side of the island*. The production of all bituminous substances has certainly with plausibility been attributed to the action of subterranean fires on beds of coal, being separated in a similar manner as when effected by artificial heat, and thus they may be traced through the various transformations of vegetable matter. I was accordingly particular in my inquiries with regard to the existence of beds of coal, but could not learn that there was any certain trace of that substance in the island; and though it may exist at a great depth, I saw no strata that indicate it. A friend, indeed, gave me specimens of a kind of bituminous shale mixed with sand, which he brought from Point Cedar, about twenty miles distant, and I find Mr. Anderson speaks

* Vide Philos. Trans. vol. lxxix. or Ann. Register for 1789.

of

of the soil near the pitch lake containing burnt cinders, but I imagine he may have taken for them the small fragments of the bitumen itself.

An examination of this tract of country could not fail, I think, to be highly gratifying to those who embrace the Huttonian theory of the earth; for they might behold the numerous branches of one of the largest rivers of the world (the Orinoco) bringing down so amazing a quantity of earthy particles as to discolour the sea in a most remarkable manner for many leagues distant*; they might see these earthy particles deposited by the influence of powerful currents on the shores of the Gulf of Paria, and particularly on the western side of the island of Trinidad; they might there find vast collections of bituminous substances, beds of porcelain jasper and such other bodies as may readily be supposed to arise from the modified action of heat on such vegetable and earthy materials as the waters are known actually to deposit. They would further perceive no very vague traces of subterranean fire, by which these changes may have been effected and the whole tract elevated above the ordinary level of the general loose soil of the country: as for instance, hot springs, the vortices above mentioned, the frequent occurrence of earthquakes, and two singular semi-volcanic mounds at Point Icaque, which, though not very near, throw light on the general character of the country. Without pledging myself to any particular system of geology, I confess an explanation similar to this appears to me sufficiently probable, and consonant with the known phænomena of nature. A vast river, like the Orinoco,

* No scene can be more magnificent than that presented on a near approach to the north-western coast of Trinidad. The sea is not only changed from a light green to a deep brown colour, but has in an extraordinary degree that rippling, confused and whirling motion, which arises from the violence of contending currents, and which prevail here in so remarkable a manner, particularly at those seasons when the Orinoco is so swollen by periodical rains, that vessels are not unfrequently several days or weeks in stemming them, or perhaps are irresistibly borne before them far out of their destined tract. The dark verdure of lofty mountains, covered with impenetrable woods to the very summits, whence, in the most humid of climates, torrents impetuously rush through deep ravines to the sea; three narrow passages into the Gulf of Paria, between rugged mountains of brown micaceous schist, on whose cavernous sides the eddying surge dashes with fury, and where a vessel must necessarily be for some time embayed, with a depth of water scarcely to be fathomed by the lead,—present altogether a scene which may well be conceived to have impressed the mind of the navigator who first beheld it with considerable surprise and awe. Columbus made this land in his third voyage, and gave it the name of the *Bocas del Drago*. From the wonderful discoloration and turbidity of the water, he sagaciously concluded that a very large river was near, and consequently a great continent.

must for ages have rolled down great quantities of woody and vegetable bodies, which from certain causes,—as the influence of currents and eddies,—may have been arrested and accumulated in particular places; they may there have undergone those transformations and chemical changes which various vegetable substances similarly situated have been proved to suffer in other parts of the world. An accidental fire, such as is known frequently to occur in the bowels of the earth, may then have operated in separating and driving off the newly formed bitumen more or less combined with siliceous and argillaceous earths, which forcing its way through the surface, and afterwards becoming inspissated by exposure to the air, may have occasioned such scenes as I have ventured to describe. The only other country accurately resembling this part of Trinidad, of which I recollect to have read, is that which borders on the Gulf of Taman in Crim Tartary: from the representation of travellers, springs of naphtha and petroleum equally abound, and they describe volcanic mounds precisely similar to those of Point Icaque. Pallas's explanation of their origin seems to me very satisfactory; and I think it not improbable that the river Don and Sea of Azof may have acted the same part in producing these appearances in the one case, as the Orinoco and Gulf of Paria appear to have done in the other *. It may be supposed that the destruction of a forest or perhaps even a great savanna on the spot, would be a more obvious mode of accounting for this singular phænomenon; but, as I shall immediately state, all this part of the island is of recent alluvial formation, and the land all along this coast is daily receiving a considerable accession from the surrounding water. The pitch lake with the circumjacent tract being now on the margin of the sea, must in like manner have had an origin of no very distant date; besides, according to the above representation of Capt. Mallet, and which has been frequently corroborated, a fluid bitumen oozes up and rises to the surface of the water on both sides of the island, not where the sea has encroached on and overwhelmed the ready-formed land, but where it is obviously in a very rapid manner depositing and forming a new soil.

From a consideration of the great hardness, the specific gravity, and the general external characters of the specimens submitted a few years ago to the examination of Mr. Hatchett,

* Vide Universal Magazine for February 1808, Mrs. Guthrie's Tour in the Tauride, or Voyages de Pallas.

that

that gentleman was led to suppose that a considerable part of the aggregate mass at Trinidad was not pure mineral pitch or asphaltum, but rather a porous stone of the argillaceous genus, much impregnated with bitumen. Two specimens of the more compact and earthy sort, analysed by Mr. Hatchett, yielded about 32 and 36 per cent. of pure bitumen: the residuum in the crucible consisted of a spongy, friable and ochraceous stone; and 100 parts of it afforded, as far as could be determined by a single trial, of silica 60, alumina 10, oxide of iron 10, carbonaceous matter by estimation 11; not the smallest traces of lime could be discovered; so that the substance has no similarity to the bituminous limestones which have been noticed in different parts of the world*. I have already remarked, that this mineral production differs considerably in different places. The specimens examined by Mr. Hatchett by no means correspond in character with the great mass of the lake, which, in most cases, would doubtless be found to be infinitely more free from combination with earthy substances; though from the mode of origin which I have assigned to it, this intermixture may be regarded as more or less unavoidable. The analysis of the stone after the separation of the bitumen, as Mr. Hatchett very correctly observes, accords with the prevalent soil of the country; and I may add, with the soil daily deposited by the gulf, and with the composition of the porcelain jasper in immediate contact with the bituminous mass.

All the country which I have visited in Trinidad is either decidedly primitive or alluvial. The great northern range of mountains which runs from east to west, and is connected with the Highlands of Paria on the continent by the Islands at the Bocas, consists of gneiss, of mica slate containing great masses of quartz, and in many places approaching so much to the nature of talc as to render the soil quite unctuous by its decomposition, and of compact blueish gray limestone, with frequent veins of white crystallized carbonate of lime. From the foot of these mountains, for many leagues to the southward, there is little else than a thick fertile argillaceous soil, without a stone or a single pebble. This tract of land, which is low and perfectly level, is evidently formed by the *detritus* of the mountains, and by the copious tribute of the waters of the Orinoco, which being deposited by the influence of currents, gradually accumulates; and in a climate where vegetation is astonishingly rapid, is speedily covered with the mangrove

* Vide Linnean Trans. vol. viii.

and

and other woods. It is accordingly observed, that the leeward side of the island constantly encroaches on the gulf, and marine shells are frequently found on the land at a considerable distance from the sea. This is the character of Naparima and the greater part of the country I saw along the coast to la Braye. It is not only in forming and extending the coast of Trinidad, that the Orinoco exerts its powerful agency; co-operating with its mighty sister flood, the Amazons, it has manifestly formed all that line of coast and vast extent of country included between the extreme branches of each river. To use the language of a writer in the Philosophical Transactions of Edinburgh: "If you cast your eye upon the map, you will observe from Cayenne to the bottom of the Gulf of Paria this immense tract of swamp, formed by the sediment of these rivers, and a similar tract of shallow muddy coast, which their continued operation will one day elevate. The sediment of the Amazons is carried down thus to leeward (the westward) by the constant currents which set along from the southward and the coast of Brazil. That of the Oroonoko is detained and allowed to settle near its mouths by the opposite island of Trinidad, and still more by the mountains on the main, which are only separated from that island by the Bocos del Drago. The coast of Guiana has remained, as it were, the great eddy or resting-place for the washings of great part of South America for ages; and its own comparatively small streams have but modified here and there the grand deposit *."

Having been amply gratified with our visit to this singular place, which to the usual magnificence of the West Indian landscape unites the striking peculiarity of the local scene, we re-embarked in our vessel, and stood along the coast on our return. On the way we landed, and visited the plantations of several gentlemen, who received us with hospitality, and made us more fully acquainted with the state of this island: a colony which may with truth be described as fortunate in its situation, fertile in its soil, and rich beyond measure in the productions of nature; presenting, in short, by a rare combination, all which can gratify the curiosity of the naturalist, or the cupidity of the planter; restrained in the development of its astonishing resources, only by the inadequacy of population, the tedious and ill-defined forms of Spanish justice, and the severe, though we may hope transient, pressure of the times.

* Vide Mr. Lochhead's Observ. on the Nat. Hist. of Guiana. Edin. Trans. vol. iv.

XXXIV. *Ex-*

XXXIV. *Experiments and Observations on the different Modes in which Death is produced by certain vegetable Poisons. By* B. C. Brodie, *Esq. F.R.S. Communicated by the Society for promoting the Knowledge of Animal Chemistry.*

[Concluded from p. 94.]

*Experiments with the Empyreumatic Oil of Tobacco**.

Exp. 13. Less than a drop of this oil was applied to the tongue of a young cat. Instantly violent convulsions took place in all the muscles, and the respirations became very frequent. In five minutes after the application, she lay on one side insensible, with slight spasmodic actions of the muscles. At the end of eleven minutes she retched, but did not vomit. In a quarter of an hour she appeared to be recovering. I repeated the application of the poison, and she was again seized with violent convulsions, and became insensible, breathing at long intervals; and in two minutes from the second application respiration had entirely ceased, and she was apparently dead. On opening the thorax, I found the heart acting with regularity and strength, circulating dark-coloured blood. I introduced a tube into the trachea, and produced artificial respiration; the contractions of the heart became augmented in force and frequency, and there was no evident diminution in six or seven minutes, during which the artificial respiration was continued.

On dissection, nothing remarkable was found in the appearance of the tongue or brain.

The symptoms and mode of death, in this experiment, did not essentially differ from those produced by the essential oil of almonds. I was surprised to find the effects of the empyreumatic oil so entirely different from those of the infusion of tobacco. Supposing that this difference might arise from the poison being more concentrated in the oil than in the infusion, I made the following experiments.

Exp. 14. A drop of the oil of tobacco was suspended in an ounce and a half of water by means of mucilage of gum arabic, and the whole was injected into the rectum of a dog. In two minutes afterwards he became faint, retched, but did not vomit. He appeared to be recovering from this state, and in twenty-five minutes after the first injection it was repeated

* I was furnished with the empyreumatic oil of tobacco by Mr. W. Brande. It may be procured by subjecting the leaves of tobacco to distillation in a heat above that of boiling water: a quantity of watery fluid comes over, on the surface of which is a thin film of unctuous substance.

repeated in the same quantity. He was then seized with symptoms similar to those in the last experiment, and in two minutes and a half he was apparently dead.

Two minutes after apparent death, on the thorax being opened into, the heart was found acting regularly one hundred times in a minute, and it continued acting for several minutes.

Exp. 15. A drop of the empyreumatic oil of tobacco with an ounce of water was injected into the rectum of a cat. The symptoms produced were in essential circumstances similar to those which occurred in the last experiment. The animal was apparently dead in five minutes after the injection, and the heart continued to contract for several minutes afterwards.

We may conclude from these experiments, that the empyreumatic oil of tobacco, whether applied to the tongue or injected into the intestine, does not stop the action of the heart and induce syncope, like the infusion of tobacco; but that it occasions death by destroying the functions of the brain, without directly acting on the circulation. In other words, its effects are similar to those of alcohol, the juice of aconite, and the essential oil of almonds.

III. *Experiments with Poisons applied to wounded Surfaces.*

Experiments with the Essential Oil of Almonds.

Exp. 16. I made an incision in the thigh of a rabbit, and introduced two drops of essential oil between the skin and the muscles. In four minutes after the application, he was seized with violent convulsions, and became insensible, and in two minutes more he was apparently dead; but the heart was felt through the ribs acting one hundred and twenty times in a minute, and it continued acting for several minutes. There were no other appearances in the limb, than would have resulted from an ordinary wound.

Exp. 17. Two drops of the essential oil of almonds were introduced into a wound in the side of a mouse. Two minutes afterwards he was affected with symptoms similar to those which occurred in the last experiment, and in two minutes more he was apparently dead, but the heart continued to contract for some minutes afterwards.

From the experiments which I have just related, and from others which it appears unnecessary to detail, as the general results were the same, I have learned that where the essential oil of almonds is applied to a wound, its effects are not so instantaneous as when it is applied to the tongue; otherwise

otherwise there is no difference in its effects, in whatever manner it is applied.

Experiments with the Juice of the Leaves of Aconite.

Exp. 18. I made a wound in the side of a young rabbit, and introduced, between the skin and muscles, about twenty drops of the juice of aconite. Twenty-three minutes afterwards he was affected with symptoms in all essential respects similar to those which occurred in an experiment already related, where the juice was injected into the rectum, and at the end of forty-seven minutes from the application of the poison he was apparently dead. Two minutes after apparent death, the heart was found contracting, but very feebly.

Experiments with the Woorara.*

Exp. 19. A small quantity of the woorara in powder was applied to a wound in the side of a Guinea pig. Ten minutes afterwards the animal was unable to walk; then he became quite motionless, except some slight occasional convulsions. He gradually became insensible; the respirations were laboured, and at the end of fourteen minutes from the application of the poison the respiration had entirely ceased, and he was apparently dead; but on opening the thorax, the heart was found acting seventy times in a minute, circulating dark-coloured blood, and it continued to contract for several minutes afterwards. On dissection no preternatural appearances were observed in the brain, nor was there any other appearance in the limb than would have arisen from an ordinary wound.

Exp. 20. I made a wound in the side of a Guinea pig, and introduced into it about two grains of the woorara in powder. At the end of twenty-five minutes, symptoms took place very similar to those which occurred in the last experiment, and in thirteen minutes more the animal was apparently dead; but the heart continued to contract one hundred and eight times in a minute, and by means of artificial respiration the circulation was kept up for more than twenty minutes.

The results of other experiments which I have made with the woorara were similar to those just described. The heart continued to act after apparent death, and the circulation

* The Woorara is a poison with which the Indians of Guiana arm the points of their arrows. It appears not to differ essentially from the Ticunas, which was employed in the experiments of the Abbé Fontana. I am indebted to Dr. E. N. Bancroft, who not only furnished me with some of the woorara which he had in his possession, but also lent me his assistance in the experiments which were made with it.

might

might be kept up by means of artificial respiration. It is evident that this poison acts in some way or other on the brain, and that the cessation of the functions of this organ is the immediate cause of death.

I found in these experiments, that the best mode of applying the woorara is when it is dissolved in water to the consistence of a thin paste. I first made the wound, and then smeared the poison over it with the end of the scalpel. I found that the animal was more speedily and certainly affected, if there was some hæmorrhage, unless the hæmorrhage was very copious, when it produced an opposite effect, by washing the poison away from the wound. When the poison was applied in large quantity, it sometimes began to act in six or seven minutes. Never more than half an hour elapsed from the time of the poison being inserted, to that of the animal being affected, except in one instance, where a ligature was applied on the limb, which will be mentioned afterwards. The woorara, which I employed, had been preserved for some years, which will account for its having been less active than it has been described to be by those who had witnessed its effects when in a recent state.

*Experiments with the Upas Antiar**.

Exp. 21. About two grains of this poison were made into a thin paste with water, and inserted into a wound in the thigh of a dog. Twelve minutes afterwards he became languid; at the end of fifteen minutes, the heart was found to beat very irregularly, and with frequent intermissions; after this, he had a slight rigor. At the end of twenty minutes, the heart beat very feebly and irregularly; he was languid; was sick and vomited; but the respirations were as frequent and as full as under natural circumstances, and he was perfectly sensible. At the end of twenty minutes he suddenly fell on one side, and was apparently dead. I immediately opened into the thorax, and found the heart distended with blood in a very remarkable degree, and to have entirely ceased contracting. There was one distinct and full inspiration after I had begun making the incision into the thorax. The cavities of the left side of the heart contained scarlet blood, and those of the right side contained dark-coloured blood, as in a living animal.

Exp. 22. A small quantity of the upas antiar, prepared

* We are informed that the island of Java produces two powerful vegetable poisons to one of which the natives give the name of *Upas tieutè*, and to the other that of *Upas antiar*. I was supplied with a quantity of the latter through the kindness of Mr. Marsden, who had some of it in his possession.

as

as before, was inserted into a wound in the thigh of a young cat. She appeared languid in two minutes after the poison was inserted. The symptoms which took place did not essentially differ from those which occurred in the last experiment, except that there were some convulsive motions of the limbs. At eight minutes after the poison was inserted, she lay on one side motionless and insensible, the heart could not be felt, but the respiration had not entirely ceased. On opening into the thorax, I found the heart to have ceased contracting. It was much distended with blood: and the blood in the cavities of the left side was of a scarlet colour. There were two full inspirations after the incision of the thorax was begun. On irritating the heart with the point of the sçalpel, slight contractions took place in the fibres of the appendices of the auricles, but none in any other part.

Exp. 23. The experiment was repeated on a rabbit. The symptoms produced were similar to those in the last experiment; but the animal did not vomit, and the convulsive motions were in a less degree: he died eleven minutes after the poison was inserted. On opening the chest, the heart was found to have entirely ceased contracting; it was much distended with blood; and the blood in the cavities of the left side was of a scarlet colour. On irritating the heart with the point of the scalpel, the ventricles contracted, but not sufficiently to restore the circulation.

Exp. 24. About a grain of the upas antiar was inserted into a wound in the side of a rabbit. He was affected with symptoms similar to those before described, and died in ten minutes after the poison was applied. On opening the thorax immediately after death, the heart was found to have ceased contracting, and the blood in the cavities of the left side was of a scarlet colour.

It appears from these experiments, that the upas antiar, when inserted into a wound, produces death (as infusion of tobacco does when injected into the intestines) by rendering the heart insensible to the stimulus of the blood, and stopping the circulation. The heart beats feebly and irregularly before either the functions of the mind or the respiration appear to be affected. Respiration is performed even after the circulation has ceased; and the left side of the heart is found after death to contain scarlet blood, which never can be the çase where the cause of death is the cessation of the functions of the brain or lungs. The convulsions which occur when the circulation has nearly ceased, probably arise from the diminution of the supply of blood to the brain, resembling

resembling those which take place in a person who is dying from hæmorrhage.

There remains an interesting subject of inquiry, "through what medium do poisons influence the brain when applied to wounds?" That poisons applied in this manner do not produce their effects precisely in the same way as poisons taken internally, is rendered probable by this circumstance; that some poisons, which are very powerful when applied to wounds even in small quantities, are either altogether inefficient when taken internally, or require to be given in very large quantities, in order to produce their effect, and *vice versâ*.

A poison applied to a wounded surface may be supposed to act on the brain in one of three ways,

1. By means of the nerves, like poisons taken internally.

2. By passing into the circulation through the absorbent vessels.

3. By passing directly into the circulation through the divided veins.

Exp. 25. In order to ascertain whether the woorara acts through the medium of the nerves, I exposed the axilla of a rabbit, and divided the spinal nerves supplying the upper extremity, just before they unite to form the axillary plexus. The operation was performed with the greatest care. I not only divided every nervous filament, however small, which I could detect, but every portion of cellular membrane in the axilla, so that the artery and vein were left entirely insulated. I then made two wounds in the fore-arm, and inserted into them some of the woorara formed into a paste. Fourteen minutes after the poison was applied, the hind legs became paralytic, and in ten minutes more he died; with symptoms precisely similar to those which took place in the former experiments, and the heart continued to act after apparent death. On dissection, the nerves of the upper extremity were particularly examined, but not the smallest filament could be found undivided.

I made the following experiment, to ascertain whether the woorara passes into the circulation through the absorbent vessels.

Exp. 26. I tied a ligature round the thoracic duct of a dog, just before it perforates the angle of the left subclavian and jugular veins. I then made two wounds in the left hind leg, and introduced some of the woorara in powder into them. In less than a quarter of an hour he became affected with the usual symptoms, and died in a few minutes afterwards.

After

After death, I dissected the thoracic duct with great care. I found it to have been perfectly secured by the ligature. It was very much distended with chyle; and about two inches below its termination its coats had given way, and chyle was extravasated into the cellular membrane. The lymphatic vessels in the left axilla were distended in a very remarkable degree; and on dividing them, not less than a drachm of lymph issued from the divided ends.

Since neither the division of the nerves nor the obstruction of the thoracic duct interfere in the slightest degree with the effects of the woorara, there is presumptive evidence that it acts on the brain by entering the circulation through the divided veins. I endeavoured to ascertain, by experiment, whether this is really the case.

To apply ligatures to the large vessels of a limb only would evidently lead to no satisfactory conclusion, since the anastomosing vessels might still carry on the circulation. The only way which I could devise of performing the experiment, was to include all the vessels, small as well as large, in a ligature.

Exp. 27. In order to make the experiment more satisfactorily, I exposed the sciatic nerve of a rabbit in the upper and posterior part of the thigh, and passed under it a tape half an inch wide. I then made a wound in the leg, and having introduced into it some of the woorara mixed with water, I tied the tape moderately tight on the fore-part of the thigh. Thus I interrupted the communication between the wounds and the other parts of the body, by means of the vessels, while that by means of the nerve still remained. After the ligature was tightened, I applied the woorara a second time, in another part of the leg. The rabbit was not at all affected, and at the end of an hour I removed the ligature. Being engaged in some other pursuit, I did not watch the animal so closely as I should otherwise have done; but twenty minutes after the ligature was removed, I found him lying on one side, motionless and insensible, evidently under the influence of the poison; but the symptoms were less violent than in most instances, and after lying in this state he recovered, and the limb became perfectly warm, and he regained the power of using it.

Exp. 28. I repeated the last experiment with this difference, that after having applied the poison, I made the ligature as tight as I could draw it. I removed the ligature at the end of an hour and twenty minutes, but the animal was not at all affected either before or after the removal of the

 ligature,

ligature, and on the following day he had recovered the use of the limb.

Exp. 29. I repeated the experiment a third time, drawing the ligature very tight. At the end of forty-five minutes the animal continued perfectly well, and the ligature was removed. I watched him for three quarters of an hour afterwards, but there were no symptoms of his being affected by the poison. On the following day the rabbit died; but this I attribute to the injury done to the limb and sciatic nerve by the ligature, as there was the appearance of inflammation in the parts in the neighbourhood of the ligature.

These three experiments were made with the greatest care. From the mode in which the poison was applied, from the quantity employed, and from my prior experience, I should have entertained not the smallest doubt of the poison taking effect in every instance in less than twenty minutes, if no ligature had been applied. In two of the three, the quantity of woorara was more than had been used in any former experiments.

I have not judged it necessary to make any more experiments, with the ligature on the limb, because the numerous experiments of the Abbé Fontana on the ticunas, coincide in their results with those which have just been detailed, and fully establish the efficacy of the ligature in preventing the action of the poison. It is not to be wondered at, that the ligature should sometimes fail in its effects, since these must evidently depend on the degree in which the circulation is obstructed, and on the length of time during which the obstruction is continued.

There can be little doubt that the woorara affects the brain, by passing into the circulation through the divided vessels. It is probable that it does not produce its effects, until it enters the substance of the brain, along with the blood, in which it is dissolved; nor will the experiments of the Abbé Fontana, in which he found the ticunas produce almost instant death when injected into the jugular vein of a rabbit, be found to militate against this conclusion, when we consider how short is the distance which, in so small an animal, the blood has to pass from the jugular vein to the carotid artery, and the great rapidity of the circulation; since in a rabbit under the influence of terror, during such an experiment, the heart cannot be supposed to act so seldom as three times in a second.

I have made no experiments to ascertain through what medium

medium other poisons when applied to wounds affect the vital organs, but from analogy we may suppose that they enter the circulation through the divided blood-vessels.

IV.

The facts already related led me to conclude that alcohol, the essential oil of almonds, the juice of aconite, the oil of tobacco, and the woorara, occasion death simply by destroying the functions of the brain. The following experiment appears fully to establish the truth of this conclusion.

Exp. 30. The temperature of the room being 58° of Fahrenheit's thermometer, I made two wounds in the side of a rabbit, and applied to them some of the woorara in the form of paste. In seven minutes after the application, the hind legs were paralysed, and in fifteen minutes respiration had ceased, and he was apparently dead. Two minutes afterwards the heart was still beating, and a tube was introduced through an opening into the trachea, by means of which the lungs were inflated. The artificial respiration was made regularly about thirty-six times in a minute.

At first, the heart contracted one hundred times in a minute.

At the end of forty minutes, the pulse had risen to one hundred and twenty in a minute.

At the end of an hour, it had risen to one hundred and forty in a minute.

At the end of an hour and twenty-three minutes, the pulse had fallen to a hundred, and the artificial respiration was discontinued.

At the commencement of the experiment, the ball of a thermometer being placed in the rectum, the quicksilver rose to one hundred degrees; at the close of the experiment it had fallen to eighty-eight and a half.

During the continuance of the artificial respiration, the blood in the femoral artery was of a florid red, and that in the femoral vein of a dark colour, as usual.

It has been observed by M. Bichat, that the immediate cause of death, when it takes place suddenly, must be the cessation of the functions of the heart, the brain, or the lungs. This observation may be extended to death under all circumstances. The stomach, the liver, the kidneys, and many other organs are necessary to life, but their constant action is not necessary; and the cessation of their functions cannot therefore be the *immediate* cause of death. As in this case the action of the heart had never ceased; as the circulation of the blood was kept up by artificial respiration for more than an hour and twenty minutes after the poison had produced its full effects; and as during this time the usual changes in the colour of the blood took place in the lungs; it is evident that the functions of the heart and lungs were unimpaired: but that those of the brain had ceased, is proved by the animal having continued in a state of complete insensibility; and by this circumstance, that animal heat, to the generation of which I have formerly shown the influence of the brain to be necessary, was not generated.

Having learned that the circulation might be kept up by artificial respiration for a considerable time after the woorara had produced its full effects, it occurred to me that in an animal under the influence of this or of any other poison that acts in a similar manner, by continuing the artificial respiration for a sufficient length of time after natural respiration had ceased, the brain might recover from the impression which the poison had produced, and the animal might be restored to life. In the last experiment, the animal gave no sign of returning sensibility; but it is to be observed, 1. That the quantity of the poison employed was very large. 2. That there was a great loss of animal heat, in consequence of the temperature of the room being much below the natural temperature of the animal, which could not therefore be considered under such favourable circumstances as to recovery, as if it had been kept in a higher temperature. 3. That the circulation was still vigorous when I left off inflating the lungs, and therefore it cannot be known what would have been the result, if the artificial respiration had been longer continued.

Exp. 30. A wound was made in the side of a rabbit, and one drop of the essential oil of almonds was inserted into it, and immediately the animal was placed in a temperature of 90°. In two minutes he was under the influence of the poison. The usual symptoms took place, and in three minutes more respiration had ceased, and he lay apparently dead, but the heart was still felt beating through the ribs. A tube was then introduced into one of the nostrils, and the lungs were inflated about thirty-five times in a minute. Six minutes after the commencement of artificial respiration, he moved his head and legs, and made an effort to breathe. He then was seized with convulsions, and again lay motionless, but continued to make occasional efforts to breathe. Sixteen minutes after its commencement, the artificial respiration was discontinued. He now breathed spontaneously seventy times in a minute, and moved his head

head and extremities. After this, he occasionally rose, and attempted to walk. In the intervals he continued in a dozing state; but from this he gradually recovered. In less than two hours he appeared perfectly well, and he continued well on the following day.

The inflating the lungs has been frequently recommended in cases of suffocation, where the cause of death is the cessation of the functions of the lungs: as far as I know, it has not been before proposed in those cases, in which the cause of death is the cessation of the functions of the brain*. It is probable that this method of treatment might be employed with advantage for the recovery of persons labouring under the effects of opium, and many other poisons.

V.

The experiments which have been detailed lead to the following conclusions.

1. Alcohol, the essential oil of almonds, the juice of aconite, the empyreumatic oil of tobacco, and the woorara, act as poisons by simply destroying the functions of the brain; universal death taking place, because respiration is under the influence of the brain, and ceases when its functions are destroyed.

2. The infusion of tobacco when injected into the intestine, and the upas antiar when applied to a wound, have the power of rendering the heart insensible to the stimulus of the blood, thus stopping the circulation; in other words, they occasion syncope.

3. There is reason to believe that the poisons, which in these experiments were applied internally, produce their effects through the medium of the nerves without being absorbed into the circulation.

4. When the woorara is applied to a wound, it produces its effects on the brain, by entering the circulation through the divided blood-vessels, and, from analogy, we may conclude that other poisons, when applied to wounds, operate in a similar manner.

5. When an animal is apparently dead from the influence of a poison, which acts by simply destroying the functions

* Since this paper was read, I have been favoured by the Right Hon. the President with the perusal of a Dissertation on the Effects of the Upas Tieutè, lately published at Paris by M. Delile, by which I find that he had employed artificial respiration for the purpose of recovering animals, which were under the influence of this poison, with success. M. Delile describes the Upas Tieutè as causing death, by occasioning repeated and long-continued contractions of the muscles of respiration, on which it acts through the medium of the spinal marrow, without destroying the functions of the brain.

 of

of the brain, it may, in some instances at least, be made to recover, if respiration is artificially produced, and continued for a certain length of time.

From analogy we might draw some conclusions respecting the mode in which some other vegetable poisons produce their effects on the animal system; but I forbear to enter into any speculative inquiries; as it is my wish, in the present communication, to record such facts only, as appear to be established by actual experiment.

Addition to the Croonian Lecture for the Year 1810.

In the experiments formerly detailed, where the circulation was maintained by means of artificial respiration after the head was removed, I observed that the blood, in its passage through the lungs, was altered from a dark to a scarlet colour, and hence I was led to conclude that the action of the air produced in it changes analogous to those which occur under ordinary circumstances. I have lately, with the assistance of my friend Mr. W. Brande, made the following experiment, which appears to confirm the truth of this conclusion.

An elastic gum bottle, having a tube and stop-cock connected with it, was filled with about a pint of oxygen gas. The spinal marrow was divided in the neck of a young rabbit, and the blood-vessels having been secured, the head was removed, and the circulation was maintained by inflating the lungs with atmospheric air for five minutes, at the end of which time the tube of the gum bottle was inserted into the trachea, and carefully secured by a ligature, so that no air might escape. By making pressure on the gum bottle, the gas was made to pass and repass into and from the lungs about thirty times in a minute. At first, the heart acted one hundred and twenty times in a minute, with regularity and strength; the thermometer, in the rectum, rose to 100°. At the end of an hour, the heart acted as frequently as before, but more feebly; the blood in the arteries was very little more florid than that in the veins; the thermometer in the rectum had fallen to 93°. The gum bottle was then removed. On causing a stream of the gas which it contained to pass through lime-water, the presence of carbonic acid was indicated by the liquid being instantly rendered turbid. The proportion of carbonic acid was not accurately determined; but it appeared to form about one-half of the quantity of gas in the bottle.

B. C. Brodie.

XXXV. *An*

XXXV. *An Account of "The Sulphur," or "Souffrière," of the Island of Montserrat. By* NICHOLAS NUGENT, *M.D. Honorary Member of the Geological Society* *.

On my voyage last year (October 1810) from Antigua to England, the packet touched at Montserrat, and my curiosity having been excited by the accounts I received of a place in the island called "The Sulphur," and which, from the descriptions of several persons, I conceived might be the crater of an inconsiderable volcano, I determined to avail myself of the stay of the packet to visit that place.

The island of Montserrat, so called by the Spaniards from a fancied resemblance to the celebrated mountain of Catalonia, is every where extremely rugged and mountainous, and the only roads, except in one direction, are narrow bridle-paths winding through the recesses of the mountains: there is hardly a possibility of using wheeled carriages, and the produce of the estates is brought to the place of shipment on the backs of mules. Accompanied by a friend, I accordingly set out on horseback from the town of Plymouth, which is situated at the foot of the mountains on the sea shore. We proceeded by a circuitous and steep route about six miles, gradually ascending the mountain, which consisted entirely of an uniform porphyritic rock, broken every where into fragments and large blocks, and which in many places was so denuded of soil as to render it a matter of astonishment how vegetation, and particularly that of the cane, should thrive so well. The far greater part of the whole island is made up of this porphyry, which by some systematics would be considered as referable to the newest floëtz trap formation, and by others would be regarded only as a variety of lava. It is a compact and highly indurated argillaceous rock of a grey colour, replete with large and perfect crystals of white felspar and black hornblende. Rocks of this description generally pass in the West Indies by the vague denomination of fire-stone, from the useful property they possess of resisting the operation of intense heat. A considerable quantity of this stone is accordingly exported from Montserrat to the other islands which do not contain it, being essential in forming the masonry around the copper boilers in sugar-works. We continued our ride a considerable distance beyond the estate called "*Galloway's*," (where we procured a guide,) till we came to the side of a very deep ravine which extends in a winding direction the

* From the Transactions of the Geological Society, vol. i.

whole way from one of the higher mountains to the sea. A rugged horse-path was traced along the brink of the ravine, which we followed amidst the most beautiful and romantic scenery. At the head of this ravine is a small amphitheatre formed by lofty surrounding mountains, and here is situated what is termed "The Sulphur." Though the scene was extremely grand and well worthy of observation, yet I confess I could not help feeling a good deal disappointed, as there was nothing like a crater to be seen, or any thing else that could lead me to suppose the place had any connexion with a volcano. On the north, east and west sides were lofty mountains wooded to the tops, composed apparently of the same kind of porphyry we had noticed all along the way. On the south, the same kind of rock of no great height, quite bare of vegetation, and in a very peculiar state of decomposition. And on the south-eastern side, our path and the outlet into the ravine. The whole area thus included, might be three or four hundred yards in length, and half that distance in breadth. The surface of the ground, not occupied by the ravine, was broken and strewed with fragments and masses of the porphyritic rock, for the most part so exceedingly decomposed as to be friable and to crumble on the smallest pressure. For some time I thought that this substance, which is perfectly white and in some instances exhibits an arrangement like crystals, was a peculiar mineral; but afterwards became convinced, that it was merely the porphyritic rock singularly altered, not by the action of the air or weather, but, as I conjecture, by a strong sulphureous or sulphuric acid vapour which is generated here, and which is probably driven more against one side by the eddy wind up the ravine, the breeze from any other quarter being shut out by the surrounding hills*.

* This peculiar decomposition of the surrounding rock has been frequently observed in similar situations, and under analogous circumstances, and has I find been accounted for by other persons in the same way: thus Dolomieu says, "La couleur blanche des pierres de l'interieur de tous les craters inflammés est due a une veritable alteration de la lave produite par les vapeurs acido-sulfureuses qui les penetrent, et qui se combinent avec l'argile qui leur sert de base, y formant l'alun que l'on retire des matières volcaniques." *Voy. aux Isles de Lipari*, p. 18.

And he afterwards adds, "Cette alteration des laves par les vapeurs acido-sulfureuses, est une espèce d'analyse que la nature fait elle même des matières volcaniques. Il y a des laves sur lesquelles les vapeurs n'ont pas encore eu assez de tems d'agir pour les dénaturer entièrement, et alors on les voit dans différens etats de decomposition que l'on reconnoit par la couleur."

Alum is doubtless formed at this place, as well as elsewhore under similar circumstances: the potash necessary for the composition of this salt, being, as well as the argil, derived from the surrounding rock. See Vauquelin's *Memoire, Journ. des Mines*, vol. x. p. 441.

Amidst

Amidst the loose stones and fragments of decomposed rock are many fissures and crevices, whence very strong sulphureous exhalations arise, and which are diffused to a considerable distance: these exhalations are so powerful as to impede respiration, and near any of the fissures are quite intolerable and suffocating. The buttons of my coat, and some silver and keys in my pockets, were instantaneously discoloured. An intense degree of heat is at the same time evolved, which, added to the apprehension of the ground crumbling and giving way, renders it difficult and painful to walk near any of these fissures. The water of a rivulet which flows down the sides of the mountain and passes over this place, is made to boil with violence, and becomes loaded with sulphureous impregnations. Other branches of the same rivulet which do not pass immediately near these fissures, remain cool and limpid: and thus you may with one hand touch one rill which is at the boiling point, and with the other hand touch another rill which is of the usual temperature of water in that climate. The exhalations of sulphur do not at all times proceed from the same fissures, but new ones appear to be daily formed, others becoming, as it were, extinct. On the margins of these fissures, and indeed almost over the whole place, are to be seen most beautiful crystalizations of sulphur, in many spots quite as fine and perfect as those from Vesuvius, or indeed as any other specimens I have ever met with. The whole mass of decomposed rock in the vicinity is, in like manner, quite penetrated by sulphur. The specimens which I collected of the crystallized sulphur, as well as of the decomposed and undecomposed porphyry, were left inadvertently on board the packet at Falmouth, which prevents my having the pleasure of exhibiting them to the Society. I did not perceive at this place any trace of pyrites or any other metallic substance, except indeed two or three small fragments of clay iron-stone at a little distance, but did not discover even this substance any where *in situ*. It is very probable that the bed of the glen or ravine might throw some light on the internal structure of the place; but it was too deep, and its banks infinitely too precipitous, for me to venture down to it. I understood that there was a similar exhalation and deposition of sulphur on the side of a mountain not more than a mile distant in a straight line; and a subterranean communication is supposed to exist between the two places.

Almost every island in the western Archipelago, particularly those which have the highest land, has in like manner its "Sulphur," or, as the French better express it,

its

its "*Souffrière.*" This is particularly the case with Nevis, St. Kitt's, Guadaloupe, Dominica, Martinico, St. Lucia, and St. Vincent's. Some islands have several such places, analogous I presume to this of Montserrat; but in others, as Guadaloupe, St. Lucia, and St. Vincent's, there are decided and well characterized volcanos, which are occasionally active, and throw out ashes, scoriæ and lava with flame. The volcano of St. Vincent's is represented by Dr. Anderson, and others who have visited it, as extremely large and magnificent, and would bear a comparison with some of those of Europe. These circumstances appear to have been entirely overlooked by geologists in their speculations concerning the origin and formation of these islands. It has indeed occurred to most persons, on surveying the regular chain of islands extending from the southern Cape of Florida to the mouths of the Orinoco, as exhibited on the map, to conclude that it originally formed part of the American continent, and that the encroachments of the sea have left only the higher parts of the land, as insular points above its present level. But this hypothesis, however simple and apparently satisfactory in itself, will be found to accord very partially with the geological structure of the different islands. Many of them are made up entirely of vast accretions of marine organized substances; and others evidently owe their origin to a volcanic agency, which is either in some degree apparent at the present time, or else may be readily traced by vestiges comparatively recent. There is every reason to believe, however, that some of the islands are really of contemporaneous formation with the adjacent parts of the continent, from which they have been disjoined by the incursions of the sea, or by convulsions of nature, and it is probably in those islands which contain primitive rocks, that we are chiefly to look for a confirmation of this supposition.

XXXVI. *Description of the Patent reflecting Semicircle, invented by Sir* Howard Douglas, *Bart. Communicated by Mr.* Cary.

The objects of the reflecting circle or semicircle are to combine the measuring principle with a circular or semicircular protractor, in such a manner that, in measuring any angle, the index or limb of the instrument shall pass over the whole of the measured angle.

By this contrivance any angle taken in the field may be at at once protracted in actual magnitude on the sketch, without the trouble of reading off. This is particularly useful in military survey, where the true situations of objects can at once be determined, and the sketch corrected at the same time that it is taken.

The description of the instrument is as follows:

To the radius or limb of a semicircular (or circular) protractor ABC (Plate VI.) the index glass DE is fixed. The horizon glass FG is fixed to a bar HI, which has a motion on the centre K. This bar slides upon a pin O attached to the limb or radius carrying the index glass, which pin is adjusted so that there shall be no apparent index error, and exactly in the same circle with the point K. The sliding bar will then move over half the real angle measured, and the principal limb protract the true angle.

Thus, the new reflecting circle or semicircle is divided into 180° or 360° instead of the double number as in the repeating circle, and the arc on which the divisions are engraven is equal to that of a sextant, whose radius is equal to the length of the sliding bar, that is, diameter of the circle.

A vernier is applied to read off with accuracy.

A 4-inch plotting or diagonal scale of a mile divided into yards, is engraven on the fixed limb of the instrument, by which addition all the cases of trigonometry can be solved by construction.

To those who have used the common sextant, the use of the reflecting semicircle will be obvious. It should be held in the right hand by the end of the fixed limb, and directed so that the left or the direct object is seen through the unsilvered part of the horizon glass. Apply the thumb of the left hand to the end of the moveable limb, and move it till the other object is seen reflected in the lower part of the horizon glass, and the angle is measured, which can be protracted at once. The errors or mistakes arising from reading off in a hurry are thus avoided. There is no necessity for keeping a field-book on these rapid occasions. The operations of protracting the points and sketching the features of ground are combined; and the transfer of the sketch to the fair sheet is much sooner accomplished.

The above instrument is made by Mr. Cary, optician, No. 182, Strand.

XXXVII. De-

XXXVII. *Description of an Ourang Outang: with Observations on its intellectual Faculties.* By M. FREDERICK CUVIER *.

THE female ourang outang which formed the subject of my observations belonged to the same species with the ourang outangs described by Tulpius, Edwards, Vosmaer, Allamand, and Buffon: it is the *Simia Satyrus* of Linnæus. When erect in its natural position its height did not exceed from 26 to 30 inches: the length of the arms from the arm-pits to the tips of the fingers was 18 inches, and the lower extremities from the top of the thigh to the tarsus were only from eight to nine inches. The upper jaw had four sharp incisors, the two in the middle were double the breadth of the lateral, two short canine teeth, similar to those of men, and three molaria on each side, with soft tubercles. The lower jaw had also four incisors, two canine teeth, and six molaria, but the incisors were of equal size. The number of the molaria was not complete. The germ of a tooth was seen on each side at the extremity of the upper and under jaws, and it is probable that others would be produced at subsequent periods. The form of these teeth was the same with that of the molaria of men and apes in general.

The hands had five fingers precisely like those of men, only the thumb extended no further than the first joint of the fore finger. The feet also had five toes, but the great toe was placed much lower than that of a man, and in its ordinary position, instead of being parallel to the other toes, it formed with them nearly a right angle. All the toes were similar in structure to the fingers and were very free in their motions, and the whole of them without exception had nails. It had almost no calves to the legs, or buttocks. The head resembled that of a man, much more than that of any animal; the forehead was high and salient, and the capacity of the cranium was great; but the neck was very short. The tongue was soft and similar to that of other apes; and although the lips were extremely thin and scarcely apparent, they possessed the power of extension in a considerable degree. The nose, which was completely flat and on a level with the face at its base, was slightly salient at its extremity, and the nostrils opened downwards. The eyes were like those of other apes, and the ears completely resembled those of men.

The vulva was very small, its labia scarcely perceptible,

* *Annales du Museum d'Hist. Nat.* tome xvi. p. 46.

and

and the clitoris entirely hid; but on each side of the vulva there was a flesh-coloured streak where the skin seemed to be softer than that of the other parts. Is this an indication of labia? Two mammæ were placed on the breast like those of females. The belly was naturally very large. This animal had neither tail nor callosities.

It was almost entirely covered with a reddish hair, more or less dark in colour, and of various thicknesses on the different parts of the body. The colour of the skin was generally that of slate; but the ears, the eye-lids, the muzzle, the inside of the hands and feet, the mammæ, and a longitudinal band on the right side of the belly, were of copper-coloured skin. The hair of the head, of the fore-arms and of the legs, was of a deeper red than that of the other parts; and on the head, the back, and the upper part of the arms it was thicker than any where else: the belly was but scantily supplied with it, and the face still less: the upper lip, the nose, the palms of the hands and the soles of the feet alone were bare. The nails were black, and the eyes brown. All the hair was woolly, that of the fore-arm grew upwards as did that of the arm downwards to the elbow. The hair of the head, which was harder in general than that of the other parts, grew forward. The skin, but chiefly that of the face, was coarse and rough, and that under the neck was so flabby that the animal seemed to have a goitre when lying on its side.

The ourang outang in question was entirely formed for living among trees. When it wanted to ascend a tree, it laid hold of the trunk or branches with its hands and feet, making use of its arms only and not of its thighs as a man would do in similar circumstances. It could pass easily from one tree to another when the branches met, so that in a thick forest it would never be necessary for it to descend to the ground, on which it moves with considerable difficulty. In general, all its motions are slow, but they seem to be painful when it is made to walk from one place to another: at first it rests its two hands on the ground, and brings its hinder parts slowly forward until its feet are between its hands or fore paws; afterwards, supporting itself on its hind legs, it advances the upper part of its body, rests again on its hands as at first, and thus moves forward. It is only when we take it by one hand that it walks on its feet, and in this case it uses its other hand to support it. I have scarcely ever seen it stand firmly on the sole of the foot; most frequently it only rested on the outer edge, apparently desirous of preserving its toes from all friction on the

the ground; nevertheless it sometimes rested on the whole of the foot, but in this case it kept the two last phalanges bent inwards, except the great toe, which was stretched out. When resting, it sate on its buttocks with its legs folded under it in the manner of the inhabitants of the East. It lay indiscriminately on its back or on its side, drawing up its legs and crossing its hands over its breast; and it was fond of being covered, for it drew over it all the clothes it could reach.

This animal used its hands in all the essential motions in which men employ theirs; and it is evident that it only requires experience to enable it to use them on almost every occasion. It generally carried its food to its mouth with its fingers; but sometimes also it seized it with its long lips; and it was by suction that it drank, like all other animals which have lips capable of being lengthened. It made use of its sense of smelling in order to decide upon the nature of the aliments which were presented to it and which it was not acquainted with, and it seemed to consult this sense with great assiduity. It ate almost indiscriminately, fruits, pulse, eggs, milk, and animal food: bread, coffee, and oranges were its most favourite aliments; and it once emptied an ink-bottle which came in its way without being incommoded. It had no particular times for going to meals, and ate at all seasons like an infant. Its sight and hearing were good. Music made no impression upon it. The mammiferæ are not formed by nature to be sensible to its charms, none of their wants seem to require it, and even with mankind it is an artificial want; on savages it has no other effect than a noise would have.

When defending itself, our ourang outang bit and struck with its hands; but it was only against children that it showed any roguery, and it was always caused by impatience rather than by anger. In general it was gentle and affectionate, and seemed to delight in society. It was fond of being caressed, gave real kisses, and seemed to experience a great deal of pleasure in sucking the fingers of those who approached it; but it did not suck its own fingers. Its cry was guttural and sharp, but it was only heard when it eagerly wanted any thing. All its signs were then very expressive: it darted its head forward in order to show its disapprobation, pouted when it was not obeyed, and when angry it cried very loudly, rolling itself on the ground. On these occasions its neck was prodigiously swelled.

By the above description it will be seen that the ourang outang in question had attained a size sufficiently great for its age, which was not more than 15 or 16 months: its teeth, limbs, and powers were almost perfect; whence it may be inferred that it had nearly acquired its full growth, and that its life does not extend beyond 25 years.

This ourang outang arrived at Paris in the beginning of March 1808. M. Decaen, an officer of the French navy and brother to the governor of the Isles of France and Bourbon, brought it from the former place and presented it to the Empress Josephine, whose taste for natural history is conspicuous. When it arrived in the Isle of France from Borneo, where it was born, it was only three months old: it remained three months in the Isle of France, was three months on its voyage to Spain where it was landed, and having been two months in its journey to Paris, it must have been ten or eleven months old when it arrived in the winter of 1808. The fatigues of a long sea voyage, but above all, the cold which the animal experienced in crossing the Pyrenees amid the snows, reduced it to the last extremity; and when it arrived at Paris several of its toes were frozen, and it laboured under a hectic fever brought on by obstructions in the spleen accompanied by a cough: it refused all sustenance and was almost motionless. In this state it came into the possession of M. Godard, a friend of M. Decaen, who succeeded in partially restoring it to health.

I visited it almost every day while it lived; and Messrs. Godard and Decaen enabled me to add to the observations I made.

The means which succeeded in restoring this animal to some degree of health, were good victuals, a proper temperature, and above all, cleanliness. At first the disease was combated with tonics: bark being inadmissible in the usual way was administered in baths and frictions; but these remedies fatigued the animal more than they relieved it, and they were given up. The constipation of the bowels was nevertheless obstinate, and it was necessary to have frequent recourse to bathing, and this treatment was pursued till the animal's death. The desire for sucking which it evinced, suggested the idea of suckling it again, but it refused the breast of a woman who volunteered on this singular service. It also refused to suckle the teats of a goat. At first it seemed fond of milk, but it soon got tired of it, and of every other aliment, which was given it in succession, with the exception of oranges, which it seemed fond of to the last. In about five months the animal died; and on opening its body,

body; most of the viscera were found to be disorganized and full of obstructions.

Such was the animal who formed the subject of my observations; and, far different from those which have hitherto been described, it had never been subjected to any particular education, and was only influenced by the circumstances in which it happened to be placed: it owed nothing to habit, nothing mechanical entered into its actions, all of them were the simple effects of volition, or at least of nature. Now that I have described the organs of this animal and their uses, I ought to make known the phænomena which its intelligence presented: but before entering upon these details I ought to say a word on the influence which the intellect is liable to from the modifications of our senses.

It appears to me, that some authors have made intelligence depend much more than was just on the greater or less perfection of the hands or fingers. Now although the hand of an ape and of an ourang outang differs very little from ours, and these animals could undoubtedly make the same use of them as we do, if they were actuated by the same ideas, yet an ourang outang would no more be a man with more perfect hands or fingers, than a man would be an ape because he was born without arms. The influence of the senses on the mind has been particularly exaggerated: some authors have thought that upon the degree of perfection of these organs the degree of the perfection of the understanding in a great measure depended. Nevertheless it must be admitted that several animals have senses completely similar to ours; and the description which we have given of the ourang outang shows that this animal, which certainly is not a man, has received senses equally numerous, and at least equally delicate with ours. Besides, if we consider the real influence exercised on the operations of the understanding by more or less delicate organs, we see that it is limited to the multiplying of ideas in a greater or less ratio, without making any change in the manner of setting these elements at work. The most humble artisan, who has exercised his sight least, and who cannot distinguish the most striking shades of colour, will not be less of the same species with the painter who has studied all the accidents of light, and who can recognise them in the slightest undulations of a drapery. Lastly, the understanding may have ideas without the aid of the senses: two thirds of the brute creation are moved by ideas which they do not owe to their sensations, but which flow immediately from

from their brain. Instinct constitutes this order of phænomena; it is composed of ideas truly innate, in which the senses have never had the smallest share. Every thing unites, therefore, in my opinion, to convince us that it is neither in the conformation of the limbs, nor in the greater or less perfection of the senses, that we must seek the principal cause of the intellectual qualities which distinguish us from the lower animals, and even the cause of those which perhaps distinguish the animals of certain classes. The operations, the phænomena of our intelligence which characterize us, ought to proceed from higher and more potent causes; faculties, even of the understanding, or of the organ in which these faculties reside, *i. e.* the brain. Consequently, we apply ourselves much more to appreciate the use which our ourang outang made of its sensations, the results which he knew how to draw from its ideas, than to analyse these sensations themselves, or to seek for the elements and the nature of these ideas.

All the faculties of animals concur to the same end,—the preservation of the species and of individuals. The individual is preserved by defending himself against dangers, and by procuring what is necessary for his existence. The preservation of the species is effected by generation. It is, therefore, to preserve his existence, and to propagate, that an animal employs all his faculties and refers all his actions; and it is with respect to defending itself against danger, and procuring necessaries for its existence, that the following observations more particularly apply. Our ourang outang was not old enough to have experienced the calls of nature in respect to generation, and to exhibit their effects. This plan simplifies the study of the intellectual faculties of brutes. Hitherto the science which has had these faculties for its object, has consisted of isolated facts, the number of which might still increase indefinitely without increasing our knowledge, if we did not endeavour to subject them to fixed and proper rules, to regard them in their true point of view, and to appreciate their real value. We know that the faculties of the understanding are not developed until the organs are formed: we are at liberty to suppose, therefore, that if our ourang outang had arrived at an adult age, she would have exhibited phænomena still more curious than those which we have to detail: but if we reflect that this animal was scarcely 16 months old when it died, we shall find plenty of subjects of astonishment in the observations which it afforded, and of which we are about to give an account.

Of the intellectual Phænomena which have for their Object to defend the Animal against Danger.

Nature has given the ourang outang but few means of defence. Next to man, it is an animal perhaps which finds in its own resources the feeblest defence against dangers: but in recompense it has a great facility in ascending trees, and thus escaping the enemies which it cannot combat. These sole considerations would be sufficient for encouraging the presumption that nature has endowed the ourang-outang with great circumspection. In fact, the prudence of this animal is conspicuous in all its actions, and chiefly in those which have for their object to save it from some dangers. Nevertheless its tranquil life, while under my inspection, and the impossibility of subjecting it to severe experiments in the weak state in which it was, prevented me from making many observations: but assisted by those which had been made by M. Decaen during the voyage from the Isle of France to Europe, my readers will obtain some idea of its intellectual faculties.

During the first week after its embarkation this ourang outang evinced great fears for its safety, and seemed greatly to exaggerate the dangers of the rolling of the vessel. It never ventured to walk, without firmly grasping in its hands the ropes or other parts of the vessel: it constantly refused to ascend the masts, however solicitous the crew were to induce it, and it was only prevailed on to do so from a sentiment, or a want, which nature seems to have carried to a high degree of perfection in animals of this kind: this sentiment was that of affection, which our animal constantly evinced, and I have no doubt that it would lead the ourang outangs to live in society and to defend themselves mutually, when certain dangers menaced them, like other animals which nature forms for herding together. However this may be, our ourang outang never had the courage to ascend the masts until M. Decaen did so himself: it followed him up for the first time; and having thus acquired some confidence in its own powers, it used frequently to repeat the experiment.

The means employed by the ourang outangs in defending themselves are in general those which are common to all timid animals,—artifice and prudence: but the former have a strength of judgement far superior to the latter, and which they employ occasionally to remove enemies from them who are stronger. This was proved to us in a very remarkable manner by the animal in question. Living in a state

state of liberty, he was accustomed in fine weather to visit a garden, where he could take exercise in the open air by ascending and sitting among the trees. One day that it was perched on a tree, a person approached it as if with an intention to catch it; but the animal instantly laid hold of the adjoining branches and shook them with all its force, as if it was his intention to frighten the person who attempted to ascend, by suggesting the risk of his falling. This experiment was repeatedly made with the same results.

In whatever way we regard the above action, it must be impossible for us to overlook the result of a combination of acute intelligence, or to deny to the animal the faculty of *generalizing*. Our ourang outang, by an experiment which the wantonness of the sailors had frequently made on it, perceived that the violent agitation of bodies, which support men or animals, makes them lose their equilibrium, and fall; and it reflected that, when placed in analogous circumstances, others would experience what it had experienced itself, and that the fear of falling would hinder them from ascending. It extended, therefore, to beings who were strangers to it, an idea which was personal to itself: and from a particular circumstance it formed a general rule.

It was frequently fatigued with the numerous visits which it received, and would hide itself under its coverlid; but it never did this except when strangers were present.

My observations on the intellectual means resorted to by ourang outangs for their defence, are confined to these facts alone; but they are sufficient, in my opinion, to prove that these animals are able to make up by the resources of intellect for their feeble corporeal organization.

On the intellectual Phænomena which have for their Object to procure for the Animal such Things as are necessary for its Subsistence.

The natural wants of the ourang outangs are so easily satisfied, that these animals must find in their organization enough of resources, not to compel them to a great exertion of their intellectual faculties in this respect. Fruits are their principal food, and, as we have already seen, their limbs are peculiarly adapted for ascending trees. It is probable, therefore, that, in their state of nature, these animals employ their intelligence much oftener to preserve themselves from harm than to procure food. But all their habits must change, the instant they are in the society or under the protection of men: their dangers must be diminished, and their wants increased. This is evinced by all the domestic

N 2 animals,

animals, and *à fortiori* by our ourang outang. In short, its intelligence was much more frequently called into action to satisfy its wants than to avert danger. I ought to place in this first division a custom of this animal, which appeared to be a phænomenon of instinct, the only one of the kind which it exhibited. While the season did not admit of its leaving the house, it practised a custom which appeared singular, and which was at first difficult to account for: this consisted in mounting upon an old desk to perform the functions of nature; but as soon as the warmth of spring admitted of its going into the garden, this extraordinary custom was accounted for: it never failed to ascend a tree when it wanted to perform these functions, and this method has even been resorted to, with success, as a remedy for its habitual constipation: when it did not ascend the tree of itself, it was placed upon it; and if its efforts produced no evacuation, it was a proof that bathing was necessary.

We have already seen that one of the principal wants of our ourang outang was to live in society, and to attach itself to persons who treated it with kindness. For M. Decaen it had a particular affection, of which it gave daily proofs. One morning it entered his apartment while he was still in bed, and threw itself upon him embracing him strongly, and applying its lips to his breast, which it sucked as it used to do his fingers. On another occasion it gave him a still stronger proof of its attachment. It was accustomed to come to him at meal times, which it knew very well, in expectation of victuals. With this view it leapt up behind his chair, and perched upon the back of it; when he gave it what he thought proper. On his arrival in Spain, M. Decaen went ashore, and another officer of the ship supplied his place at table: the ourang outang placed itself on the back of the chair as usual; but as soon as it perceived a stranger in its master's place it refused all food, threw itself on the floor, and rolled about in great distress, frequently striking its head and moaning bitterly. I have frequently seen it testify its impatience in this way: when any thing was refused it which it wanted, not being able or not daring to attack those who opposed its wishes, it would throw itself on the floor, strike its head, and thereby endeavour to excite interest or pity in a more lively manner. This method of expressing sorrow or anger is not observable in any animal, man excepted. Was this ourang outang led to act in this manner from the same motives which actuate us in similar circumstances? I am inclined to answer this ques-

tion

tion in the affirmative: for in its passion it would occasionally raise its head from the ground and suspend its cries, in order to see if it had produced any effect on the people around, and if they were disposed to yield to its entreaties: when it thought there was nothing favourable in their looks or gestures, it began crying again.

This desire for marks of kindness generally led our ourang outang to search for persons whom it knew, and to shun solitude, which seemed to displease it so much that one day it employed its intelligence in a singular way to break loose from it. It was shut into a closet adjoining the room where the people of the house usually met: several times it ascended a chair in order to open the door, which it effected, as the chair usually stood near the door, which was fastened with a latch. In order to prevent it from repeating this operation, the chair was removed some distance from the door; but scarcely was it shut when it again opened, and the ourang outang was seen descending from the chair, which it had pushed towards the door in order to enable it to reach the latch. Can we refuse to ascribe this action to the faculty of generalizing? It is certain that the animal had never been taught to make use of a chair for opening doors, and it had never even seen any person do so. All that it could learn from its own experience was, that by mounting upon a chair it could raise itself to a level with things that were higher than it; and it may have seen from the actions of others that chairs might be moved from one place to another, and that the door in question was moved by lifting the latch: but these very ideas are generalizations, and it is only by combining them with each other that the animal could have been led to the action which we have related. I do not think that any other animal ever carried the force of reasoning further. To conclude:—men were not the only beings of a different species to which the ourang outang attached itself: it conceived an affection for two cats which was sometimes attended with inconvenience: it generally kept one or other under its arm, and at other times it placed them on its head; but as in these various movements the cats were afraid of falling, they seized with their claws the skin of the ourang outang, which patiently endured the pain which it experienced. Twice or thrice indeed it attentively examined their feet, and after discovering their nails, it attempted to remove them, but with its fingers only: not being able to accomplish this object, it seemed resigned to the pain they gave it, rather than renounce the pleasure of toying with the animals. This desire of placing the cats on its head was displayed on a great many other occasions, and I never was able to divine the cause of it. If some small pieces of paper fell into its hands, it raised them to its head, and it did the same with ashes, earth, bones, &c.

It has already been mentioned that it took its food with its hands or mouth: it was not very expert in handling our knives and forks, and in this respect it resembled some savages whom we have heard of, but it made up for its awkwardness by its ingenuity: when the meat which was on its plate did not lie conveniently for its spoon, it gave the spoon to the person next it, in order that he might fill it. It drank very well out of a glass, which it could hold in its two hands. One day, after having put down the glass, it saw that it was likely to fall, and it instantly placed its hand at the side to which the glass inclined, and thereby saved it. Several persons were witnesses to these circumstances.

Almost all animals have occasion to protect themselves against the effects of cold, and it is probable that the ourang outangs are in this predicament in the rainy season. I am ignorant of the means resorted to by them in their state of nature, but our ourang outang almost continually kept itself covered. When on ship-board it laid hold of every thing that came in its way; and when a sailor had lost any of his clothes, he was sure to find them in the ourang outang's bed. The care which it took to keep itself covered furnished us with an excellent proof of its intelligence, and proved not only that it could generalize its ideas, but that it had the sentiment of future wants. Its coverlid was spread every day on a piece of grass in the garden in front of the dining room, and every day after dinner it went straight to the garden, took its coverlid upon its shoulders, and leaped upon the shoulders of a domestic that he might carry it to bed. One day that the coverlid was not in its usual place it searched until it found it, and then threw it over its shoulders as usual.

I have already remarked that this animal was by far too young to exhibit any of the phænomena connected with generation, &c. I shall here terminate my observations, although I could add a great many more facts, but they would throw no additional light on the subject of our inquiries.

What has been just stated, ought to show that it is not necessary to multiply our experiments in order to obtain general and precise ideas as to the intellectual faculties of the

the mammiferæ. If we pick out one or two species in each genus, and examine them under the point of view which I have adopted, I am convinced that we might succeed in establishing the laws to which this faculty is subject in the whole class, and in appreciating the successive degradations which it undergoes, its connexion with the senses, and the supplementary means which nature furnishes: in a word, we might lay the foundation of this interesting branch of natural history, which has been hitherto obscured by imaginary systems or obscure facts. For my part, I am happy in having had an opportunity of studying the animal which approaches most closely to man. I regard this as a point of comparison to which I shall in future refer all the other species of the mammiferæ, if circumstances admit of my continuing the inquiries, which I long ago commenced, into the intellectual characters which distinguish these species from each other.

XXXVIII. *Notes relating to Botany, collected from the Manuscripts of the late* Peter Collinson, *Esq., F.R.S. and communicated by* Aylmer Bourke Lambert, *Esq., F.R.S. and A.S., V.P.L.S.**

Being lately on a visit to John Cator, Esq., of Beckenham-place, and looking one day over his library, amongst a collection of books left him by his uncle, who married the daughter of the celebrated Peter Collinson, I discovered several which had formerly belonged to that eminent naturalist. One of them was his own copy of Miller's Gardener's and Botanist's Dictionary, the last edition published by the author, with the following note at the bottom of the title-page: "The gift of my old friend the author to P. Collinson, F.R.S." This book contains a great deal of his manuscript notes relating to the plants cultivated in those days, both in his own gardens and in those of the most celebrated of his contemporaries; with a complete catalogue of the plants he had cultivated in his garden at Mill-Hill, and a list of all those which he had himself introduced into this country from Russia, Siberia, America, and other parts of the world; also some original letters from Dillenius, Miller, Bartram, and others; and a short account of his own life, which appears not to have been known to his biographers. Mr. Cator having obligingly permitted me to

* From Transactions of the Linnean Society, vol. x. part ii. p. 270.

take a copy of the whole, I now submit to the Linnean Society those parts which I think most worthy of their notice. A. B. L.

I was born in the house against Church-alley, Clement's Lane, Lombard-street, from whence my parents removed into Grace-church-street, where I have now lived many years. [July 18th, 1764.] Gardening and gardeners have wonderfully increased in my memory. Being sent at two years old to be brought up with my relations at Peckham in Surry, from them I received the first liking to gardens and plants. Their garden was remarkable for fine cut greens, the fashion of those times, and for curious flowers. I often went with them to visit the few nursery gardens round London, to buy fruits, flowers, and clipt yews in the shapes of birds, dogs, men, ships, &c. For these Mr. Parkinson in Lambeth was very much noted; and he had besides a few myrtles, oleanders, and other evergreens. This was about the year 1712. At that time Mr. Wrench, behind the earl of Peterborough's at Parson's Green near Chelsea, famous for tulip-trees, began the collecting of evergreens, arbutuses, phillyreas, &c.; and from him came the gold and silver hedgehog-holly, being accidental varieties from the hedgehog variety of the common holly. He gave rewards to encourage people to look out for accidental varieties from the common holly: and the saw-leaved holly was observed by these means, and a variegated holly goes by his name to this nay. He and Parkinson died about the year 1724. Contemporary with them were Mr. Derby and Mr. Fairchild; they had their gardens on each side the narrow alley leading to Mr. George Whitmore's, at the further end of Hoxton. As their gardens were small, they were the only people for exotics, and had many stoves and green-houses for all sorts of aloes and succulent plants; with oranges, lemons, and other rare plants. At the other end of the town were two famous nurserymen, Furber and Gray, having large tracts of ground in that way, and vast stocks: for the taste of gardening increased annually. Doctor Compton, bishop of London, was a great lover of rare plants; as well such as came from the West Indies as from North America, and had the greatest collection then in England. After his death the see was filled by bishop Robinson, a man destitute of any such taste; who allowed his gardener to sell what he pleased, and often spoiled what he could not otherwise dispose of. Many fine trees, come to great maturity, were cut down to make room for produce for the table.

The

The abovementioned gardeners Furber and Gray availed themselves of making purchases from this noble collection, and augmented their nurseries with many fine plants not otherwise to be procured.

Brompton Park was another surprising nursery of all the varieties of evergreens, fruits, &c., with a number of others all round the town; for, as the taste increased, nursery gardens flourished.

Mr. Hunt at Putney, and Mr. Gray, are now living, aged about 70. But more modern cultivators are the celebrated James Gordon at Mile-end, whom for many years, from my extensive correspondence, I have assisted with plants and seeds, and who, with a sagacity peculiar to himself, has raised a vast variety of plants from all parts of the world; and the ingenious Mr. Lee of Hammersmith, who, had he the like assistance, would be little behind him. Mr. Miller of the Physic Garden, Chelsea, has made his great abilities well known by his works, as well as his skill in every part of gardening, and his success in raising seeds procured by a large correspondence. He has raised the reputation of the Chelsea garden so much, that it excels all the gardens in Europe for its amazing variety of plants of all orders and classes, and from all climates, as I beheld with much delight this 19th of July, 1764.

October 3d, 1759, after nine years absence from Goodwood after the death of my intimate friend the late duke of Richmond, I accompanied the present duchess there, and to my agreeable surprise found the hardy exotic trees much grown. There were two fine great magnolias about twenty feet high in the American grove that flowered annually. (My tree flowered this year, 1760, that I raised from seed about twenty years before.) Some of the larches measured near the ground seventeen inches round, the rest fourteen inches and a half. I saw a larch of the old duke's planting cut down, that in twenty-five years was above fifty feet high, and cut into planks above a foot in diameter, and above twenty feet long: but there were some larches of the same date seventy feet high. They grow wonderfully in chalky soil.

October 30th, 1762, the young lord Petre came of age. The late lord Petre, his father, died July 2d, 1742: he was my intimate friend, the ornament and delight of the age he lived in. He went from his house at Ingatestone in Essex, to his seat at Thorndon-hall in the same county, to extend a large row of elms at the end of the park behind the house. He removed in the spring of the year 1734, being the 22d of his age, twenty-four full grown elms about sixty feet high and two feet diameter. All grew finely, and now are not known from the old trees they were planted to match. In the year 1738 he planted the great avenue of elms up the park from the house to the esplanade. The trees were large, perhaps fifteen or twenty years old. On each side the esplanade, at the head or top of the park, he raised two mounts, and planted all with evergreens in April and May 1740. In the centre of each mount was a large cedar of Lebanon of twenty years growth, supported by four larches of eleven years growth. On the same area on the mount were planted four smaller cedars of Lebanon aged twenty years each, supported by four larches aged six years. On the sides Virginian red cedars of three years growth, mixed with other evergreens, which now (anno 1760) make an amazingly fine appearance.

In the years 1741 and 1742, from this very nursery, he planted out forty thousand trees of all kinds, to embellish the woods at the head of the park on each side of the avenue to the lodge, and round the esplanade. It would occupy a large work to give a particular account of his building and planting. His stoves exceed in dimensions all others in Europe. He dying, his vast collection of rare exotic plants, and his extensive nursery, were soon dispersed.

I paid to John Clarke for a thousand cedars of Lebanon, June the 8th, 1761, seventy-nine pounds six shillings, in behalf of the duke of Richmond. These thousand cedars were planted at five years old, in my sixty-seventh year, in March and April, anno 1761.

In September 1761 I was at Goodwood, and saw these cedars in a thriving state.

This day, October 20th, 1762, I paid Mr. Clarke for another large parcel of cedars for the duke of Richmond. It is very remarkable that Mr. Clarke, a butcher at Barnes, conceived an opinion that he could raise cedars of Lebanon from cones from the great tree at Hendon-place. He succeeded perfectly; and annually raised them in such quantities, that he supplied the nurserymen, as well as abundance of noblemen and gentlemen, with cedars of Lebanon: and he succeeded not only in cedars, but he had a great knack in raising the small magnolia, Warner's Cape jessamine, and other exotic seeds. He built a large stove for pine apples, &c.

Any person who has curiosity enough may go to Goodwood in Sussex, and see the date and progress of those cedars, which were at planting five years old. The duke's father was a great planter; but the young duke much exceeds

ceeds him, for he intends to clothe all the lofty naked hills above him with evergreen woods. Great portions are already planted, and he annually raises infinite numbers in his nurseries from seeds of pines, firs, cedars, and larches.

In the duke of Argyle's wood stands the largest New-England or Weymouth pine. This, and his largest cedars of Lebanon now standing, were all raised by him from seed in the year 1725 at his seat at Whitton near Hounslow.

This spring, 1762, all the duke of Argyle's rare trees and shrubs were removed to the princess of Wales's garden at Kew, which now excels all others, under the direction of lord Bute.

Mr. Vernon, Turkey merchant at Aleppo, transplanted the weeping-willow from the river Euphrates, brought it with him to England, and planted it at his seat at Twickenham-park, where I saw it growing anno 1748. This is the original of all the weeping-willows in our gardens *.

October the 18th, 1765, I went to see Mr. Rogers's vineyard, all of Burgundy grapes, and seemingly all perfectly ripe. I did not see a green half-ripe grape in all this great quantity. He does not expect to make less than fourteen hogsheads of wine. The bunches and fruit are remarkably large, and the vines very strong. He was formerly famous for ranunculuses.

October 18th, 1765, I visited Mrs. Gaskry, at Parson's Green, near Fulham. This long, hot, dry summer has had a remarkably good effect on all wall fruits. Apricots, peaches, and nectarines ripened much earlier than usual, and have been excellent: but the most remarkable was the plenty of pomegranates, near two dozen on each tree, of a remarkable size and fine ruddy complexion, of the size of middling oranges. One that was split showed the redness and ripeness within.

John Buxton, esq., of Shadwell near Thetford in Norfolk, from the acorns of 1762, sowed or planted on forty-two acres of land 120 bushels, containing as near as can be computed 1,432,320 acorns; which is nearly 34,103

* This is the first authentic account we have had of its introduction; the story of its being raised from a live twig of a fruit-basket, received from Spain by Pope, being only on newspaper authority so late as August 1801. —See Miller's Dictionary by Martyn. —— A. B. L.

Sir Thomas Vernon of London, Knight, and some time member for that city, died in 1705, leaving two sons. Henry the eldest died unmarried at Aleppo in Syria, aged 31; his monument is in St. Stephen's church, Coleman-street. Thomas Vernon, the second son, resided at Twickenham-park, Middlesex.

The above communicated to me by sir William A'Court, bart., nephew to Mr. Vernon.——A. B. L.

acorns

acorns on each acre. For this Mr. Buxton had a present of a gold medal from the Society of Arts, &c. Years or ages hence it may be worth a journey to go and observe the progress of vegetation in the dimensions and heights of this famous plantation, whose beginning is so certainly known,

By a letter (November 28th, 1762,) from Thomas Knowlton, gardener to the duke of Devonshire at his seat of Londesburgh near York, and director of his grace's new kitchen-garden, stoves, &c., at Chatsworth, I am informed that the duke of Devonshire is now sowing seventy quarters of acorns, that is, 560 bushels; an immense quantity: but this year there was the greatest crop of acorns ever remembered. Besides this vast sowing, some hundred thousands of young seedling oaks are planting out this winter: between forty and fifty men are employed about this work. In the year 1761, as many oaks were transplanted from the nursery, of two, three, and four years old.

1761. Our last winter, if it may be called so, exceeded for mildness 1759. The autumnal flowers were not gone before spring began in December with aconites, snowdrops, polyanthuses, &c. and continued without any alloy of intervening sharp frosts, all January, except two or three frosty nights and mornings: a more delightful season could not be enjoyed in southern latitudes. In January and February my garden was covered with flowers.

This summer, 1762, I was visiting Mr. Wood, of Littleton, Middlesex. He showed me a curiosity which surprised me. On a little slender twig of a peach-tree about four inches long, that projected from the wall, grew a peach, and close to it, on the other side of the twig, a nectarine. This Mr. Miller also assured me he had himself known, although not mentioned here (in his Dictionary); and another friend* assured me that he had a tree which produced the like in his garden at Salisbury: but this I saw myself, and it induces me to think that the peach is the mother of the nectarines; the latter being a modern fruit, as there is no Greek or Latin name for it.

Copied from my nephew Thomas Collinson's Journal of his Travels, 1754.—"In the reign of Queen Elizabeth, anno the first orange- and lemon-trees were introduced into England by two curious gentlemen, one of them sir

* I well knew the gentleman here alluded to, Dr. Hancock of Salisbury, who assured me of this fact; and a drawing showing both the fruits on the same branch is now in the possession of H. P. Wyndham, esq. of Salisbury. Dr. Hancock told me that he had the tree taken up to send to the earl of Harburgh, but it was killed by removing.——A. B. L.

Nicholas

Nicholas Carew, at Bedington, near Croydon, in Surrey." (The title is lately extinct, anno 1763.) These orange-trees were planted in the natural ground; but against every winter an artificial covering was raised for their protection. I have seen them some years ago in great perfection. But this apparatus going to decay, without due consideration a green-house of brick-work was built all round them, and left on the top uncovered in the summer. I visited them a year or two after, in their new habitation, and to my great concern found some dying, and all declining; for, although there were windows on the south side, they did not thrive in their confinement; but being kept damp with the rains, and wanting a free, airy, full sun all the growing months of summer, they languished, and at last all died.

A better fate has hitherto attended the other fine parcel of orange-trees, &c., brought over at the same time by sir Robert Mansell, at Margam; late lord Mansell's, now Mr. Talbot's, called Kingsey-castle, in the road from Cowbridge to Swansey, in South Wales. My nephew counted eighty trees of citrons, limes, burgamots, Seville and China orange-trees, planted in great cases all ranged in a row before the green-house. This is the finest sight of its kind in England. He had the curiosity to measure some of them. A China orange measured in the extent of its branches fourteen feet. A Seville orange was fourteen feet high, the case included, and the stem twenty-one inches round. A China orange twenty-two inches and a half in girth.

July 11th, 1777. I visited the orangery at Margam in the year 1766, in company with Mr. Lewis Thomas, of Eglews Nynngt in that neighbourhood, a very sensible and attentive man, who told me that the orange-trees, &c. in that garden were intended as a present from the king of Spain to the king of Denmark; and that the vessel in which they were shipped being taken in the Channel, the trees were made a present of to sir R. Mansell.

December 10th, 1765 A few days ago died my friend Mr. Bennet, who was very curious and industrious in procuring seeds and plants from abroad. He had a garden behind the Shadwell water-works near the spot where he lived, and built several very handsome stoves at a great expense, filling them with fine exotics of all kinds; but the erecting a fire-engine to raise the water so hurt his plants by the smoke, that he removed to a large garden of two or three acres, in the fields at the back of Whitechapel laystalls. Here he built a large house for pines and other rare exotics, which he left well stocked. In this garden he raised water

melons

melons to a great size and perfection; I have told above forty lying ripe on the ground. They were raised in frames, and transplanted out under bell-glasses. A basket of these melons was sent to the king. Mr. Bennet had besides a great collection of hardy-ground plants. His garden and all his plants were sold by auction April 14, 1766.

The seeds of the rhubarb with broad curled leaves were first raised by me. They were sent by Dr. Amman, professor of botany at Petersburg, whose father-in-law was Russian governor of the province near which the rhubarb grows. The seed of that with long narrow curled leaves was sent by the Jesuits in China to my friend Dr. Tanches, at Petersburg, by the Russian caravan, and he sent it to me.

Lord Rochefort, our ambassador in Spain, in a letter dated Madrid, November 1765, says, that in the parts where he had been, there are very few forest-trees worth notice; but the ilexes about the Escurial are fine. One sort produces acorns of a monstrous size, which they eat in Spain at their best tables, and they are as sweet as chesnuts.

May 17th, 1761. I was invited by Mr. Sharp, at South Lodge, on Enfield Chase, to dine, and see the Virginia dogwood (*Cornus florida*). The calyx of the flowers is as large as those figured by Catesby, and (what is remarkable) this is the only tree that bears these flowers amongst many hundreds that I have seen: it began to bear them in May, 1759.

Anno 1747. Raised a new species of what appears to be a three-thorned acacia, from seeds from Persia, that came with Azad or Persian hornbeam, given me by Mr. Baker: it thrives well in my garden. I gave seed to Mr. Gordon, and he also raised it.

The eastern hornbeam (Miller's Dictionary, edition 8th,) was raised from seed given to me, which came from Persia by the name of *Azad*. I gave it to Mr. Gordon, gardener at Mile-End, who was so fortunate as to have it come up anno 1747, and from him my garden and other gardens have been supplied. There is a large tree in my field at Hendon, Middlesex.

Mr. Miller is greatly mistaken in saying the Arundo No. 2, or *Donax*, dies down every year. In my garden the stalks have continued for some years making annually young green shoots from every joint, and bear a handsome tassel of flowers. The first time I ever saw it in flower was September 15th, 1762. This very long hot dry season has made many exotics flower.

Donax

Donax seu Arundo flowered this year also (1762) at Mr. Gordon's at Mile-End.

October the 22d, 1746, I received the first double Spanish broom that was in England, sent me by my friend Mr. Brewer at Nuremberg: it cost there a golden ducat; and, being planted in a pot nicely wickered all over, came from thence down the river Elbe to Hamburgh, from whence it was brought by the first ship to London. I inarched it on the single-flowered broom, and gave it to Gray and Gordon, gardeners, and from them all have been supplied.

Anno 1756. Some roots of Siberian martagon sent me by Mr. Demidoff, proprietor of the Siberian iron mines, flowered for the first time, May 24, 1756. The flower is but little reflexed, and is, I think, the nearest to black of any flower that I know.

In the year 1727, my intimate friend sir Charles Wager, first lord of the admiralty, brought plants from Gibraltar-Hill, of the *Linaria procumbens Hispanica flore flavescente pulchrè striato, labiis nigro-purpureis*, which I have yet in my garden, anno 1761; and at the same time he brought the broad-leaved *Teucrium*, and a species of periwinkle, neither of which were in our gardens before; and some roots of what is called *Hyacinths of Peru*.

In the year 1756, the famous tulip-tree in Lord Peterborough's garden at Parson's Green, near Fulham, died. It was about seventy feet high, the tallest tree in the ground, and perhaps a hundred years old, being the first tree of the kind that was raised in England. It had for many years the visitation of the curious to see its flowers, and admire its beauty, for it was as straight as an arrow, and died of age by a gentle decay. But it was remarkable, that the same year that this died, a tulip-tree which I had given to sir Charles Wager flowered for the first time in his garden, which was opposite lord Peterborough's. This tulip-tree I raised from seed, and it was thirty years old when it flowered.

April 8th, 1749. I removed from my house at Peckham, Surrey, and was for two years in transplanting my garden to my house at Mill-Hill, called Ridgeway-House, in the parish of Hendon, Middlesex.

Anno 1751. I raised the China or paper mulberry from seed gived me by Dr. Mortimer.

XXXIX. *Me-*

XXXIX. *Memorandum on the Subject of the Earl of* Elgin's *Pursuits in Greece**.

MEMORANDUM, &c.

In the year 1799, when lord Elgin was appointed his majesty's ambassador extraordinary to the Ottoman Porte, he happened to be in habits of frequent intercourse with Mr. Harrison, an architect of great eminence in the west of England, who had there given various very splendid proofs of his professional talents, especially in a public building of Grecian architecture at Chester. Mr. Harrison had besides studied many years, and to great purpose, at Rome. Lord Elgin consulted him, therefore, on the benefits that might possibly be derived to the arts in this country, in case an opportunity could be found for studying minutely the architecture and sculpture of ancient Greece; and his opinion very decidedly was, that although we might possess exact measurements of the buildings at Athens, yet a young artist could never form to himself an adequate conception of their minute details, combinations, and general effect, without having before him some such sensible representation of them as might be conveyed by *casts*. This advice, which laid the groundwork of lord Elgin's pursuits in Greece, led to the further consideration, that, since any knowledge which was possessed of these buildings had been obtained under the peculiar disadvantages which the prejudices and jealousies of the Turks had ever thrown in the way of such attempts, any favourable circumstances which lord Elgin's embassy might offer should be improved fundamentally; and not only modellers, but architects and draftsmen, might be employed, to rescue from oblivion, with the most accurate detail, whatever specimens of architecture and sculpture in Greece had still escaped the ravages of time, and the barbarism of conquerors.

On this suggestion, lord Elgin proposed to his majesty's government, that they should send out English artists of known eminence, capable of collecting this information in the most perfect manner; but the prospect appeared of too doubtful an issue for ministers to engage in the expense attending it. Lord Elgin then endeavoured to engage some of these artists at his own charge; but the value of their time was far beyond his means. When, however, he reached Sicily, on the recommendation of sir William Hamilton, he was so fortunate as to prevail on don Tita Lusieri, one of

* London, printed for William Miller, Albemarle Street.

of the best general painters in Europe, of great knowledge in the arts, infinite taste, and most scrupulously exact in copying any subject he is to represent, to undertake the execution of this plan; and Mr. Hamilton, who was then accompanying lord Elgin to Constantinople, immediately went with M. Lusieri to Rome; where, in consequence of the late revolutions in Italy, they were enabled to engage two of the most eminent *formatori* to make the *madreformi* for the casts: signior Balestra, the first architect there, along with Ittar, a young man of great talent, to undertake the architectural part of the plan; and one Theodore, a Calmouk, who had distinguished himself during several years at Rome, in the capacity of figure-painter.

After much difficulty, lord Elgin obtained permission from the Turkish government to establish these six artists at Athens; where they prosecuted the business of their several departments during three years, acting on one general system, with the advantage of mutual control, and under the general superintendance of M. Lusieri. They at length completed lord Elgin's plan in all its parts.

Accordingly, every monument, of which there are any remains in Athens, has been thus most carefully and minutely measured; and, from the rough draughts of the architects, (all of which are preserved,) finished drawings have been made of the plans, elevations, and details of the most remarkable objects; in which the Calmouk has restored and inserted all the sculpture, with exquisite taste and ability. He has besides drawn, with astonishing accuracy, all the bas-reliefs on the several temples, in the precise state of decay and mutilation in which they at present exist.

Most of the *bas*-reliefs, and nearly all the characteristic features of architecture, in the various monuments at Athens, have been moulded, and the moulds of them have been brought to London.

Besides the architecture and sculpture at Athens, all remains of them which could be traced through several other parts of Greece, have been measured and delineated, with the most scrupulous exactness, by the second architect, Ittar.

And picturesque views of Athens, of Constantinople, of various parts of Greece, and of the Islands of the Archipelago, have been executed by don Tita Lusieri.

In the prosecution of this undertaking, the artists had the mortification of witnessing the very wilful devastation, to which all the sculpture, and even the architecture, were daily exposed, on the part of the Turks and travellers. The

Ionic Temple, on the Ilyssus, which, in Stuart's time, (about the year 1759,) was in tolerable preservation, had so completely disappeared, that its foundation can no longer be ascertained. Another temple, near Olympia, had shared a similar fate, within the recollection of man. The Temple of Minerva had been converted into a powder magazine, and been completely destroyed, from a shell falling upon it, during the bombardment of Athens by the Venetians towards the end of the seventeenth century; and even this accident had not deterred the Turks from applying the beautiful Temple of Neptune and Erechtheus to the same use, whereby it is constantly exposed to a similar fate. Many of the statues on the *posticum* of the Temple of Minerva, (Parthenon,) which had been thrown down by the explosion, had been absolutely pounded for mortar, because they furnished the whitest marble within reach; and the parts of the modern fortification, and the miserable houses where this mortar was so applied, were discovered. Besides, it is well known that the Turks will frequently climb up the ruined walls, and amuse themselves in defacing any sculpture they can reach; or in breaking columns, statues, or other remains of antiquity, in the fond expectation of finding within them some hidden treasures.

Under these circumstances, lord Elgin felt himself impelled, by a stronger motive than personal gratification, to endeavour to preserve any specimens of sculpture, he could, without injury, rescue from such impending ruin. He had, besides, another inducement, and an example before him, in the conduct of the last French embassy sent to Turkey before the revolution. French artists did then remove several of the sculptured ornaments from several edifices in the Acropolis, and particularly from the Parthenon. In lowering one of the metopes, the tackle failed, and it was dashed to pieces; but other objects from the same temple were conveyed to France, where they are held in the very highest estimation, and some of them occupy conspicuous places in the gallery of the Louvre*. And the same agents were remaining at Athens during lord Elgin's embassy, waiting only the return of French influence at the Porte to renew their operations. Actuated by these inducements, lord Elgin made use of all his means, and ultimately with

* *Vide* Dictionnaire des Beaux Arts, par A. L. Millin, 1806, article *Parthenon*; and the Memoir, on the subject of a fragment of the frize of that temple, brought by M. De Choiseul Gouffier from Athens, and constituted national property during the French revolution. The Memoir is published in M. Millin's *Monumens Antiques inédits*.

such success, that he has brought to England from the ruined temples at Athens, from the modern walls and fortifications, in which many fragments had been used as so many blocks of stone, and from excavations made on purpose, a greater quantity of original Athenian sculpture, in statues, alti and bassi relievi, capitals, cornices, frizes, and columns, than exists in any other part of Europe.

Lord Elgin is in possession of several of the original metopes from the Temple of Minerva. These represent the battles between the Centaurs and Lapithæ, at the nuptials of Pirithous. Each metope contains two figures, grouped in various attitudes; sometimes the Lapithæ victorious, sometimes the Centaurs. The figure of one of the Lapithæ, who is lying dead and trampled on by a Centaur, is one of the finest productions of the art; as well as the group adjoining to it, of Hippodamia, the bride, carried off by the Centaur Eurytion; the furious style of whose galloping, in order to secure his prize, and his shrinking from the spear that has been hurled after him, are expressed with prodigious animation. They are all in such high relief, as to seem groupes of statues; and they are in general finished with as much attention behind as before. They were originally continued round the entablature of the Parthenon, and formed ninety-two groupes. The zeal of the early Christians, the barbarism of the Turks, and the explosions which took place when the temple was used as a gun-powder magazine, have demolished a very large portion of them; so that, with the exception of those preserved by lord Elgin, it is in general difficult to trace even the outline of the original subject.

The frize, which was carried along the top of the walls of the cell, offered a continuation of sculptures in low relief, and of the most interesting kind. This frize, being unbroken by triglyphs, had presented much more unity of subject than the detached and insulated groupes on the metopes of the peristyle. It represented the whole of the solemn procession to the Temple of Minerva during the Panathenaic festival: many of the figures are on horseback; others are about to mount: some are in chariots; others on foot: oxen, and other victims, are leading to sacrifice: the nymphs called Canephoræ, Skiophoræ, &c. are carrying the sacred offerings in baskets and vases; priests, magistrates, warriors, &c. &c. forming altogether a series of most interesting figures, in great variety of costume, armour, and attitude. Some antiquaries, who have examined this frize with minute attention, seem to think it contained portraits of many of the leading characters at Athens, during the Peloponnesian war, particularly of Pericles, Phidias, Socrates, Alcibiades, &c. The whole frize, which originally was six hundred feet in length, is, like the temple itself, of Pentelic marble, from the quarries in the neighbourhood of Athens.

The tympanum over each of the porticoes of the Parthenon was adorned with statues. That over the grand entrance of the temple from the west contained the mythological history of Minerva's birth from the brain of Jove. In the centre of the groupe was seated Jupiter, in all the majesty of the sovereign of the gods. On his left were the principal divinities of Olympus; among whom Vulcan came prominently forward, with the axe in his hand which had cleft a passage for the goddess. On the right was Victory, in loose floating robes, holding the horses of the chariot which introduced the new divinity to Olympus. One of the bombs fired by Morosini, the Venetian, from the opposite hill of the Museum, injured many of the figures in this tympanum; and the attempt of general Kœnigsmark, in 1687, to take down the figure of Minerva, ruined the whole. By purchasing the house of one of the Turkish janizaries, built immediately under and against the columns of the portico, and by demolishing it in order to excavate, lord Elgin has had the satisfaction of recovering the greatest part of the statue of Victory, in a drapery which discovers the fine form of the figure with exquisite delicacy and taste. Lord Elgin also found there the torsi of Jupiter and Vulcan, the breast of the Minerva, together with other fragments.

On the opposite tympanum had been represented the contest between Minerva and Neptune for the honour of giving a name to the city. One or two of the figures remained on this tympanum, and others were on the top of the wall, thrown back by the explosion which destroyed the temple, but the far greater part had fallen; and a house being built immediately below the space they had occupied, lord Elgin, encouraged by the success of his former excavations, obtained leave, after much difficulty, to pull down this house also, and continue his researches. But no fragments were here discovered: and the Turk, who had been induced, though most reluctantly, to give up his house to be demolished, then exultingly pointed out the places in the modern fortification, and in his own buildings, where the cement employed had been formed from the very statues which lord Elgin had been in hopes of finding. And it was afterwards ascertained, on incontrovertible evidence, that these statues had

had been reduced to powder, and so used. Then, and then only, did lord Elgin employ means to rescue what still remained from a similar fate. Among these objects is a horse's head, which far surpasses any thing of the kind, both in the truth and spirit of the execution. The nostrils are distended, the ears erect; the veins swollen, one might almost say throbbing: his mouth is open, and he seems to neigh with the conscious pride of belonging to the Ruler of the Waves. Besides this inimitable head, lord Elgin has procured, from the same pediment, two colossal groupes, each consisting of two female figures. They are formed of single massive blocks of Pentelic marble: their attitudes are most graceful; and the lightness and elegance of the drapery exquisite. From the same pediment has also been procured a male statue, in a reclining posture, supposed to represent Neptune; and, above all, the figure denominated the Theseus, which is universally admitted to be superior to any piece of statuary ever brought into England. Each of these statues is worked with such care, and the finishing even carried so far, that every part, and the very plinth itself in which they rest, are equally polished on every side.

From the opisthodomos of the Parthenon, lord Elgin also procured some valuable inscriptions, written in the manner called Kionedon or columnar, next in antiquity to the Boustrophedon. The greastest care is taken to preserve an equal number of letters in each line; even monosyllables are separated occasionally into two parts, if the line has had its complement, and the next line then begins with the end of the broken word. The letters range perpendicularly, as well as horizontally, so as to render it almost impossible to make any interpolation or erasure of the original text. The subjects of these monuments are public decrees of the people; accounts of the riches contained in the treasury, and delivered by the administrators to their successors in office; enumerations of the statues; the silver, gold, and precious stones, deposited in the temples; estimates for the public works, &c.

The Parthenon itself, independently of its decorative sculpture, is so chaste and perfect a model of Doric architecture, that lord Elgin conceived it to be of the highest importance to the arts, to secure original specimens of each member of that edifice. These consist of a capital; assizes of the columns themselves, to show the exact form of the curve used in channelling; a triglyph, and motules from the cornice, and even some of the marble tiles with which the ambulatory was roofed: so that, not only the sculptor may be gratified by studying every specimen of his art, from the colossal statue to the basso-relievo*, executed in the golden age of Pericles, by Phidias himself, or under his immediate direction; but the practical architect may examine into every detail of the building, even to the mode of uniting the tambours of the columns, without the aid of mortar, so as to give to the shafts the appearance of single blocks.

Equal attention has been paid to the Temple of Theseus; but as the walls, and columns, and sculpture of this monument are in their original position, no part of the sculpture has been displaced, nor the minutest fragment of any kind separated from the building. The metopes in mezzo-relievo, containing a mixture of the Labours of Hercules and Theseus, have been modelled and drawn, as well as the frize representing the battle between the Centaurs and Lapithæ, some incidents of the battle of Marathon, and some mythological subjects. The temple itself is very inferior in size and decorative sculpture to the Parthenon; having been built by Cimon, the son of Miltiades, before Pericles had given to his countrymen a taste for such magnificence and expense, as he displayed on the edifices of the Acropolis.

The original approach to the Acropolis, from the plain of Athens, was by a long flight of steps, commencing near the foot of the Areopagus, and terminating at the Propylæa. The Propylæa was a hexastyle colonnade, with two wings, and surmounted by a pediment. Whether the metopes and tympanum were adorned with sculpture, cannot now be ascertained; as the pediment and entablature have been destroyed, and the intercolumniations built up with rubbish, in order to raise a battery of cannon on the top. Although the plan of this edifice contain some deviations from the pure taste that reigns in the other structures of the Acropolis, yet each member is so perfect in the details of its execution, that lord Elgin was at great pains to obtain a Doric and an Ionic capital from its ruins. On the right hand of the Propylæa, was a temple dedicated to Victory without wings; an epithet to which many explanations have been given. This temple was built from the sale of the spoils won in the glorious struggles for freedom at Marathon, Salamis, and Platæa. On its frize were sculptured many incidents of these memorable battles; in a style that has been thought by no means inferior to the metopes of the Parthenon. The only fragments of it that had escaped the

the ravages of barbarians, were built into the wall of a gun-powder magazine near it, and the finest block was inserted upside downwards. It required the whole of lord Elgin's influence at the Porte, very great sacrifices, and much perseverance, to remove them; but he at length succeeded. They represent the Athenians in close combat with the Persians, and the sculptor has marked the different dresses and armour of the various forces serving under the great king. The long garments and zones of the Persians had induced former travellers, from the hasty and imperfect view they had of them, to suppose the subject was the battle between Theseus and the Amazons, who invaded Attica, under the command of Antiope; but the Persian tiaras, the Phrygian bonnets, and many other particulars, prove them to be mistaken. The spirit with which the groupes of combatants are pourtrayed, is wonderful;—one remarks, in particular, the contest of four warriors to rescue the dead body of one of their comrades, which is expressed with uncommon animation. These bas-reliefs, and some of the most valuable sculpture, especially the representation of a marriage, taken from the parapet of the modern fortification, were embarked in the Mentor, a vessel belonging to lord Elgin, which was unfortunately wrecked off the island of Cerigo: but Mr. Hamilton, who was at the time on board, and most providentially saved, immediately directed his whole energies to discover some means of rescuing so valuable a cargo; and, in the course of several months devoted to that endeavour, he succeeded in procuring some very expert divers from the islands of Syme and Calymno, near Rhodes; who were able, with immense labour and perseverance, to extricate a few of the cases from the hold of the ship, while she lay in twelve fathoms water. It was impossible to recover the remainder, before the storms of two winters had effectually destroyed the timbers of the vessel.

[To be continued.]

XL. *Report of the National Vaccine Establishment.*

[Continued from p. 158.]

I. *Case of the Rev. Joshua Rowley.*

The rev. Joshua Rowley, brother to sir W. Rowley, when an infant, was inoculated by the late Mr. Adair, 1770; the scar left by the inoculation is perfectly visible; his mother, the

the dowager lady Rowley, remembers perfectly his having a tolerable sprinkling of small-pox, and says, he was afterwards repeatedly exposed to variolous infection in the nursery, when his three younger brothers were successively inoculated, all of whom had some degree of eruption; and since that time, frequently, in performing the clerical duties of his profession.

On Wednesday the 5th of June, he felt much indisposed, complained of pain in his head and back, attended with considerable restlessness and prostration of strength: on Friday the 7th, an eruption appeared chiefly on his face and breast; he was attended by Mr. Woodman, of Bognor, only, till the Monday following, when Mr. Guy, surgeon, of Chichester was first consulted. On examining the eruption, Mr. Guy was immediately struck with its resemblance to the small-pox; and on gently hinting his suspicion to Mr. Rowley, received the information above related. On the following day the progress of the eruption towards maturation, and the swelling of the face, which is characteristic of the small-pox, left no doubt of the nature of the malady. The eruption was perfectly distinct; it was very full all over the trunk and body, and there were about two hundred pustules on the face. Mr. Guy is of opinion, that this was a clearly marked case of small-pox.

The history of the previous variolous inoculation in 1770 was procured from the dowager lady Rowley by Mr. Dundas, sergeant surgeon to his majesty; and the account of the present case was transmitted to the director of vaccination of this establishment, on the application of the Board, by Mr. Guy, an eminent surgeon of Chichester.

II. *Case of Miss Sarah Booth, of Covent Garden Theatre.*

Dr. Bree was called to visit miss S. Booth, on Monday, June 25th. She was said to be ill with the small-pox; and the following circumstances were reported by the mother and sisters.

Miss Booth is 18 years of age; she had been inoculated for the small-pox at five years of age, and had been affected with the usual degree of fever; the arm had been violently inflamed, and an eruption of small-pox pustules had appeared round the inoculated part, from which matter had been taken by Mr. Kennedy, the surgeon who attended her. Mr. Kennedy expressed himself satisfied that miss S. Booth had passed regularly through the disease.

The usual scar of small-pox inoculation is perfectly evident on the arm.

On

On Thursday, June 20th, miss Booth was seized with fever, distinguished by vomiting, violent head-ache, pains in the back and loins.

The symptoms continued till Saturday, June 22d, in the evening of which day some pustules came out on the forehead and scalp.

Sunday, June 23d, a more complete eruption appeared on the face and neck, and she was relieved from the violence of the fever. The vomiting however continued, the throat became very sore, and a salivation began.

Monday, June 24. The eruption extended itself on the body, the fever was still more abated, but the salivation, soreness of the throat, and vomiting, were urgent symptoms.

Tuesday, June 25th, the fourth day of the eruption. The salivation and retching continued, with soreness of throat.

Wednesday, June 26th, fifth day of the eruption. Pustules were noticed on the lower extremities, those on the face advance, and the eyes are swelled; the number of the pustules on the head and face is about two dozen.

Thursday, June 27th, sixth day of the eruption. The pustules on the face begin to turn. She still suffers from sore throat and salivation. This evening, contrary to advice, she went to her business at the theatre.

Friday, June 28th, seventh day of the eruption. The pustules on the face are turned, those on the lower extremities are few in number, but well filled, and not yet changed.

Saturday, June 29th, eighth day of the eruption. She only complains of sickness. After this day the pustules turned and dried on the lower extremities, and no complaint remained.

This case appeared to have been a very mild case of distinct small-pox.

ROBERT BREE.

This case was visited by the greater number of members of the Board, and also by the director, and was attended by Mr. Hewson, of James-street, Covent Garden, who entertains no doubt of this having been a case of distinct small-pox.

III. *Case of John Godwin.*

Mrs. Godwin, No. 6, Stratton-street, Piccadilly, states, that she was brought to bed of this son in October, 1800; that six weeks after he was born, the small-pox prevailed very much in her neighbourhood, and one child died of it in in the house in which she lived. About this time her son was attacked with very violent fever, succeeded by a copious eruption all over the face and body, which was declared by Mr. Smith, an apothecary who attended him, to be the small-pox, and which was ten or twelve days before it completely scabbed and dried off.

Some time after this, a brother of her husband, a medical man, who had not seen the child during its illness, inoculated him for the small-pox, in order to insure his complete security; a small pimple on the part was only formed, which soon disappeared, and no fever or eruption ensued. About six weeks ago, this boy, now eleven years old, was attacked with fever, followed with an eruption, which broke out on the face, body, and limbs, exhibiting the ordinary appearance of small-pox, and which turned on the eighth day.

Mr. Kerrison, of New Burlington-street, who attended this boy, states, that the eruption exhibited the exact appearance, and passed through all the stages of distinct small-pox. He also from this boy inoculated a child who had fever at the usual time, followed by a slight variolous eruption.

The history of the former disease was procured from Mrs. Godwin, and the history of the second attack of small-pox from Mr. Kerrison, by Mr. Moore, director of vaccination at this establishment.

IV. *Case of Peter Sylvester, No. 10, Cross Street, Carnaby Market.*

This boy's parents are both dead. He was born on June 7th, 1798, and on the 21st of February following was inoculated for the small pox by Mr. Ring, of New-street, surgeon. Mr. Ring showed the director of vaccination at this establishment, his account book of that period, in which there is a charge regularly entered for inoculating this boy for the small-pox.

The cicatrix on his arm is still conspicuous, and six or seven small-pox pits, occasioned by the former eruption, have marked his face,

On the 24th of June last, this boy was taken ill with fever; on the 27th an eruption on the skin took place. Mr. Moore, the director, saw him on the 30th: the spots on the skin were very numerous, but distinct, and the skin round their bases was inflamed; many had formed within the mouth and throat.

July 1st, the eruption has now assumed the appearance of of genuine small-pox, the pustules are augmenting, and the face is beginning to swell. 2d. The pustules are larger, and the face much swelled. 3d. The pustules on the face are at the height, and the eyes are nearly closed. 4th. The pustules on the face have all begun to turn; all fever is gone.

This case is drawn up from the notes of Mr. Moore. The case was visited by several members of the Board, and by many other medical gentlemen of the highest respectability.

From the period at which the violent opposition to small-pox inoculation subsided, till the establishment of vaccination, no reasonable parent has refused to allow his children the benefit of inoculation, although it has been generally acknowledged that the inoculation of the small-pox sometimes produces a fatal disease; and if at that time the instances in which the natural small-pox had occurred after inoculation, had been communicated to the public, every intelligent man would undoubtedly have still continued the same course, from a desire of affording his children the best chance of safety, although his confidence in the absolute security from natural small-pox must have been in some degree abated.

In the same manner, no effect injurious to vaccination ought to result from the knowledge of the above failures. Parents always had been apprised that there were occasional failures of vaccination, but they were always aware that none of their children would die of vaccine inoculation; and that when it failed, the succeeding small-pox was almost always much mitigated and disarmed of half its terrors. It was natural therefore, that they should choose vaccination as the less dangerous disorder, and the same reason still exists for their perseverance in that choice. If there be constitutions, which are twice susceptible of small-pox, a disorder which produces a violent action upon the human frame, and often destroys life, it is natural to expect that vaccination should not in every instance prevent the small-pox, and that the anomaly which occurs in the one disease should likewise take place in the other. It is ever to be kept in view, that the number of deaths from inoculated small-pox, exceeds the number of failures of vaccination. It appears from the present state of our information, that one person in three hundred dies from the inoculated small-pox, and that there is perhaps one failure in a thousand after vaccination. An individual, who, under such circumstances, should prefer the inoculation of his children for the small-pox, to submitting them to vaccination, would be be guilty of an improvidence similar to that of a parent who should choose for his son a military service, in which there was one chance in three hundred of being killed, in preference to a station, where there was only one chance in a thousand of being slightly wounded.

The Board are of opinion, that vaccination still rests upon the basis on which it was placed by the Reports of the several Colleges of Physicians and Surgeons of the United Kingdom, which were laid before Parliament in the year 1807. That the general advantages of vaccination are not discredited by the instances of failure which have recently occurred, the proportion of failures still remaining less in number than the deaths which take plaee from the inoculated small-pox. They are led by their information to believe, that since this practice has been fully established, no death has in any instance occurred from small-pox after vaccination.—That in most of the cases in which vaccination has failed, the small-pox has been a disease remarkably mild, and of unusually short duration; and they are further of opinion, that the severity of the symptoms with which Mr. Grosvenor was affected, forms an exception to a general rule.

That absolute security from the natural small-pox is not even to be attained by small-pox inoculation, is sufficiently evident from the annexed cases; and the Board are enabled to state, that they have been made acquainted with instances of individuals who have twice undergone the natural small-pox.

Under all these circumstances, the Board feel justified in still recommending and promoting vaccination, and in declaring their unabated confidence in this practice. Since in some peculiar frames of constitution the repetition of small-pox is neither prevented by inoculation nor casual infection, the Board are of opinion, that in such peculiar constitutions the occurrence of small-pox after vaccination may be reasonably expected, and perhaps in a greater proportion; but with this admission, they do not hesitate to maintain, that the proportionate advantages of vaccination to individuals and the public, are infinitely greater than those of small-pox inoculation.

They are anxious, that the existence of certain peculiarities of the human frame, by which some individuals are rendered by nature more or less susceptible of eruptive fevers, and of the recurrence of such disorders, should be publicly known; for they feel confident, that a due consideration of these circumstances, and a just feeling of the welfare of the community,

community, will induce the public to prefer a mild disease like vaccination, which where it fails of superseding the small-pox, yet mitigates its violence, and prevents its fatal consequences, to one whose effects are frequently violent; to one which often occasions deformity and blindness; and, when it is contracted by casual infection, has been supposed to destroy one in six in all that it attacks. And it must not be forgotten, that in a public view this constitutes the great objection to inoculation of the small-pox, that by its contagion it disseminates death throughout the empire, whilst vaccination, whatever be the comparative security which it affords to individuals, occasions no subsequent disorder, and has never, by the most violent of its opposers, been charged with producing an epidemical sickness.

By Order of the Board,

July 18, 1811. JAS. HERVEY, Register.

XLI. *Observations on the Article* "Fermentation," *contained in M.* CHAPTAL's *Nouveau Cours complet d'Agriculture. By M.* DUPORTAL, *M. D. Professor of Physic and Chemistry in the Academy of Montpellier, &c.**

THE equilibrium in the composition of vegetable substances is speedily destroyed when their life escapes from them. These substances very soon undergo a change in their appearance, the principles which compose them reacting upon each other; they are arranged in a new order, and in new proportions, whence result products very different from those substances which gave rise to their production.

These products vary according to the nature of the substances, and according to the various circumstances which accompany their change. Thus, vegetable substances which are decomposed in some peculiar circumstances, undergo a spontaneous alteration which is called fermentation, of which the product is bread, an intoxicating liquor, or vinegar, according to the matter subjected to fermentation; while recent herbaceous plants, which putrefy, give rise to the formation of mould.

These are the facts pointed out by M. Chaptal in the work I am now to analyse. Examining first the fermentation of vegetables of a fleshy and juicy texture, when collected into a large heap, he details the conditions, the phænomena, and the result of the process. He afterwards considers the operation in each of the separate parts of

* *Annales de Chimie*, 1810.

which

which vegetables consist; he confines himself to the three kinds of fermentation, called the *pannary*, the *vinous*, and the *acetous*. We shall follow the author in his development, and make some observations on the most interesting of his facts.

1st. *Of the Pannary Fermentation.*

The making of bread, the food of almost all Europeans, is a domestic chemical operation, since in it those substances which are the most essential to the sustenance of man undergo a change in their nature. These substances are found united in the meal of the farinaceous seeds, especially in those of wheat, which furnishes the best bread. M. Chaptal has found this latter farina to consist of starch, gluten, mucilage, and sugar. We may add to them the ferment, the vegetable albumen, calcareous phosphate, &c. which must be reckoned in the number of materials which compose it. What share has each of these principles in carrying on the pannary fermentation? It is generally believed that the farina being reduced into a paste, the mucous saccharine principle undergoes the vinous fermentation, that the starch has a tendency to become acid, and that the gluten and albumen enter into putrefaction.

I cannot entirely accord with this doctrine. It appears to me to be more correct, to suppose that the ferment, after having converted the sugar of the farina into carbonic acid gas, and into alcohol, changes this into acetic acid; that at the same time the gluten and the albumen are in part decomposed, acetic acid is again produced, some ammonia, and more carbonic acid gas, &c.; and that, the starch uniting with the undecomposed gluten, there results a compound, the further alteration of which is prevented by the action of fire, which combines still more intimately these principles.

This theory of the pannary fermentation seems to me to be supported by the following facts.

1st. Those farinæ which are deprived of the fermenting principle, or those which scarcely contain any of it, always afford heavy bread, although the muco-saccharine principle forms a part of them; for this substance not being a fermentable principle, it cannot ferment of itself, although it does so by means of a ferment. Thus, it is customary to add to the dough a leaven, taken from bread already fermented, or the yeast of beer, as is the practice in Paris.

2. Dough is always acid, notwithstanding that the vola-

tile

tile alkali formed in the operation neutralizes one part of the acetic acid, as is proved by the ammoniacal odour of dough treated by potass. Bread itself always contains a little of this acid, which heightens the flavour of it.

3. The starch, the undecomposed gluten, and the other materials of the dough, are so intimately united by the baking, that it is no longer possible to separate them. We can discover by the distillation of bread an animal matter, for it forms ammoniacal acetate; but a less quantity of this is obtained from it than of farina, according to the observations of M. Vauquelin.

4. The formation of carbonic acid gas is rendered evident by the volume which the dough acquires, and by the numerous cavities which are seen in it. This gas escaping while the bread is baking, dilates the mass still more, which causes the air to lodge in those cavities; an important circumstance,—whence results, say they, the remarkable whiteness of bread, full of little holes, so light, delicate and sapid, in comparison with the bread destitute of them, which is heavy, compact, and of a disagreeable taste.

It is therefore more particularly the *ferment* which has the most active share in producing pannification. Added to dough in small quantity, the operation is slow and incomplete; in too large proportion, the fermentation goes on so rapidly that it becomes necessary to check it. In this last case M. Chaptal proposes to knead some carbonate of potass with the dough, which will neutralise the excess of acetic acid. Our good housewives content themselves with uncovering the dough, dividing it, and exposing it to the air, in order to diminish the temperature of the fermenting mass; and this management sometimes succeeds.

2. *Of the Vinous Fermentation.*

This operation can only take place when sugar, water, and a ferment are mixed together. Sugar is the matter of fermentation; the ferment is the agent of it; the presence of water is a necessary condition, as well as a certain degree of temperature. It is because these three substances exist in a state of union in the saccharine juices, that these are capable of the vinous fermentation.

What are the chemical changes which substances subjected to the vinous fermentation undergo? If one considers the composition of these substances, and that of the products of the operation, it will be easy to conceive with M. Thenard, that in it the ferment takes away from the sugar

sugar a small quantity of oxygen, whereby it becomes a substance *sui generis*, whose principles not being able to remain in their present arrangement, react upon each other, combine in a new and different order, producing alcohol dissolved in water, and more carbonic acid gas; the ferment which caused these phænomena is itself altered in part, and precipitated; while the water only serves to bring the molecules into contact, and to retain the alcohol.

The methods in use for subjecting different substances to the vinous fermentation, may, according to M. Chaptal, be reduced to two; decoction and expression. The first is practised by means of water in the fermentation of the farinaceous grains, in making beer; the second is employed in fermentation of juices which afford the different sorts of wine. The details furnished by the author on the preparation of beer being borrowed from Thomson, I shall not notice them, especially as I have a great number of facts to relate on the art of making wine.

On the vinous Fermentation of the Juice of the Grape.

The sugar and the ferment existing isolated in the grape, it becomes necessary to press this fruit to obtain from it the juice called *must*, in which these two vegetable principles are mixed together. This *must* speedily ferments at the temperature of 12° of Reaumur*. M. Chaptal says it is necessary to fill the vat all at once, in order to avoid the successive fermentations that take place when the *must* is put in at various times, as this circumstance renders the wine of a bad quality.

This may be the case indeed in countries to the north, where the grapes being very watery, and but little sweet, cannot support any derangement in the fermentation; but in these southern climates this phænomenon seldom shows itself. It must necessarily require many days to fill a vat which contains 50 muids, and yet, however, the wine is very excellent which is made in this enormously large vessel.

Before the *must* is put into the vat, this latter ought to be cleaned with the greatest care; then the liquor is to be left to ferment in it. According to M. Chaptal, the vinous fermentation is influenced by a variety of circumstances, into which I am successively to inquire.

1. *Of the Influence of Temperature on Vinous Fermentation.*

Twelve degrees of Reaumur's thermometer appears to be

* About 60 of Fahrenheit.

the

the most suitable temperature for the vinous fermentation. Below this degree it languishes; above it, it becomes tumultuous. But it is not the temperature of the place only, where the fermentation is going on, which influences it; the Abbé Rozier has proved that the temperature of the grapes, at the time of the vintage, has a considerable effect upon it, that the fermentation is always slow in proportion to the low temperature of the grapes when they are gathered. This phænomenon was observed last year at Montpellier. The vintage did not commence till the latter end of October, and the weather was cool; the *must* fermented badly in the vat, and the wine produced from it was not so strong; it appeared more tart than usual when it was tunned. This wine did not part with its bad qualities until it had undergone a new fermentation in the vessel, which continued some months.

One very singular circumstance, and which has been shown by M. Chaptal, is, the difficulty of restoring the temperature of *must*, when it is very low, so as to make the fermentation go on in a regular manner. "I diluted," says the author, "some extract of the *must* of grapes, with water at four degrees above the freezing point. I added some yeast of beer to accelerate the fermentation. The fermentation took place in a short time, when the temperature was elevated to 16 degrees, but it very soon diminished. A like quantity of extract diluted, and heated to the temperature of 16 degrees, for two days before the yeast was added to it, underwent a very regular and complete fermentation."

2. *On the Influence of Air on the Vinous Fermentation.*

In order that fermentation may take place, and go through its stages in a steady and regular manner, it is necessary that there be a free communication between the fermenting mass and the air. Should we not conclude from this fact, that the air enters as a principle into the product of this operation, or as an element of decomposition? The experiments of M. Chaptal contradict this conclusion, for he has never seen the air absorbed in the vinous fermentation. Its influence is confined to the facilitating the disengagement of the carbonic acid gas produced, the presence of which would check and even stop the fermentation. The free contact of air, although so useful in this respect, has, however, one disadvantage, that it occasions a considerable loss of aroma and alcohol. Thus it is well known that wine fermented in vessels nearly close, is often the most generous and of the most agreeable flavour. It is to secure these advantages, without totally interrupting the communication with the air, that M. Chaptal advises to cover the vat with boards upon which is suspended a covering of old linen cloths;—an excellent method, and easily put in practice, as the cost of it is so trifling.

The loss of alcohol in the vinous fermentation is proved by the experiments of Dom Gentil, and by the happy application of them by M. Chaptal, in his manufactory of vinegar. It is also proved, probably, by the two following facts. Some white grapes found whole, by M. Coste, at the top of the vat in the time of tunning, tasted precisely like grapes preserved in brandy. I also saw some grapes, under similar circumstances, entirely coated with small crystals of acidulated tartrite of potass. Do not these two phænomena show that the grapes had absorbed a portion of the alcohol which escaped during fermentation, and were thereby deprived of a certain quantity of their water of vegetation? I have no hesitation in thinking so.

[To be continued.]

XLII. *Intelligence and Miscellaneous Articles.*

THE COMET.

Observations of the Appearance of the Comet. By W. CRANE, *Esq. of Boston, Lincolnshire.*

THE brilliant appearance which the comet now makes in the north, having excited the attention not only of the students in astronomy, but also that of the public at large, I hope it will direct many to the pursuit of that beautiful and interesting science; for, in this country, it must be observed with regret that it has not been of late cultivated with the ardour its importance demands.

The following observations on the comet have been taken, at eight o'clock in the evening, according to the dates below:

From the circumstance of my being, at present, in a town where I cannot have access to astronomical instruments, I am under the necessity of using a quadrant of my own construction: it is made of well dried mahogany, and its radius is 14¼ inches, which gives nearly 3-10ths of an inch for a degree. I therefore flatter myself they are not very erroneous. Its situation respecting the right ascension and declination, being taken from Senex's 18-inch celestial globe,

globe, can only be regarded as an approximation; but as these have been done with great care, they will, it is hoped, be found sufficiently correct to delineate its apparent path in the heavens.

Sept. 7, 1811, eight o'clock in the evening, Boston, Lincolnshire, lat. 53° 1′ N. long. 0° 5′ E.

From the star β in Ursa Major 14° 0′. Ditto star γ 15° 15′. Right ascension 160° 20′. Declination 42° 30′.

Sept. 8.—From the star β 13° 30′. Ditto star γ 15° 0′. Right ascension 161° 30′. Declination 42° 45′.

Sept. 9.—From the star β 13° 15′. Ditto star γ 14° 30′. Right ascension 162° 0′. Declination 42° 50′. On the 8th and 9th the star ω was seen through the tail.

Sept. 10.—From the star β 13° 0′. Ditto star γ 13° 15′. Right ascension 163° 30′. Declination 43° 15′.

Sept. 11.—From the star γ 12° 45′. Ditto star χ in the same constellation 8° 0′. Right ascension 164° 50′. Declination 43° 40′.

A line drawn from Cor Caroli to the star λ in the foot of Ursa Major passed through the comet; also one drawn from the star χ, in the same constellation, to the star θ in Draco, very nearly.

Sept. 12.—A cloudy night.

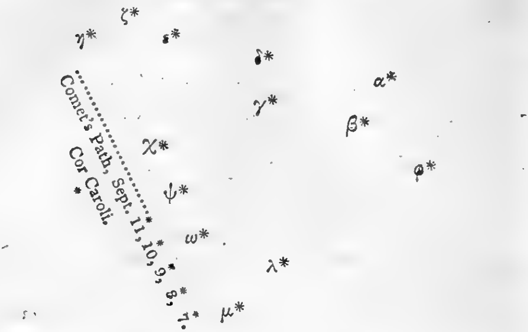
Sept. 13.—From the star γ 12° 0′. Ditto star χ 6° 0′. Right ascension 166° 40′. Declination 43° 60′.

A line drawn from Cor Caroli to ω in Ursa Major, and one drawn from φ through ψ, in the same constellation, passed through the comet.

From the above it appears that the comet will pass through the neck of Asterion, about 5° below the star η in Ursa Major, which I have endeavoured to show in the diagram: that it also passed the ecliptic about 25° of Cancer. In a small table of comets which have been observed, I find this account of those which passed through Cancer in their perihelion.

	Cancer.
1532	21° 7′ 0″
1580	19 5 50
1661	25 58 40
1739	12 38 40
1742	2 41 45
1748	6 9 24
1762	15 14 0

URSA MAJOR.



* These are taken by supposing a line to be drawn from the comet to a point directly under the star.

To Mr. Tilloch.—Sir, You will find annexed (see Plate V.) a sketch of the situation of the comet, as it appeared to the eye on the 14th of September 1811, at about 8 P. M.

The comet's place is laid down from the actual distances measured by an exceedingly good sextant made by Messrs. W. and S. Jones; the distances set down are the mean of three observations to each distance, and can be depended on to within a few seconds. They are as follows: viz.

8h 0′ 7″ mean time comet at Alioth = 18° 46′ 48″
8 3 22 do..........do at Dubhe = 19 9 40

I am, &c. H. F. P.

London, Sept. 16, 1811.

Longitude and Latitude of the Comet deduced from the Observations made at the Royal Observatory, Greenwich.

Sept. 1811	*Mean Time.*	*Longitude.*	*Latitude.*
5	8h 0	4s 25° 3′ 10″	28° 36′ 39″
6	8 0	4 25 36 12	29 23 26
7	8 0	4 26 10 5	30 10 13
8	8 0	4 26 47 12	30 52 38
9	8 0	4 27 23 7	31 41 4
10	8 0	4 28 2 53	32 30 43
11	8 0	4 28 42 47	33 20 48
12	8 0	4 29 25 35	34 9 31
13	8 0	5 0 10 21	34 57 27
14	8 0	5 0 57 24	35 53 15
15	8 0	5 1 42 59	36 44 28
16	8 0	5 2 32 7	37 37 49

ACCOUNT

ACCOUNT OF A NEW VOLCANIC ISLAND.

Extract of a Letter from a Gentleman on board His Majesty's Ship Agincourt, to his Friend in Arbroath, dated River Tagus, August 4, 1811.

"Not having it in my power to inform you of the progress of the arts in this quarter of the globe, I embrace the present opportunity of giving you a circumstantial, and, I doubt not, authentic account of the proceedings of nature, which, I presume, you will find not less interesting. His majesty's sloop Sabrina arrived here lately from a cruize off the Western Isles or Azores, and brought us the following account. On the 16th of June they observed two columns of white smoke arising from the sea, off the west end of the island of St. Michael's, which for some time they supposed to be an engagement, and made all sail towards it; but were prevented, by the wind dying away. The smoke continued to ascend with sometimes large flames of fire, and they then concluded that it was a volcano. Next day they were close in with the island of St. Michael's, and found the volcano situated about two miles west of that island, and still raging in the most awful manner. They learnt from the British consul at St. Michael's that smoke was first observed arising from that place on the 14th of June; previous to which there had been several very severe shocks of an earthquake felt at St. Michael's, so that the destruction of the whole island was much feared; but they ceased as soon as the volcano broke out. On the 18th the Sabrina went as near the volcano as she could with safety, and found it still raging with unabated violence, throwing up from under the water large stones, cinders, ashes, &c. accompanied with several severe shocks. About noon on the same day they observed the mouth of the crater just showing itself above the surface of the sea, where there were formerly 40 fathoms or 240 feet of water. They christened it 'Sabrina Island.' At three P. M. same day, it was about 30 feet above the surface of the water, and about a furlong in length. On the 19th they were within five or six miles of the volcano, and found it about 50 feet in height, and two-thirds of a mile in length; still raging as before, and throwing up large quantities of stones, some of which fell a mile distant from the volcano. The smoke drew up several water-spouts, which spreading in the air, fell in a heavy rain, accompanied with vast quantities of fine black sand, which completely covered the Sabrina's decks at the distance of three or four miles from the volcano. On the 20th they went on a cruize, leaving the volcano about 150 feet high, and a mile in length, still raging as formerly, and continuing to increase in size. On the 4th of July they again visited the volcano, and found it perfectly quiet. They went on shore on Sabrina Island (as it is now called), and found it very steep; its height not less than from 200 to 300 feet. It was with difficulty they were able to reach the top of the island; which they at last effected, in a quarter where there was a gentle declivity; but the ground, or rather the ashes, composed of sulphureous matter, dross of iron, &c. was so very hot for their feet, that they were obliged soon to return. They, however, took possession of the island, in the name of his Britannic Majesty, and left an English union-jack flying on it. The circumference is now from two to three miles. In the middle is a large basin full of boiling-hot water, from which a stream runs into the sea; and at the distance of 50 yards from the island, the water, although 30 fathoms deep, is too hot for one to hold his hand in. In short, the whole island is but a crater: the cliff on the outside appearing as walls, as steep within as they are without. The basin of boiling water is the mouth, from which the smoke, &c. issued. When the Sabrina left it, several parts of the cliff continued to smoke a little; and it was their opinion that it would soon break out again.—I presume you are informed of this strange phænomenon before now: however, as I had the foregoing account from a young gentleman belonging to the Sabrina, who was an eye-witness of what is related, I conceived it likely to contain some particulars of which you have hitherto been uninformed."

ANTIQUITIES.

In page 208 of our present Number we have given some interesting particulars respecting lord Elgin's discoveries in Greece, and the rich treasure of ancient sculptures with which his lordship has enriched this country.

We have great pleasure in being able to add, that accounts have been received from Mr. C. R. Cockerell, at Athens, of a recent discovery in the island of Ægina, highly interesting to the Arts. In excavating the earth to ascertain the Hyperthral in the ancient Temple of Jupiter Panhellenius, in the pursuit of his architectural inquiries, a great number of fragments of Parian marble of the most beautiful sculpture have been raised, the parts of which nearly complete 16 statues, between five and six feet in height, many of them in powerful action, and described as not inferior to the celebrated sculptures of the Elgin collection. It is remarkable, that of the travellers of all nations who have visited that celebrated Temple for more than a thoua thousand years past, no one preceding Mr. Cockerell should have dug three feet deep, the whole of the sculptures having been found so near the surface. It is confidently hoped, that the benefit of this extraordinary discovery will be secured to this country, by their prompt conveyance on board one of his majesty's ships of war to a British port.—Indeed we have reason to believe that orders to this effect have already been issued.

We are happy to learn that the above is not the only opportunity which will be afforded to the admirers of ancient architecture. A mission from the Dilettanti Society is on the eve of departing, under the sanction of Government, in a Turkish frigate destined for Smyrna, and commanded by capt. Gibraltar. The object of the mission is to make diligent search for antiquities and ancient relics in Asia Minor and the Ionian Isles. A young architect and draftsman of very-superior talents has generously abandoned a lucrative office, to aid by his talents the views and objects of this Society. We have no doubt that the result of this mission will gratify every admirer of the arts, and every man of taste, curiosity, and letters.

HERCULANEUM MANUSCRIPTS.

The unrolling and explanation of the manuscripts found in Herculaneum are pursued with much industry by Messrs. Rosini, Scotti, and Pessette. They have, under the patronage of the Neapolitan government, published lately some fragments of a Latin poem upon the war between Mark Antony and Augustus, and a considerable part of the second book of Epicurus upon Nature: the above gentlemen do not despair even yet of finding the whole treatise of this author. There has also been committed to the press a moral work of Pisistratus, the celebrated disciple of Epicurus; likewise some fragments of Colote upon the Lycidas of Plato, and of Caniscus upon Friendship. The entire work of Philodemus upon Rhetoric is at this moment in a state of forwardness.

ASCENT OF MR. SADLER'S BALLOON AT HACKNEY.

At about seventeen minutes before three, on Monday the 12th of August, Mr. Sadler and captain Paget having taken their seats, a barometer, thermometer, compass, two grappling-irons, a telescope, ballast amounting to about 130 lbs. weight, and refreshment, having been previously stowed in the car, Mr. Sadler, jun. desired all hands to let go; and immediately afterwards the machine began to ascend in a majestic manner, almost in a perpendicular line, to a height of about 300 yards; it then took an easterly direction.

At three o'clock, the balloon still continuing to ascend, the aërial travellers observed beneath them what appeared to be two large cisterns of water, but which subsequent observation proved to them were the East India docks. The thermometer now stood at $52\frac{1}{2}$, but from some accident which happened to the barometer, no observations on that could be made during the continuance of the voyage. The balloon being quite distended, it became necessary to let out some of the gas; and this was done at intervals till the balloon descended. Ballast, however, was thrown out: the ascent of the balloon now became very rapid, and the travellers were soon at an immense height. At ten minutes past three they crossed the Thames at Galeons-reach, and the sound of a piece of ordnance from Woolwich was distinctly heard by Mr. Sadler and his companion: they observed the smoke, which apparently arose from the earth. Mr. Sadler upon this waved his flag, and another piece of ordnance was discharged, as if to return the compliment as they passed. The city of London, the bridges, the Thames, and the German Ocean, were then distinguishable to the aëronauts; and at this period captain Paget drew the cork of à bottle of Madeira, and the health of the prince regent was drunk in a bumper. The prospect, which at this period for the first time presented itself to captain Paget, was beyond the power of description: the capital was at that time pronounced by him to be a small village; nor could he be persuaded to the contrary, till the four bridges, namely, London, Blackfriars, Westminster, and Battersea, which from their intercepting the river were rendered more conspicuous than other objects, were pointed out to him by Mr. Sadler. As the aëronauts continued their course down the river, they were saluted by the discharge of several more pieces of artillery; and at half past three they drank the health of all their friends at Hackney. About this time Mr. Sadler, perceiving that the balloon was approaching the sea, felt it prudent to look out for a spot on which to effect a landing. They then descended till the ships in the river, from Woolwich to the Nore, became perfectly distinguishable.

On crossing the river at St. Clement's reach, the balloon descended so low, that the travellers distinctly heard persons conversing in the Gravesend boats, which were passing down the river, some of whom cried out—"Where are you going?" Mr. Paget threw out a loaf, which fell to leeward

of

of one of the boats: the people on board, however, saw the action, and answered it by three cheers. At ten minutes before four Tilbury Fort came in sight, and they had a perfect view of the town of Gravesend. Mr. Sadler, observing that the country round the fort was perfectly flat, remarked to his fellow voyager, that it would be desirable to land on that side the river; and measures were taken to accomplish that object. On their nearer approach to the earth, they saw several reapers at work in a wheat-field, and hailed them for assistance: an immediate chase commenced over hedges and ditches. The balloon, however, for some time took the lead. A brisk gale was now blowing, which rendered the descent extremely difficult: the grappling-irons were, however, thrown out, and dragged along the ground. In their course they caught the clothes of a labourer, and he became so completely entangled that he could not extricate himself till his shirt was literally torn from his back. During this time the car frequently touched the ground, and rebounded again for several yards. By one of these shocks Mr. Paget was thrown out of the car, but had sufficient presence of mind to catch hold of its rim, which he persevered in holding till assistance arrived, when his companion and himself were released from their perilous situation, and safely landed on *terra firma*. At this time it wanted five minutes to four o'clock, and the travellers were within 300 yards of Tilbury Fort, and about 150 from the river, the voyage having occupied a space of one hour and thirteen minutes. The balloon was soon secured; and, being placed in a boat, the aëronauts passed over the river to Gravesend, where they dined, and immediately after proceeded in a post chaise-and-four to town, followed by a crowd of spectators, which increased to such a degree, that, long before their arrival in town, the chaise could only proceed at a walking pace. In this manner they proceeded to Hackney, at which place they arrived at ten minutes past nine o'clock, in perfect health and spirits. The only extraordinary sensation which captain Paget experienced was an extreme pain in the ear when the balloon was at its greatest height, which gradually went off as it descended, and left him perfectly free from any inconvenience.

MR. SADLER'S SECOND ASCENT.

Mr. Sadler again ascended from Hackney in his balloon, on Thursday the 29th of August, at about 20 minutes before three P. M. accompanied by Mr. Beaufoy, son of col. Beaufoy. The wind blew strongly from the south-west,

west, and the balloon rose in a majestic manner. It continued rapidly ascending, and the travellers finding themselves much inconvenienced from the number of things with which the car was loaded, immediately began to make the necessary arrangements for their proposed experiments —in the course of which the flag which had been held by Mr. Beaufoy, and which he had placed behind him on the seat, unluckily fell over the side of the car, and was picked up in Church-fields. Mr. Beaufoy describes the scene which was now for the first time presented to his view, in the most glowing terms, and says it far exceeded any thing the most fervent imagination could have pictured; and although the prospect was considerably circumscribed by a thick mist, he yet had a view of the Nore, and of all the country in that direction; but from the density of the atmosphere towards London, he was unable to get a sight of the metropolis. On their passing over Walthamstow, they distintly heard the report of several minute guns, which were discharged by Mr. Forster the banker, in consequence of previous concert. The concussion occasioned by this noise, however, had no effect upon the balloon, which pursued its course in the most majestic style. At about half-past three, while over Chelmsford, it entered a cloud, which was so extremely dense, that the earth was soon completely hidden from the view of the aëronauts, who were now exposed to the effects of a severe storm, which very much agitated the balloon.

While in this state they were visited by a hail storm, which rattled against their vehicle with great violence, and, from the subsequent melting of the stones, exposed them to all the inconveniences of the water, which trickled from the balloon. Notwithstanding these difficulties, Mr. Beaufoy expressed a desire to ascend still higher; but Mr. Sadler observing by the compass that they were taking a direction towards the sea, and apprehensive of falling in the water, thought it expedient to descend without delay, which they did by suffering a considerable quantity of gas to escape. They soon once more gained a view of the earth, towards which they rapidly approached. Throwing out a little ballast, they again ascended: but shortly afterwards they determined to finish their course; and taking measures accordingly, they threw out their grappling-irons, which becoming firmly fixed in a meadow near East Thorpe, they got out of the car with perfect ease; and with the assistance of the multitude collected on the occasion, they emptied the

the balloon of the inflammable air, and having obtained a chaise from Kelvedon, and fixed their aërial vehicle thereon, proceeded to Hackney.

Mr. Sadler states, that when the carrying pigeons which he took up with him were emancipated from the bag in which they were confined, they took a circular flight, and immediately returned to the balloon, on which they perched as if unwilling to leave it. Six of them were, however, at length forced from the car, and they winged their course towards the region from whence they had come. The seventh, on which the greatest dependence had been placed for carrying intelligence to the friends of Mr. Beaufoy, instead of obeying the wishes of his master, flew to the top of the balloon, and there kept its station till the descent, when it flew into a tree, from whence it was driven by a boy sent up for the purpose. It did not reach home till the morning following.

Metereological Observations made at Clapton in Hackney, from Aug. 29 to Sept. 23, 1811.

Aug. 29.—Clear warm morning, with strong southerly wind. A single parhelion, or mock sun, was seen at Walthamstow about seven o'clock. Soon after noon it clouded over; and then hard showers came on. The evening was fair again, with flying clouds and very variable wind. The upper currents also blew in different directions, as appeared by Mr. Sadler's balloon, which went up from Hackney, and by many small balloons.

Aug. 30.—The *cirrus* prevailed through the day, ramifying about in a lofty region, while *cumuli* floated along in a lower current. At night the wind next the earth was north; above it there was a current from the east; while beds of *cirrocumulus*, still higher, passed over from the south-west.

Aug. 31.—Misty morning; fine day, with various clouds and variable winds.

Sept. 1.—Between three and four this morning Mr. T. F. Forster first saw the comet in the north-east. This day was calm, with large spreading *cumuli.*

Sept. 2.—Clouded with intervals in the morning. In the evening the passage of sheets of light flimsy confluent *cirrocumulus* before the moon exhibited a *corona* coloured towards its edges with pale orange. Wind N. and N. E.

Sept. 3.—Wind easterly, and a clear day. The evening was clouded, and a breeze arose about eleven o'clock.

Sept. 4.—Cool north-east wind, and generally cloudy.

Sept. 5.—Early the wind was North, with much cloud. It

It afterwards blew strong from the east, and the sky became remarkably clear, and the air dry.

Sept. 6.—Perfectly clear all day, with strong east wind.

Sept. 7.—Again the sun rose and set without a cloud. The wind east and the air dry. In the evening the western horizon seemed of a bright golden colour.

Sept. 8.—A thin *stratus* obscured the atmosphere early, followed by a few light clouds; but the afternoon became clear. By twilight a variety of tints ornamented the sky; immediately above the set sun was a bright gleam of white light; above this a rich orange fading away into pale lake-colour. There was also a fine crimson blush all around. Wind S. E. and S.

Sept. 9.—A *stratus* followed by some evanescent *cumuli*, as the day cleared. Clear afternoon. Some light *cirri* appeared in the evening. Small meteors, called falling stars, by night *. Wind easterly.

Sept. 10.—A *stratus* followed by clear day, with a few *cirri*. Small meteors by night. Wind variable.

Sept. 11.—Lower current of air from S. W. Very hot day. A *stratus* was followed by a sky full of large *cirri*, generally pointing to the east. *Cirrocumuli* and other light clouds in the evening.

Sept. 12.—Wind easterly. Linear *cirri* followed by *cirrocumulus*, &c.

Sept. 13.—Filiform and other *cirri* above, while *cumuli* float below. Wind S. E. Afternoon and night very clear.

Sept. 14.—The multiform and rapid changes of the *cirrus* to-day exceed description. In some places long sheets of it first divided into lines; then, subdividing, became elegant rows of *cirrocumulus*. To the northward it presented itself in light erect tufts, curved like the architectural cyma; while in the zenith, long horizontal columns of it tapering towards the end, gave the idea of a crocodile's tail. The formation of *cirrocumulus* in different altitudes went on rapidly. In the evening the clouds were confused and lofty, and refracted a rich crimson light. The night became clear. Wind easterly.

Sept. 15.—Various appearance of the *cirrus*, and others, like yesterday. A few small meteors at night.

Sept. 16. — Clouded morning, followed by flying *cumuli*.

Sept. 17.—Early the eastern sky appeared very red. A

* On the night of the 10th of August small meteors of a very peculiar kind prevailed. They were of a whitish phosphoric light, and left long trains in the tract in which they passed.

clear

clear morning, with a few evaporating *cumuli*, which passed rapidly over between eight and ten o'clock, followed by very clear day. I found by my rain gauge that about nine inches of rain had fallen since midsummer.

Sept. 18.—Very clear, with a few light clouds. Fine golden sun-set: a crimson blush extended all round. Evaporation of water very great to-day.

Sept. 19.—Light *cirri*, mixed with *cirrocumulus*, above, little petroid *cumuli* floating under. During the day abundance of *cirrocumulus* with *cumulostratus* prevailed. In the evening, among numberless configurations of the clouds, long sheets of *cirrostratus* seen horizontally, with pendent fringes, gave the idea of the great ant eater. Wind variable.

Sept. 20.—*Cumulostratus* prevailed through day with misty air. In the evening it was cloudy, with drops of rain and flashes of lightning.

Sept. 21.—A *stratus* was succeeded by lowering clouds, and a gentle rain, with distant thunder and lightning. Air getting more damp. Wind very variable.

Sept. 22.—Clouded morning, followed by fair day, and spreading *cumulostratus* and *cumulus*. In the evening large *cirri* appeared. The horizon misty. Wind S. W.

Sept. 23.—Small rain succeeded by hard showers, in the clear intervals of which various clouds in different stations appeared. Some flashes of lightning at night. Much damper by the hygrometer than hitherto. Wind S. W.

Yours, &c.

Clapton, Sept. 24. 1811. THOMAS FORSTER.

[In future this journal will be continued to the 23d of every month.]

LIST OF PATENTS FOR NEW INVENTIONS.

To Ralph Sutton, of Birmingham, brass-founder, for his improved self-acting curtain or window-blind rack.—July 2, 1811.

To Robert Dawson, of Rownham Place, in the parish of Clifton, in the county of Gloucester, mechanic, for his improved mode of applying any moving power to machinery, and of increasing such power, and of rendering machinery more easily susceptible of a multiplicity of such powers at the same or different times.—July 3.

To Joseph Bagnall, of Walsall, in the county of Stafford, sadler and ironmonger, for his improved mode of making bridle bitts, snaffles, and bradoons for horses, and martingale hooks and rings, whereby the leather or other work belonging

belonging to the same may with much greater convenience be separated therefrom for the purpose of altering, changing, repairing, or preserving the same, as occasion may require.—July 11.

To John Trotter, of Soho Square, in the county of Middlesex, esq., for his improvements in the application of steam and other powers to useful purposes by means of suitable apparatus.—July 19.

To Claude Celestin Monnoyeur, of the parish of St. Luke, Chelsea, in the county of Middlesex, gent., for his improved process for the purification of ardent spirits, by which a pure neutral spirit is obtained without rectification or the aid of fire.—July 22.

To Joseph Badstone, of Bridgewater, in the county of Somerset, cabinet-maker, for improvements applicable to bedsteads and various other things.—July 24.

To Donald Cumming, of Whitefield, in the parish of Rothbury, in the county of Northumberland, farmer, for his machine for reaping and cutting corn, grass, and other articles.—July 26.

To Henry James, of Birmingham, merchant, and John Jones, of Birmingham aforesaid, gun-barrel maker, for certain improvements in the manufacture of barrels for all descriptions of fire arms and artillery.—July 26.

To Matthew James Mayer, of Pentonville, in the county of Middlesex, mathematical instrument-maker, for his improved construction of the instantaneous light machine.—July 31.

To Peter Durand, of Hoxton Square, in the county of Middlesex, merchant, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention of certain improvements in, or additional to, lamps, for rendering the illumination more soft and agreeable to the eye.—August 3.

To John Ashley, of Homerton, in the county of Middlesex, plumber, for his improved filtering vessel for purifying and cleansing water.—August 7.

To Thomas Gilbert, of Great Yarmouth, in the county of Norfolk, gent., for certain improvements in machinery for the delivery of bricks, tiles, ornaments, pottery ware, and other articles made in moulds, after the moulds are fitted.—August [illegible]

To Houstown Rigg Brown, of Edinburgh, coachmaker, for certain improvements in the construction of wheel carriages, wheels, axles, and boxes.—August 7.

METEORO-

METEOROLOGICAL TABLE,
BY MR. CAREY, OF THE STRAND,
For August 1811.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
July 27	66	71°	67°	30·21	62	Fair
28	68	85	68	·02	86	Fair
29	66	74	60	29·95	61	Fair
30	58	68	51	30·15	58	Cloudy.
31	61	67	56	·06	46	Cloudy
August 1	57	69	60	·01	48	Fair
2	59	76	64	29·85	66	Fair
3	60	74	60	·70	62	Fair
4	56	69	62	·76	36	Fair
5	57	66	57	·69	30	Showery
6	56	66	50	·55	10	Rain
7	54	69	51	·68	70	Fair [Rain
8	56	64	53	·45	0	Thunder with
9	55	60	54	·55	0	Showery
10	54	59	50	·85	29	Cloudy
11	50	62	51	30·10	36	Fair
12	52	66	59	·16	40	Fair
13	60	73	58	·16	36	Fair
14	55	68	55	·29	60	Fair
15	56	68	58	·24	61	Fair
16	60	66	54	·01	32	Cloudy
17	55	71	57	·11	72	Fair
18	59	72	60	·18	56	Fair [Thunder
19	63	68	58	29·70	32	Storms with
20	62	67	54	·92	39	Showery
21	56	69	58	30·10	49	Fair
22	63	72	62	·05	28	Showery
23	58	70	58	29·92	30	Cloudy
24	60	72	57	·70	48	Cloudy
25	56	67	56	.50	0	Rain
26	55	68	55	·75	62	Fair

N. B. The Barometer's height [illegible]en at one o'clock.

METEOROLOGICAL TABLE,

By Mr. Carey, of the Strand.

For September 1811.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Aug. 27	58	69	54	29·90	36	Cloudy
28	54	68	55	30·11	44	Fair
29	56	68	62	·05	36	Showery
30	55	68	58	·20	60	Fair
31	54	72	60	·05	63	Fair
Sept. 1	52	66	54	·10	52	Fair
2	50	64	54	·30	44	Fair
3	53	67	53	·29	42	Fair
4	52	62	54	·21	51	Cloudy
5	54	60	56	·20	56	Fair
6	56	71	55	·18	82	Fair
7	54	70	54	·20	89	Fair
8	52	70	55	·25	45	Fair
9	51	73	55	·20	66	Fair
10	55	74	62	·15	65	Fair
11	58	76	68	·08	78	Fair
12	56	72	58	·20	66	Fair
13	55	70	54	·20	60	Fair
14	52	71	55	·02	55	Fair
15	54	69	56	·16	57	Fair
16	52	63	56	·10	46	Fair
17	56	68	57	29·99	60	Fair
18	57	72	58	·89	63	Fair
19	58	73	56	·72	60	Fair
20	59	73	57	·56	47	Fair
21	57	64	52	·53	0	Rain
22	57	65	55	·73	40	Fair
23	56	63	54	·45	0	Rain
24	55	64	52	·55	45	Fair
25	60	60	50	28·84	0	Rain
26	[illegible]	61	48	29·22	29	Showery

N. B. The Barome[illegible] height is taken at one o'clock.

XLIII. *On the new Nomenclature adopted by the Royal College of Physicians in the new Edition of the London Pharmacopœia.*

To Mr. Tilloch.

Sir, Another Pharmacopœia has lately received the royal sanction, and our physicians are again to learn to write a new language, and our apothecaries to read it. The names which twenty years ago were chosen as sonorous and significant, are now discovered to be barbarous and false; and Greek syllables are accumulated in yet more formidable array, to the terror of old practitioners and the unspeakable happiness of young ones, who "mouthing out (not) Homer's Greek like thunder," will with little labour and no genius fill the vulgar world with wonder and awe at their astonishing knowledge.

This rage for innovation, this contemptible and pedantic folly of substituting words for things,—as if in a new word there could be a magic virtue not existing in an old one, and that the getting by heart a bead-roll of new appellations could really give a new insight into the properties of the things spoken of,—came to us, as many useless and many mischievous fashions have come, from France; and we have adopted like plain fools, what they proposed like acute rogues. It was *their* policy to subvert all former order, to confound men's ideas for a time, and then to substitute new ones: for this purpose the changing of the boundaries and the names of their provinces, the adoption of new weights and measures, with new and barbarous names pretending to be half Geek half Latin, but in truth neither the one nor the other; for this purpose, these violent innovations with many others of the same sort had their use. But for us, whose medical system was to remain unaltered,—for us, in a science where certainty and stability of names are of the highest consequence, to adopt without a shadow of necessity, or the least prospect of advantage, this wild lust of change, has, I own, always appeared to me quite astonishing; and the more, as knowing the real science and sound sense of the late sir George Baker, under whose presidency this baleful innovation was first adopted by the London College of Physicians.

There is no doubt that classification is of absolute necessity in every science, particularly in natural history, chemistry, and medicine, where many different substances are to be treated of. Without some arrangement, the whole would be a mass of inextricable confusion. It will also happen, that as science advances, substances anciently imperfectly understood will often be transferred from one class to another: but this is totally different from changing their names, which it is almost always useless and often dangerous to do.

Will any man assert, that a single physician or even apothecary did not know the composition and virtues of corrosive sublimate and calomel to the full as well under those names as under the new appellations of muriate and submuriate of mercury? But under the former nomenclature no mistake could well be committed:—under the new ones, let him who reads tremble lest the haste of the prescriber or ignorance of the compounder should pour down the throat of his child inevitable death, instead of a most salutary medicine. So paregoric elixir was known to every village nurse, nor could ignorance itself confound it with laudanum: but now they are both *tincturæ opii*, and only a third word, marked in general by its single initial C, which in hasty or careless writing may easily be mistaken for a flourish of the pen, distinguishes a medicine of drops from one of spoonsful, and discriminates to the apothecary's boy the quieter of a cough, from the quietus at once of cough and life.

When Linnæus formed his admirable system of botany, it was necessary for him to remove from their former arrangement many plants, and reclass them by his improved method. Yet he too was sometimes wanton in the exercise of his power, and without necessity changed several names. Was it necessary, or useful, for the medical man to give up the trivial and universally known names of plants for the new botanical ones? Will an emetic work the better for the infusion of *anthemis nobilis* being ordered instead of chamomile flowers? Is gum arabic harder to remember than *mimosa nilotica?* which by the by is inaccurate, as being the name of the tree producing the gum, not of the gum itself.

Numerous instances as strong as these might be adduced of the foolishness of these changes, but I think the above may suffice.

It were something (though indeed but very little) if in the pedantry of Greek names, Greek were adhered to; but in this Babylonish dialect we are presented with a heterogeneous mixture of Greek and Latin. We have *hyper*oxigenated muriatic acid (tremendous to pronounce!) and in the next line *sub*muriate of mercury. It should at least have

have been *hypo*muriate. So, too, mercury is *hydrargyrus*, (a word by the by neither Greek nor Latin, though pretending to be the former), while silver is *argentum*, plain Latin.

But perhaps this new language is universal and permanent. The change, though inconvenient, is to be made once for all. Alas! so far from it, that, whereas the old Pharmacopœia had lasted for centuries in every corner of our own islands at least, and been every where read and every where understood; now, in the progressive state of chemistry, we every year are proving that our last year's ideas were false. Hence every year is to change our Pharmacopœia with our almanacs; and what is to the full as bad, our three principal schools of medicine have each their own nomenclature, fitted, I suppose, like their almanac, to their own latitude; and Dublin and Edinburgh and London are nearly unintelligible to each other; so that an unfortunate patient who wishes that an English apothecary should make up his Irish or Scotch prescription, must send him a book to read it by, or it might as well be written in the ancient Ogham, or dictated in Erse by Ossian the son of Fingal.

All this is mighty ridiculous: still, if it were only ridiculous, I should not have thought your publication a proper vehicle for animadversion on it, but have left it to the theatres and ballad-mongers to laugh at it as it well deserves. But, sir, when we reflect that in its consequences the lives of every one of us, and of course the lives of those the most dear to us, are in a thousand ways involved; that to the anxiety of sickness is to be superadded the doubt and dread lest medical aid should be in fact a worse enemy than the disease it professes to combat; and this evil invades the world under the specious garb of improved science,—it becomes not unworthy of the Philosophical Magazine to interest itself in the cause of humanity, and to endeavour to raise amongst us a cry which may reach the ears of the common father of his people, and induce him to withdraw the permission he has given for these pernicious and multiplied changes. I do know, that when his *fiat* was obtained for this newest change in the London Pharmacopœia, his own native good sense induced him to observe that he thought it all nonsense. Had a friendly voice then been raised to state that it was not merely harmless folly, but folly replete with danger, I doubt not that he would have rejected the new edition of nonsense, and kept the College at least to their older names, which, by the habit of twenty years or more, had lost much of the power of doing mischief. That much serious nay fatal mischief has been done, every medical man in extensive practice well knows, though he may not for obvious reasons like to tell it. One instance which I know to be true, I will mention; as happily its consequences were not fatal, and its circumstances laughable:—Soon after sir George Baker had published the new Pharmacopœia, lady Baker was unwell, and applied to her husband for relief. Sir George judged a gentle cathartic to be proper, and ordered an ounce of Epsom salts; but, proud of his new nomenclature, wrote *magnes. vitriol.* The apothecary's boy read *magnes.*, but took no heed of the *vitriol.*; and of the drug so familiar to him, *magnesia alba*, stirred up an ounce with a Q. S. of peppermint water, and sent it in a large phial to lady Baker. The dose was nearly equal to the whitewashing her dressing-room ceiling, and when emptied into a slopbason had a most formidable appearance: but lady Baker, with a courage worthy the wife of Celsus or of Musa, attacked it with a spoon; and had contrived to swallow nearly enough to make a plaster cast of the interior of her stomach, when sir George arrived, and much surprised asked his wife what she was about, and whether she had a sudden attack of green sickness. Lady Baker, clearing her mouth as well as she could from the mortar that clogged the fauces, assured him she was following his prescription. A wife so heroic in the cause of medicine became doubly dear to sir George, and he immediately stopped the further progress of his spouse in this petrific process. Inquiry at the apothecary's soon cleared up the mistake, and the consequence to lady Baker was only two or three days unwellness, and two or three doses of physic to rid her inside of its load. Had calomel been ordered, lady Baker might not have been alive to tell her story to her husband at his return from his visits.

Names are given to things for the purpose of distinguishing them from each other. Those names, therefore, are the best which are the shortest, and the most dissimilar in sound, as being the easiest remembered and the least likely to be confounded or mistaken; and for the same reason commonly received names should be as little as possible altered. Significant names have no real good, and very often tend to error. Owl or swan are better names for birds than blackbird, inasmuch as they do not pretend to describe anything; whereas blackbird might lead to a supposition that no other bird of that colour existed amongst us, and might induce a foreigner to mistake a crow or a raven for a blackbird.

That

That such sort of names convey any useful knowledge of the composition or effects of things, is too absurd to be asserted. Suppose that the worshipful company of merchant-tailors were to obtain his majesty's order, that in future breeches should be called peripygeia,—drawers, subperipygeia,—waistcoats, perithorakidia,—and coats, hyperperithorakidia,—Will any man in his senses, will even the authors of the New Pharmacopœia, say that there is the least reason to suppose our clothes will in consequence of these changes be better fitted to our persons, or that our orders for a suit will be more clearly understood? But if the omission of a *sub* in the order, sends us home black satin instead of linen clothing to our tails, the evil is not great, nor the mischief irreparable. The muriate instead of the *sub*muriate of mercury will, in an hour, transfer us from the jurisdiction of the Royal College of Physicians to that of the worshipful company of parish clerks.

If the chemist in his laboratory chooses to puzzle his head by ringing the changes on *ate* and *ite* and *et*, and *ous* and *ic*, and such-like fancies, the mischief is not very great, as life and limb are not likely to be concerned, and only the studious part of the world trouble themselves at all about the matter. But those who introduce these ever varying dangers into practical life, are in conscience answerable for every mischief incurred by their wanton innovations, although they may not be amenable to the Old Bailey, nor liable to decoction, in law phrase boiling, on the statute of poisoning.

In defiance then of the clamour that may be raised by the advocates of this pseudo science, let me exhort those really learned physicians who are at the head of the practice of London, (and who I know have no hand in the foolish changes thus repeatedly attempted to be introduced, but seriously disapprove and lament them,) to act boldly, and unite to crush the evil which they feel; to tell the king what they think of the mischief likely to ensue; to state the evils they have known to arise from these changes of names, (of which I am certain a most frightful catalogue might be soon made out,) and to restore not only the ancient Pharmacopœia, purged of its real errors in the composition of medicines, and brought up to the present improvements in processes, not in names; but at the same time to introduce by his royal authority, throughout the three united kingdoms, one uniform mode of preparing medicines, that the same word may every where mean the same thing; and that an unfortunate invalid may not, in addition to the probability of having the words of his prescription fatally mistaken, if he carries it from London to Edinburgh or Dublin, have the chance of being poisoned by taking an active medicine prepared of a strength double to that of the Pharmacopœia according to which it was prescribed.

In the hope, sir, that the publication of these observations in your widely circulated Magazine may be productive of real and important good to society, I send them to you for insertion, should you judge them worthy of it; and I am, sir,

Your obedient servant,
ALEXIPHARMACOS.

XLIV. *Observations on the Article* " Fermentation," *contained in M.* CHAPTAL'S *Nouveau Cours complet d'Agriculture. By M.* DUPORTAL, *M. D. Professor of Physic and Chemistry in the Academy of Montpellier, &c.*

[Concluded from p. 226.]

3. *On the Influence by the Bulk of the fermenting Mass produced upon the Vinous Fermentation.*

IT is an incontestable fact, that the activity of the vinous fermentation is in proportion to the bulk of the mass. M. Chaptal has seen *must* contained in a cask, not finish its fermentation until the eleventh day, while a large tub, which contained twelve times the quantity, has completely fermented in four days; the heat of the liquor in the cask never exceeded seventeen degrees, while that in the large tub reached twenty-five. It will readily be conceived that the wine in the cask could not be so good as that in the large vessel, that the combination of the principles of the *must* could not be so perfect. However, a very large tub has one disadvantage: as the heat produced is so much greater, there is a greater volatilization of the alcohol and aroma, upon which the goodness of the wine so much depends.

4. *Of the Influence of the constituent Principles of Must upon the Vinous Fermentation.*

Water, sugar, and the ferment, are the principles in *must* which produce the most considerable effect upon the vinous fermentation. Too large or too small a proportion of either of these principles equally impedes the operation. When the *must* contains too small a quantity of water, it undergoes but an incomplete fermentation, because the first portions of alcohol produced being too concentrated, preserve the

the sugar yet undecomposed from the action of the ferment. Whence results a very sweet and syrupy wine, like those of Spain. This inconvenience is to be remedied by the addition of water, which immediately restores the vinous fermentation. When the *must* is too watery, fermentation will scarcely take place, because the fermentable materials are too much diluted. In this case the wine produced is weak and almost colourless. The best mode of remedying this defect, is to add some *must* evaporated to the 18th or 20th degree of Baumé's hydrometer. M. Chaptal rightly observes, that care must be taken not to evaporate the *must* to the consistence of an extract; for then the leaven would be coagulated, and it would by this management be deprived of its property of producing fermentation. The evaporated *must* is to be added to the liquor in the tub until the whole liquor attains the ordinary consistence, which is between the eighth and fifteenth degrees of the same instrument.

Experience has proved the utility of this method: it has however this disadvantage, that the precipitation of tartar is more rapid, and by a natural consequence the colour of the wine is changed;—this at least is what takes place in Languedoc. This effect may, I think, be attributed to the precipitation of the tartar, which cannot remain dissolved in the additional portion of *must*, the first formed crystals of which attract others from the whole liquid by a molecular affinity. This precipitation being admitted, it is easy to conceive that the tartar, in falling down, carries with it the colouring matter upon which it acts as a mordant. Sugar being by itself one of the most effectual preservatives of substances, its excess in the *must* cannot but render the fermentation slow and incomplete: hence results a wine, in which is found a great deal of sugar undecomposed. In this case it is necessary to add some yeast to the *must*, so as to reestablish the proper proportion between the sugar and the ferment. The addition of tartar in a small quantity, as half a pound to 100 pounds of *must*, expediting the solution of it by boiling, is advised by M. Chaptal, who considers tartar as favouring the fermentation, and rendering the decomposition of the sugar more complete.

A deficiency of sugar presents a quick and regular fermentation, and only a meagre acescent wine is produced. This is to be remedied by adding wine already made, or honey, or, what is still better, molasses, brown sugar or white sugar, in the proportion of from five to ten parts to 100 of *must*,—if this method were allowable, with us, otherwise than in theory.

This addition is particularly necessary when the fermenting principle is in excess in the *must*, as happens in cold countries and moist soils, where they make only weak sour wine, very susceptible of decomposition on account of the superabundance of the fermentable principle. On the contrary, this principle is sometimes prejudicial by its deficiency in the juice of some extremely sweet grapes. It is then proper to employ the methods proposed above, when there is an excess of sugar.

On the Progress of the Vinous Fermentation.

After having treated of the various causes which influence the vinous fermentation, M. Chaptal shortly traces the progress of this operation. He speaks of the intestine motion which gives rise to the formation of the crust called *chapeau de la vendage*; he makes mention of the heat and the disengagement of carbonic acid gas, which are constantly observable, and he points out the results of the operation. I shall say a word or two on the most remarkable of these.

1. *Of the Disengagement of Carbonic Acid Gas.*

The production of carbonic acid gas during the vinous fermentation, is caused by the reaction of the elements of the sugar already altered by the ferment, and in which there is a subtraction of carbon and oxygen. The gas first produced is dissolved in the fluid; but as the formation of it goes on, an effervescence takes place from the disengagement of the gas, which soon spreads into the atmosphere, and mephitizes it, if the precautions pointed out by M. Chaptal are not employed.

If while the wine is yet fermenting it is inclosed in well stopped bottles, the carbonic acid gas is dissolved in the wine until this is saturated; then the fermentation is stopped by the pressure of the gas, which remains free in the empty space of the bottle, and the wine thus managed becomes very brisk and sparkling. This is the method practised in Champagne, where they pour off the wine several times, to separate the sediment which is thrown down. In Languedoc, they render the very sweet white wines sparkling, by putting some grains of corn into the bottles. These grains, doubtless, supply a ferment, which exciting a fresh fermentation occasions a disengagement of carbonic acid gas.

But, as M. Chaptal remarks, it is not to the presence of carbonic acid gas only that sparkling wines owe their excellent qualities; they are also indebted for them to the aroma

aroma and a portion of alcohol, which the disengaged carbonic acid gas holds in solution. The ingenious idea of making vinegar with the gas taken from the top of fermenting vessels, proves this solution of alcohol; it is confirmed besides by M. Humboldt. Moreover, the impression which the substance in question makes upon our organs, leaves no room to doubt that it contains more than carbonic acid gas; it therefore never happens that we can exactly imitate it by a simple condensation of this gaseous body.

2. *Of the Formation of Alcohol.*

In proportion as the sugar of the *must*, when acted on by the ferment, loses its carbon and oxygen, to form carbonic acid gas, it loses also some of its hydrogen, which combining anew with oxygen, separated from the sugar, forms water. These continual subtractions change the sugar into a particular product called alcohol; a word, M. Chaptal thinks, employed in a sense far too general. The quantity of alcohol produced is always in proportion to the quantity of sugar decomposed. A set of phænomena take place during its decomposition, which I shall not now notice. The result is an intoxicating liquor called wine, in which are found alcohol, water, mucilage, tartar, a colouring matter, &c. M. Chaptal says, the source of the colouring matter is in the pellicle of the grape, and that there is an analogy between it and resin: he observes, that it is only dissolved in the *must* during fermentation, in proportion as the alcohol is developed. It is an incontestable fact, that the colour of wine is always in proportion to the quantity of alcohol produced. But I do not know that we are to attribute the colouring of the wine solely to the solvent power of the alcohol. Our author notices a colour almost as black as ink, in the very weak wines of the banks of the Cher and the Loire; and it is not uncommon at Montpellier to eat very ripe grapes, whose juice is of a very red colour. In the year 1809 the *must* here was remarkably high-coloured at the time the grapes were pressed, and yet the wine made from it was of a paler colour, and not so strong as usual. Are not these positive facts conclusive against the colour of wine solely depending upon the alcohol acting on the pellicle of the grape?

3. *Of the Acetous Fermentation.*

This fermentation differs from the preceding, in that the product is constantly acetic acid. Owing to the great facility of furnishing this acid by a slight alteration, which various

various substances possess, the processes for making vinegar are very numerous. But to have this acid in the greatest perfection, it is necessary to attend to certain circumstances favourable to its production. These I think may be stated as follows.

1. *The Presence of Alcohol, or of Matters capable of furnishing it.*

All alcoholic liquors are capable of going through the acetous fermentation. The more alcohol these liquors contain, the more acid they afford. We must observe, however, that pure spirit of wine cannot be changed into vinegar. Besides the vegetable fermentatable matter, it is necessary that this liquid should be diluted with water, otherwise but a small quantity of very strong vinegar is produced. And as the vinegar in this state of concentration is able to dissolve the alcohol, it lays hold on the existing quantity, and prevents its being acidified. Hence it is that the best vinegars are obtained from the strongest wines. There is generally some alcohol united with the acid, and imparts to good vinegars the agreeable odour which they possess.

2. *The Presence of a vegetable fermentable Matter.*

It is almost impossible to be ignorant of the agency of this matter in the acetous fermentation. Every one knows that wine deprived of this will not become sour; therefore the vinegar-makers reject wines which have been fined. It is equally well known, that wine soonest becomes sour when the lees are well shaken; and when the cuttings of the vine, the stalks of the grapes, and the dregs from the press, as well as tartar, gluten, leaven, and other vegetable matters are added to the wine, all of which act upon it as a ferment.

3. *The Contact of Air.*

Chemists are agreed in regarding the air as indispensable to the acetous fermentation, and some correct experiments have been related in support of this proposition. Bécher, however, pretends to have made vinegar in close vessels. M. Vauquelin has also made some vinegar, by filling a bottle with a solution of sugar, containing some gluten, and well stopping the bottle when full.

4. *A Temperature from* 18 *to* 22.*

Wine will become sour at a lower temperature than this; but then the fermentation is weak, and does not go through its stages with such regularity as when the atmosphere is of

* 72 to 82 Fahrenheit.

the

the proper temperature. When wine is converted into vinegar the alcohol entirely disappears, at least, as M. Chaptal observes, if the acidification is complete. But is alcohol in wine, the only principle which is changed into vinegar? It has been said, that the other matters contained in the wine contribute to the formation of this acid. Supposing this to be true, it is not less demonstrated that it is essentially by its alcohol that wine is changed into vinegar. What are the chemical changes which this substance undergoes in its conversion? This is the object of inquiry.

The phænomena may be explained by referring them to the action of the vegetable fermentable matter, and by attributing them to the influence of the air. In the former case, M. Vauquelin supposes that the fermentable matter separates from the alcohol some of its carbon and hydrogen to form ammonia, and an oily matter, leaving a more oxygenized alcohol, which is the vinegar. In the second case, it is conceived by M. Thenard that the oxygen of the atmosphere attracts the same principles from the alcohol, whence results the formation of water and carbonic acid gas, while the alcohol is converted into water. In both these hypotheses the acidification of the alcohol is attributed to the subtraction of a certain quantity of hydrogen and of carbon from the alcohol, which renders the oxygen predominant in the acetic acid produced. The excess of this oxygen is not very great, because the analysis of this acid by the oxymuriate of potass did not afford MM. Gay Lussac and Thenard more than 2·865 of oxygen in addition.

This analysis evidently shows that a very small quantity of oxygen is required to change alcohol into vinegar. It shows also, that vinegar is less oxygenized than any other of the vegetable acids; a very different conclusion from that which places this product as the last term of vegetable oxygenation. From this analysis we can further conceive, how vinegar may so easily be procured by such a variety of different means as have at different times been practised.

XLV. *Some Speculations on the Nature of Instinct.* By Arthur Mower, *Esq.*

The philosophy of the human mind is a science which of all others is the most backward, and one which by the literary world in general seems to be the least regarded. Whether it is that mankind are tired of metaphysical disputes, and will no longer interest themselves in a theory one

one day which may be pulled down the next,—whatever may be the cause,—it is certain that, in an æra the most favourable for the arts and sciences, the philosophy of the human mind has made very little advancement. The works of Dugald Stewart, so pure, so elegant, and so profound, will it is to be hoped awaken the attention of men of letters to a science, in which the acquisition of truth more than a hundred-fold repays the labour of investigation. But with all the deference and admiration which I sincerely feel for the name and works of professor Stewart, yet I am not entirely satisfied with the narrow limits he wishes to prescribe to metaphysical speculation. "The legitimate province of this department of philosophy," says Mr. S. "extends no further than to conclusions resting on the solid basis of observation and experiment; and I have accordingly, in my own inquiries, aimed at nothing more than to ascertain, in the first place, the laws of our constitution, as far as they can be discovered by attention to the subjects of our consciousness, and afterwards to apply these laws as principles, for the synthetical explanation of the more complicated phænomena of the understanding *." This may be an excellent rule; and it may be very philosophical to condemn that premature inclination to generalize, which has given to the world so many theories. Yet is it not to the love for hypothesis that we owe what little metaphysical truth we at present possess? Would Locke, Berkeley, Hume, Priestley, Hartley, and many others, have ever written on the philosophy of the mind, if they had not wished to bring forward and support some favourite theory? and should we ever have been favoured with the works of Dr. Reid and Mr. Stewart, if they had had no theories to combat, no rubbish to clear away? One man brings forward a hypothesis; another sits down with an intention to confute it, and to advance one of his own which he thinks a better, and which in all probability is to be overthrown in its turn. This seems to have been the practice of metaphysicians in every age, and it will be said that they have spent much time and employed their talents to very little purpose. It may be so. And yet, in the collision of different opinions, there have certainly been struck many sparks of pure genuine truth, sufficiently brilliant to excuse a great deal of error and nonsense, and which but for hypothesis would never have been seen. It frequently happens that, after much study

* Philosophical Essays, Preliminary Diss. p. 3.;—See also several passages to the same effect in the Introduction to his admirable work "Elements of the Philosophy of the Human Mind."

and

and a patient attention to the operations of his own mind, a philosopher feels himself dissatisfied with all theories both ancient and modern; and, beginning to generalize, he forms a hypothesis of his own, and to one principle perhaps he refers all the phænomena of the human mind. This may be philosophically wrong, but it is very natural; and if any certain theory, without offending religion or common sense, can satisfactorily account for numerous phænomena before unexplained, such a theory is surely not to be rejected because it cannot be proved with the certainty of a mathematical problem. "Si la cause supposée explique tous les phénomènes connus; s'ils se reunissent tous à un même principe, comme autant de lignes dans un centre commun; si nous ne pouvons imaginer d'autre principe qui rende raison de tous ces phénomènes que celui-là, nous devons tenir pour indubitable l' existence de ce principe*." It is very plain, that without attention "to the subjects of our consciousness," no progress can be made in the philosophy of the human mind; but this attention is in some degree painful. After a tedious observation of mental phænomena, men form a theory by way of relaxation, (they wish to taste the wine before they have pressed the grapes,) and, pleased with their own ingenuity, their hypothesis becomes a creed. This desire then of generalizing, this propensity to form theories,—which no one feels more than a student in metaphysics,—may and ought to be controlled, but is not to be too harshly censured nor too much depressed; for though hypothesis frequently proves a dancing meteor which leads its followers into bogs and quagmires, yet it may by chance conduct us into the right road, and to a benighted traveller any light is better than no light at all.

Your valuable Magazine, which is the vehicle for papers on every other science, has very seldom, I think, if ever, contained a communication on the philosophy of the human mind. In your next number, if you permit me, I will hazard a few observations on that principle which is called Instinct, and which is considered by most people as a sort of mechanical cause of action, both in man and the brute creation. I shall be most happy if the remarks I shall make awaken the attention of some other correspondent wiser than myself, who will have the goodness to correct me when I am wrong, or to treat the subject himself in a more satisfactory manner. At any rate, there is no harm in

* Ency. Fran. article *Ame*, p. 73.

offering

offering a mite into your treasury. Do not reject my offering because it is a small one: a farthing may be of value at some period of our lives, though it may remain long at the bottom of our pockets before we have occasion to use it.

XLVI. *Memorandum on the Subject of the Earl of* Elgin's *Pursuits in Greece.*

[Continued from p. 215.]

The important service rendered to the arts of this country by the zealous, indefatigable, expensive and successful exertions of lord Elgin claim grateful applause from every admirer of the efforts of that kind of human genius which presents to the eye, imitations of nature in a state even superior to what Nature herself exhibits in producing the same forms. Under this impression, and to give every circulation in our power to the detail of his lordship's labours, we intended, when we commenced this article in our last number, to have given it without any abridgement; but, apprehensive that in doing so we may commit an impropriety, we have determined to give what remains in a more condensed form.

Near the Parthenon are three small temples of the Ionic order, so connected that they might be almost considered as a triple temple. One of them was dedicated to Neptune and Erechtheus, another to Minerva Polias, and the third to the nymph Pandrosos.

The second of these is of the most delicate and elegant proportions: the capitals and bases of the columns are ornamented with consummate taste; and the sculpture of the frize and cornice is exquisitely rich. The vestibule of the temple of Neptune (now used as a powder magazine) is of more masculine proportions; but its Ionic capitals have great merit.

"Both these temples have been measured; and their plans, elevations, and views, made with the utmost accuracy. All the ornaments have been moulded; some original blocks of the frize and cornice have been obtained from the ruins, as well as a capital and a base.

"The little adjoining chapel of Pandrosos is a most singular specimen of Athenian architecture: instead of Ionic columns to support the architrave, it had seven statues of Caryan women, or Caryatides. The Athenians endeavoured, by this device, to perpetuate the infamy of the inhabitants

of

of Carya, who were the only Peloponnesians who sided with Xerxes in his invasion of Greece. The men had been reduced to the deplorable state of Helotes; and the women not only condemned to the most servile employments; but those of rank and family forced, in this abject condition, to wear their ancient dresses and ornaments. In this state they are here exhibited. The drapery is fine, the hair of each figure is braided in a different manner, and a kind of diadem they wear on their head forms the capital. Besides drawings and mouldings of all these particulars, lord Elgin has brought to England one of the original statues. The Lacedæmonians had used a species of vengeance similar to that above mentioned in constructing the Persian portico, which they had erected at Sparta, in honour of their victory over the forces of Mardonius at Platæa: placing statues of Persians in their rich oriental dresses, instead of columns, to support the entablature."

A ground plan has been made of the Acropolis, in which are inserted not only all the existing monuments, but those the position of which could be ascertained from traces of their foundations.

"The ancient walls of the city of Athens, as they existed in the Peloponnesian war, have been traced by lord Elgin's artists in their whole extent, as well as the long walls that led to the Munychia and the Piræus. The gates, mentioned in ancient authors, have been ascertained: and every public monument, that could be recognised, has been inserted in a general map; as well as detailed plans given of each. Extensive excavations were necessary for this purpose, particularly at the great theatre of Bacchus; at the Pnyx, where the assemblies of the people were held, where Pericles, Alcibiades, Demosthenes, and Æschines, delivered their orations, and at the theatre built by Herodes Atticus to the memory of his wife Regilla. The supposed tumuli of Antiope, Euripides, and others, have also been opened; and from these excavations, and various others in the environs of Athens, has been procured a complete and valuable collection of Greek vases. The colonies sent from Athens, Corinth, &c. into Magna Græcia, Sicily, and Etruria, carried with them this art of making vases, from their mother country; and, as the earliest modern collections of vases were made in those colonies, they have improperly acquired the name of Etruscan. Those found by lord Elgin at Athens, Æginæ, Argos, and Corinth, will prove the indubitable claim of the Greeks to the invention and perfection of this art: few of those in the collections of the king of Naples

Naples at Portici, or in that of sir William Hamilton, excel some which lord Elgin has procured, with respect to the elegance of the form, the fineness of the materials, the delicacy of the execution, or the beauty of the subjects delineated on them; and they are, for the most part, in very high preservation. A tumulus, into which an excavation was commenced under lord Elgin's eye during his residence at Athens, has furnished a most valuable treasure of this kind. It consists of a large marble vase, five feet in circumference, enclosing one of bronze thirteen inches in diameter, of beautiful sculpture, in which was a deposit of burnt bones, and a lachrymatory of alabaster, of exquisite form; and on the bones lay a wreath of myrtle in gold, having, besides leaves, both buds and flowers. This tumulus is situated on the road which leads from Port Piræus to the Salaminian Ferry and Eleusis. May it not be the tomb of Aspasia?

"From the theatre of Bacchus, lord Elgin has obtained the very ancient sun-dial which existed there during the time of Æschylus, Sophocles, and Euripides; and a large statue of the Indian or bearded Bacchus*, dedicated by Thrasyllus in gratitude for his having obtained the prize of tragedy at the Panathenaic festival. A beautiful little Corinthian temple near it, raised for a similar prize gained by Lysicrates, and commonly called the Lantern of Demosthenes, has also been drawn and modelled with minute attention. It is one of the most exquisite productions of Greek architecture. The elevation, ground-plan, and other details of the octagonal temple, raised by Andronicus Cyrrhestes to the Winds, have also been executed with care; but the sculpture on its frize is in so heavy a style, that it was not judged worthy of being modelled in plaster."

A search made through the churches and convents in Athens and its neighbourhood, by permission of the archbishop, furnished many bas-reliefs, inscriptions, ancient dials, and other antiquities. From the English consul Logotheti lord Elgin obtained a bas-relief of Bacchantes, as well as a *quadriga* in bas-relief, with a Victory hovering over the charioteer; and many curious antique votive tables with sculpture and inscriptions were purchased from the peasants.

"A complete series has also been formed of capitals, of the only three orders known in Greece, the Doric, the Ionic, and the Corinthian; from the earliest dawn of art in Athens,

* This statue is represented by Stuart with a female's head, and was called by him the Personification of the Demos of Athens.

to

to its zenith under Pericles; and, from thence, through all its degradations, to the dark ages of the Lower Empire.

"At a convent called Daphne, about half way between Athens and Eleusis, were the remains of an Ionic temple of Venus, equally remarkable for the brilliancy of the marble, the bold style of the ornaments, the delicacy with which they are finished, and their high preservation. Lord Elgin procured from thence two of the capitals, a whole fluted column, and a base.

"Lord Elgin was indebted chiefly to the friendship of the captain pacha for the good fortune of procuring, while at the Dardanelles, in his way to Constantinople, the celebrated Boustrophedon inscription from the promontory of Sigæum, a monument which several ambassadors from Christian powers to the Porte, and even Louis XIV. in the height of his power, had ineffectually endeavoured to obtain."

By the aid of this valuable acquisition, "lord Elgin's collection of inscriptions comprehends specimens of every remarkable peculiarity in the variations of the Greek alphabet, throughout the most interesting period of Grecian history.

"A few bronzes, cameos, and intaglios, were also procured; in particular, a cameo of very exquisite beauty, in perfect preservation, and of a peculiarly fine stone: it represents a female centaur suckling a young one. Lord Elgin was equally fortunate in forming a collection of Greek medals, among which are several that are very rare; others of much historical merit; and many most admirable specimens of art."

From different sources, particularly from various religious establishments in Greece, a great many MSS. have been brought home, and a particular catalogue and description of such as were left behind.

"In proportion as lord Elgin's plan advanced, and the means accumulated in his hands towards affording an accurate knowledge of the works of architecture and sculpture in Athens and in Greece, it became a subject of anxious inquiry with him, in what way the greatest degree of benefit could be derived to the arts from what he had been so fortunate as to procure."

The architectural works of course must be engraved—and arrangements are suggested for bringing them, in point of expense, within the means of professional men.

"More difficulty occurred in forming a plan for deriving the utmost advantage from the marbles and casts. Lord Elgin's first attempt was to have the statues and bas-reliefs

 restored;

restored; and in that view he went to Rome, to consult and to employ Canova. The decision of that most eminent artist was conclusive. On examining the specimens produced to him, and making himself acquainted with the whole collection, and particularly with what came from the Parthenon, by means of the persons who had been carrying on lord Elgin's operations at Athens, and who had returned with him to Rome, Canova declared, That however greatly it was to be lamented that these statues should have suffered so much from time and barbarism, yet it was undeniable, that they had never been retouched; that they were the work of the ablest artists the world had ever seen; executed under the most enlightened patron of the arts, and at a period when genius enjoyed the most liberal encouragement, and had attained the highest degree of perfection; and that they had been found worthy of forming the decoration of the most admired edifice ever erected in Greece: that he should have had the greatest delight, and derived the greatest benefit, from the opportunity lord Elgin offered him of having in his possession, and contemplating, these inestimable marbles: but, (his expression was,) it would be sacrilege in him, or any man, to presume to touch them with a chisel. Since their arrival in this country, they have been thrown open to the inspection of the public; and the opinions and impressions, not only of artists, but of men of taste in general, have thus been formed and collected. From these, the judgement pronounced by Canova has been universally sanctioned; and all idea of restoring the marbles has been deprecated. Meanwhile, the most distinguished painters and sculptors have assiduously attended this museum, and evinced the most enthusiastic admiration of the perfection to which these marbles now prove to them that Phidias had brought the art of sculpture, and which had hitherto only been known through the medium of ancient authors. They have attentively examined them, and they have ascertained that they were executed with the most scrupulous anatomical truth, not only in the human figure, but in the various animals to be found in this collection. They have been struck with the wonderful accuracy, and at the same time the great effect of the minutest detail; and with the life, and expression, so distinctly produced in every variety of attitude and action. Those more advanced in years have testified the liveliest concern at not having had the advantage of studying these models. And many who have had the opportunity of forming the comparison (among these are the most eminent

nent sculptors and painters in this metropolis) have publicly and unequivocally declared, that, in the view of professional men, this collection must be far more valuable than any other collection in existence."

Two suggestions have met with much approbation, in a view to the improvement to be obtained to sculpture from these marbles and casts—"The first, that casts of all such as were ornaments on the temples should be placed in an elevation, and in a situation, similar to that which they actually had occupied; that the originals should be disposed in a view to the more easy inspection and study of them; and that particular subjects should occasionally be selected, and premiums given for the restoration of them. This restoration to be executed on casts, but by no means on the originals; and in the museum itself, where the character of the sculpture might be the more readily studied.

"Secondly: From trials which lord Elgin was induced to make at the request of professional gentlemen, a strong impression has been created, that the science of sculpture, and the taste and judgement by which it is to be carried forward and appreciated, cannot so effectually be promoted as by athletic exercises practised in the presence of similar works; the distinguishing merit of which is an able, scientific, ingenious, but exact imitation of nature. By no other way could the variety of attitude, the articulation of the muscles, the description of the passions; in short, every thing a sculptor has to represent, be so accurately or so beneficially understood and represented.

"Under similar advantages, and with an enlightened and encouraging protection bestowed on genius and the arts, it may not be too sanguine to indulge a hope, that, prodigal as nature is in the perfections of the human figure in this country, animating as are the instances of patriotism, heroic actions, and private virtues, deserving commemoration, sculpture may soon be raised in England to rival the ablest productions of the best times of Greece."

XLVII. *An Attempt to classify certain luminous Phænomena observed about the Sun and Moon.* By THOMAS FORSTER, *Esq.*

To Mr. Tilloch.

SIR, I SHALL have frequent occasion, in meteorological observations intended to be communicated through the medium of the Philosophical Magazine, to mention certain

 luminous

luminous appearances observed about the sun and moon, and occasioned by the refraction of their light through an intervening cloud of peculiar structure, which are in common called halos, coronæ, burs, &c. But as these phænomena vary considerably, and as I know of no existing nomenclature calculated to express, with precision, their several appearances; and as the above terms are very frequently confused with one another, I shall take the liberty to offer one, which, however imperfect, may serve till a better shall be found. I shall endeavour to classify them (for want of a better criterion) according to the various *shapes* or *figures* which they present. It must be remembered, that their various figures are the result of the particular construction of the cloud which refracts their light: a correct attention, therefore, to these appearances may lead to a more perfect knowledge of the structure of the refracting medium.

HALO*. Def. *Circulus vel annulus lucidus aream includens, in cujus centro sol aut luna apparet.*

Obser. By a halo I understand an extensive luminous ring, including a circular area, in the centre of which the sun or moon appears, whose light, refracted through the intervening cloud, gives rise to the phænomenon. Halones are called *lunar* or *solar*, according as they appear round the moon or sun. They are generally pretty correct circles; I once, however, saw a halo of a somewhat oval figure. Halones are sometimes coloured with the tints of the rainbow.

HALO DUPLEX. Def. *Duo annuli, in quorum centro communi sol aut luna videatur.*

Obser. A double halo is not a very common occurrence. I have observed that simple halones are generally about 45 degrees in diameter: in case of double halo, it might be worth while to take the diameters of each of the concentric circles.

HALO TRIPLEX. Def. *Tres annuli, in quorum centro communi sol aut luna apparet.*

Obser. Triple halones are extremely rare occurrences.

HALO DISCOIDES. Def. *Annulus aream reliquâ nubis parte lucidiorem continens, in cujus centro luna aut sol visus est.*

Obser. A discoid halo may be said to be a halo consti-

* The word *halo* or *halos* is evidently derived from the Greek ἅλων or ἅλως, signifying an *area*. The Latin writers appear to have spoken indifferently of halones, halyses, coronæ, circuli, &c. without sufficiently distinguishing between the *corona* and the *halo*—in other words, between the *luminous disk* and the *luminous ring*.

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tuting the boundary of a large corona: it is generally of less diameter than usual, and often coloured with the tints of the *iris*. A beautiful one appeared on the 22d of December 1809, about midnight, during the passage of a *cirrostràtus* before the moon.

CORONA. Def. *Discus lucidus, vel portio circularis nubis reliquâ lucidior, in cujus centro sol aut luna videtur.*

Obser. When the sun or moon is seen through a thin cloud, a portion of the cloud, more immediately round the sun or moon, appears much lighter than the rest of it: this luminous disk, if I may be allowed the expression, I call a *corona.*

Coronæ are of various sizes, according to the peculiarities of the refracting vapour: but they seldom exceed 10° in diameter; they are generally faintly coloured at their edges.

Frequently, when there is a halo encircling the moon, there is a small *corona* more immediately round it. Coronæ, as well as halones, have been always observed to prognosticate rain, hail, or snow. As far as I can observe, they are generally seen in the *cirrostratus* cloud.

CORONA DUPLEX. Def. *Discus lucidus, alium discum paulo lucidiorem ac minorem includens, in quorum centro communi sol vel luna observatur.*

Obser. A double corona is very common; but I never remember to have seen a triple one.

PARHELION. Def. *Imago solis falsa, vel plures imagines ejusdem generis circa solem circulatim dispositæ, et magis minúsve halonibus aliisque lucidis vittis comitatæ.*

Obser. Parhelia vary considerably in general appearance; sometimes the sun is encircled by a large halo, in the circumference of which the mock suns usually appear: these have often small halones round them: they have usually a horizontal band of white light of a pyramidal figure extending from them: sometimes a large semicircular band of light, like an inverted arch, seems to rest upon the halo which encircles the sun: but these phænomena vary too much to be particularly described here: their peculiarities ought to be minutely observed and noted down in a meteorological journal.

PARASELENE. Def. *Lunæ imago falsa, vel plures imagines hujus generis circa lunam dispositæ, et magis minúsve halonibus aliisque lucidis vittis comitatæ.*

Obser. The *paraselene*, the *parhelion*, and the several kinds of *halo* and *corona*, all appear to be the consequence

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of the intervention of cloud between the spectator and the sun, moon, or planet, the peculiar refraction of which causes the appearance: but there is another well-known phænomenon which always appears in a cloud opposite to the sun or moon, namely the

Iris. Def. *Circulus maximus coloratus in nube soli oppositâ visus, qui, quòd portio ejus tantùm videatur, arcus appareat.*

Obser. The rainbow is too familiar to every one to need any particular description.

Iris duplex. Def. *Duo circuli maximi colorati, quorum centro communi sol oppositus est; qui, quòd eorum portiones tantùm videantur, arcus appareant.*

Obser. Irises or rainbows always appear in the *nimbus* when that cloud is pouring down rain from a situation opposite to the sun.

Concerning the cause of *halones*, &c. all that can be said is, that these phænomena must depend on some peculiar unexplored structure of the refracting cloud. I believe, always a *cirrostratus*, in cases of *halo*, *parhelion*, and *paraselene;* though a *corona* is sometimes seen in thin flimsy confluent *cirrocumulus*. To me it appears that the angle of the semidiameter of a halo must always correspond with the angle at which the rays are refracted, as they must all fall physically parallel on every part of the cloud.—For more particulars relative to the structure of clouds refracting these phænomena I must refer your readers to Phil. Trans. vol. v. 1065;—xxii. 535;—xxxi. 212;—xxxix. 118;—xlvi. 196;—lii. 3;—M. Helvetius, end of "*Merc. in Sole*;"—Des Cartes's Treatise of Meteors;—M. Huygens's Posth. Works, p. 293; and Newton's Optic. 1st edit. p. 134.

Thomas Forster.

XLVIII. *Description and Analysis of a Meteoric Stone which fell in the County of Tipperary, in Ireland, in the Month of August* 1810. *By* William Higgins, *Esq.*

To Mr. Tilloch.

Dear Sir, As meteoric stones have lately engaged the attention of the philosophical world, perhaps the following description and analysis of a stone that had fallen last August, during a thunder-storm, in the county of Tipperary, in Ireland, very near the house of Maurice Crosbie Moore, esq. will be acceptable to many of the numerous readers of your very useful Journal. It will at least add to the authenticity of of those strange and unaccountable visitors, and tend to prove the resemblance to each other of the stones that have fallen in different parts of the world.

This stone was sent last spring to the Dublin Society, with an account of the circumstances attending its fall, in a letter from Mr. Moore, a printed copy of which I inclose. It was not injured by the fall, and was somewhat of a cubical shape, with the angles and edges of two sides rounded: the other two opposite sides exhibited a very uneven surface, occasioned by depressions and prominences, as if a part had been broken previous to the heat to which it must have been exposed before its fall.

It weighed seven pounds and three quarters, and the entire surface was covered over with a brownish black thin crust, evidently the effects of fusion by an intense and rapid heat. When broken, its internal appearance is of an ash-gray colour, and of a gritty coarse fracture in some degree resembling sand-stone, except some particular parts where a specular appearance occurs somewhat like blackish-gray gneiss: in this case the smooth surfaces do not adhere so firmly as the other parts; the dark colour proceeds from malleable iron, which forms here and there a very thin coating. When its texture is closely examined, the following substances are very distinguishable:

1st. Dark-gray particles of malleable iron without any regular shape, which when rubbed with a file exhibit the brightness of that metal; they are very numerously dispersed, and of unequal magnitude. Some very small bright particles of iron also occur.

2dly. Particles of martial pyrites of various colours, some reddish yellow, some yellowish white, and some very few of a purple colour not unlike copper ore; the latter is in small grains, and also the reddish yellow, although much of it is minutely disseminated.

3dly. Very few round globules about the size of mustard seed, of a grayish-brown colour, which readily give way to the file, and seem to contain no metallic matter. These different substances are held cemented together by a whitish-gray earthy matter. Very small yellowish-brown spots, very close to each other, run throughout the whole mass of the stone, which proceed from oxide of iron.

The specific gravity of the stone is 3·67.

The Analysis.

I reduced one hundred grains of the stone to powder, then

 separated

separated the metallic part by a magnet; I pounded it a second time to separate more earth: this process was repeated until I obtained the iron as free from martial pyrites and earthy particles as possible in this mechanical way: it weighed thirty-five grains: muriatic acid was poured on it, and digested nearly in a boiling heat until every thing soluble was taken up. I filtered the solution, and the insoluble part when washed and strongly ignited weighed 9·25 grains. I precipitated the iron with carbonate of ammonia, then boiled the mixture to separate the whole of the iron: when cold it was filtered, and the iron washed and ignited weighed 28 grains. The filtered liquor was evaporated to dryness, and the salt was again dissolved in distilled water. On standing some time, light brown flocks, which showed no vestige of nickel and very little of iron, were deposited; the quantity was so small it could not be weighed. The solution when filtered was very clear, and of a slight green colour: on dropping a solution of potash into it the colour changed to blue, and by continuing to add the alkali a precipitation took place. This precipitate when collected on a filter was of a pale green colour, and the filtered liquor was quite colourless. Hydro-sulphuret of ammonia produced no precipitation in this liquor; which convinced me, contrary to what was expected, that the whole of the nickel had been thrown down by the potash. The green precipitate, dried as much as possible on the filter, weighed four grains and a quarter, and was of a deep apple-green colour. When ignited it weighed only three grains. Pure ammonia was poured on it, which by means of a moderate heat dissolved the whole of the nickel: the solution was of a purple-blue colour, and one grain and a half of magnesia was left behind.

The remainder of the 100 grains, the pyritic and earthy parts (65 grains), were treated with muriatic acid in the same way. The insoluble earthy part weighed 39 grains, and the oxide of iron 11 grains. The magnesia was thrown down by potash, and when ignited weighed 7 grains and a half. The liquor after the magnesia was separated was quite colourless; and sulphuretted hydrogen, although it contained free ammonia, produced no precipitation. A quarter of a grain of nickel was separated from the magnesia by means of ammonia.

It remained now to ascertain the quantity of sulphur: for this purpose 100 grains of the stone were reduced to coarse powder: on this a larger quantity of strong muriatic acid

acid was poured than was sufficient to take up the soluble parts: when the effervescence ceased, which happened in about 24 hours, a light gelatinous looking matter floated in the solution; a portion also rested on the surface of the earthy part at bottom. This was carefully collected on a filter; washed and dried it weighed 3·50; when thrown on a hot iron it burned with a blue flame accompanied with a sulphurous smell, and a fixed residuum that weighed two grains was left behind, so that one grain and a half of sulphur only had been separated. When the earthy insoluble part was washed, and dried in a gentle heat, and then thrown into a red-hot crucible, a slight smell of sulphureous acid was very sensible; therefore the whole of the sulphur was not obtained in the first instance: perhaps, if we allow half a grain to the earthy part, we shall be nearly accurate.

The gas extricated during the solution of the stone, from its strong hepatic smell must have carried away much sulphur: to ascertain the quantity, I collected the whole of the gas given out by 100 grains of the stone in different jars over mercury, and washed it in lime-water as it was received in successive portions: much of the gas was condensed in most of them, yet no lime was thrown down. The quantity of the condensable gas gradually decreased as the solution advanced, until at last little or no diminution took place.

The gas thus washed was deprived of the hepatic smell, and presented to flame burned with an explosion. It was remarkable, that after the explosion took place the combustion continued descending gradually in the jar with a beautiful red flame, until it reached the surface of the limewater at the bottom. This phænomenon induced me to suspect some carbonic matter in the gas; but on closing the jar as soon as the combustion ceased, the surface of the limewater showed no appearance of carbonic acid. 24 cubic inches of gas were obtained in all, and of these six cubic inches were sulphuretted hydrogen. This quantity must at least contain two grains of sulphur. From the foregoing statement the proportions of the different substances contained in the stone are as follow:

Silex	48·25
Iron	39·
Magnesia	9·
Sulphur	4·
Nickel	1·75
	102·

Three

Three analyses of bits taken from different parts of the stone were made, and each gave the same substances, but the proportions varied a little. I will describe one more. A fragment of the stone with a part of the external crust adhering to it was reduced to as fine powder as possible. 100 grains of this powder were calcined in a platina crucible exposed to a strong red heat for more than half an hour. It acquired a reddish-brown colour, and although much sulphurous acid was disengaged by the process it gained a little more than one grain in weight: dilute muriatic acid was poured on it, a slight effervescence ensued, which was much increased by the application of heat: the gas evolved had the smell of sulphuretted hydrogen. When the effervescence nearly ceased, the clear solution was decanted off, and more acid poured on, and boiled down nearly to dryness; distilled water was added, and the solution was filtered; both solutions were mixed together. The insoluble earthy part was washed with distilled water repeatedly, until it passed through quite tasteless. After exposure to an obscure red heat for some time, it weighed 46 grains. The iron was thrown down from the solution by carbonate of ammonia; the mixture was boiled before it was filtered, in order to disengage the whole of the iron. The oxide of iron thus obtained, after being exposed to a red heat, weighed 42 grains.

The liquor, after the iron had been separated, was of a very light blue colour; pure potash was gradually dropped into it while any precipitation took place. The precipitate was carefully collected on a filter, and well washed: when ignited it weighed 14·50 grains: it was of a yellowish-white colour with a slight shade of green. The liquor left by the magnesia was colourless, and contained ammonia in a free state. To separate any nickel it might contain, a stream of sulphuretted hydrogen was passed in it for some time: a slight black precipitate was produced, but the quantity was so very small I barely could ascertain that it contained nickel.

The magnesia was next dissolved in muriatic acid. The solution had a green colour. Sulphuretted hydrogen threw down a black precipitate, which when collected, washed, and dried, weighed 3·25 grains; when calcined it was reduced to 1·75 grains. This powder was dissolved in muriatic acid; an excess of ammonia was poured on, which threw down somewhat less than a quarter of a grain of iron; the remainder (one grain and a half) was held in solution by muriate of ammonia; the liquor had a fine violet-blue colour, which convinced me that the substance dissolved was nickel.

nickel. The result of this analysis gives nearly the following proportions:

Silex	46·
Iron	42·
Magnesia	12·25
Nickel	1·50
Sulphur	4·
	105·75

I shall avoid giving a detail of the various experiments made during the above analyses, with a view to ascertain the presence of other substances in the stone, particularly that of lime and alumine. I collected a few of the larger grains of iron, and rendered them perfectly bright by trituration in sand and water; these were dissolved in sulphurous acid, without producing the smallest appearance of carbon. This stone, therefore, contained only the same kind of substances which the celebrated Mr. Howard found in the different stones that he so ably analysed.

Corresponding also with this chemist's results is the increase of weight discovered in summing up the quantities of the ingredients. This increase must be attributed to the absorption of oxygen by the metallic bodies.

I am, dear sir,

Your very humble servant,

WILLIAM HIGGINS.

Dublin Society House,
July 28, 1811.

Letter from Mr. MOORE *to Mr.* HIGGINS.

"SIR,—I had the honour of receiving a letter, requesting from me the particulars respecting a meteoric stone that fell near my house in the county of Tipperary, and which a short time ago I did myself the pleasure of presenting to the Dublin Society. The particulars are as follow:—Early last August, between eleven and twelve o'clock in the morning, I went from Mooresfort to Limerick; the day was dark and sultry. I returned in a few days, and was immediately informed by my steward and butler that a most wonderful phænomenon had occurred very soon after my departure; they produced the stone, and gave the following account of the occurrence: there had been thunder; some workmen who were laying lead along the gutters of my house were suddenly astonished at hearing a whistling noise in the air; one said, The chimney is on fire; another said, It proceeds from a swarm of bees in the air. On looking up, they observed

served a small black cloud very low, carried by a different current of air from the mass of clouds, from whence they imagined this stone to have proceeded: it flew with the greatest velocity over their heads, and fell in a field about three hundred yards from the house: they saw it fall. It was immediately dug up, and taken into the steward's office, where it remained two hours cooling before it could be handled. This account I have had from many who were present, and agree in the one story. I saw myself the hole the stone made in the ground; it was not more than a foot in depth. Should any thing further be wished for from me, I shall feel myself very happy in procuring from the men themselves their own account, and transmitting their own exact words and description to the Society.

"I am, sir,

"Your very humble servant,

"13, Lower Mount-street, May 22, 1811." "MAURICE CROSBIE MOORE.

XLIX. *Remarks upon the inferior Strata of the Earth occurring in Lancashire: with some Observations arising from the Subject. By Dr.* CAMPBELL, *Kendal**.

THE subjoined article presents the reader with a new species of communication,—geological and mineralogical information *illustrated with a coloured map*. It was transmitted to the Bath Society by a highly respectable gentleman, Dr. Wilkinson, with a notification that Dr. Campbell, the author, was induced to write this essay, from observing the imperfect account of the stratification of Lancashire, by Holt † and other agricultural writers. It were to be wished that the first specimen of the kind had been a geological account of the strata of Somersetshire, or some county comprehended in the title of the Society; but there is no doubt this example will be followed by others. Mr. W. Smith, a celebrated engineer, has arranged materials for a similar illustration of the counties contiguous to Bath; which, it is hoped, as soon as accomplished, will be submitted to the public. The advantages to be derived from a plan of this kind, generally adopted, must be of considerable importance, both to the agriculturist and the miner.

The article is introduced in the volume from which we have copied it, with the following prefatory note:

* From Letters and Papers of the Bath and West of England Society, vol. xii. † John Holt's Agricultural Survey of Lancashire, 8vo, 1795.

"[We

"[We are obliged to the very ingenious Dr. Wilkinson for the following chart and description of the strata in the county of Lancaster; which, though it is a trespass upon our usual plan, we shall venture to insert in our Report without any other apology than the singular importance of the subject; and we earnestly recommend it to our correspondents to pursue a plan so happily begun. A similar history of the strata in all the counties of England would be an invaluable treasure. It would enable agriculturists, as well as geologists, to collect most important facts, and draw many practical inferences. They would learn with precision to adapt their crops to the nature of the soil; and in many cases they would be able to ascertain whether a cheap corrective of any particular soil could not be procured, either at a moderate depth from its surface, or from lands that lie contiguous.—Ed."]

Lancashire appears to consist of a regular succession of strata of different kinds of rock which compose the *base* (or, as it may be termed, the *bone*) of the country; whose respective limits may be ascertained with tolerable precision, and arranged in various districts. In these divisions of the country, although extreme accuracy cannot be expected, (from the rock of one division occasionally encroaching upon the limits of the other,) yet the general conclusions will be found to be just.

I. If you place before you a map of Lancashire, and draw a line nearly east and west through Cartmell and Ulverstone, the whole district north of it is a mountainous country, whose rocky hills are universally of that kind of stone which is by naturalists denominated *schistus;* and from its component parts occurring in various proportions, has obtained the different names of *slate, whinstone, blue rag, trapp,* &c.

In this district there is neither *freestone* nor *limestone,* except a vein of the latter which lies in a crack or fissure of the blue rocks, (by miners termed a dyke,) and runs across the country from east to west, near Connistone Fells.

II. If another line be drawn nearly parallel to the former, a few miles north of Lancaster, the base of the whole district included betwixt them is *limestone,* immense rocks of which raise their gray heads above the fertile land at their feet. Such are Warton Cragg, Hampsfield Fell, and the rocks of Silverdale and Kellet. Large blocks of *granite,* mostly of a round form, are frequently to be met with on the surface of the earth in the limestone (and perhaps in the

the bluestone) district: although there be no rock of this species that I know of in the country*. They have the appearance of having had their edges broken away, and rounded by attrition, like the smaller pebbles.

This mass of limestone runs away to the north, as far as Kendal, and eastward by Farlton-Knot, Hutton-Roof, and Kirkby-Lonsdale, towards Settle and Skipton in Yorkshire, and skirting Lancashire, comes into it again about Chipping and Clitheroe.

III. This limestone district ceases about five miles north of Lancaster, and gives way to another arrangement, viz. of *shale* (or *shiver*†) and *freestone*. But as freestone is an ambiguous term, arising merely from the facility with which a stone can be wrought by the tool, and is applicable to stones of very different qualities, it will perhaps be better to distinguish the particular kind occurring here by Mr. Whitehurst's denomination of *millstone grit*; being, like that which he notices in Derbyshire and other places, composed principally of *quartz* pebbles (a species of flint) and *mica*.

From this part of the country about Kellet, to as far south as a line drawn from the vicinity of Colne towards Blackburne and Chorley, and so on to the westward, the base of the country is *millstone grit*, mostly incumbent on *shale*.

In some places, (such as Grassyard woods and Ravenscar, about five miles from Lancaster,) and near Catshaw Factory in Wyresdale, this arrangement of the gritstone resting on immense beds of shale is evident. In other places it is not so easy to be ascertained, as, the incumbent stratum of gritstone having been destroyed, the shale appears on the surface of the earth, or the grit rock has not been penetrated deep enough to arrive at the shale. Finally, the beds of shale differ in thickness, and are in some instances, perhaps, totally wanting.

IV. After proceeding as far south as Chorley and Burnley, the primitive strata change to a soft argillaceous sandy stone, containing no quartz pebbles. Such are the inferior strata which form the base of the country in each of these districts; and which have considerable influence on the incumbent soil, and its productions; which we shall proceed to notice.

I. The district of *Schistus*, or *Blue Rock*.

The summits of the highest mountains of Furness must, from

* The nearest granite rock is on the west side of the road from Kendal to Shap, near the twelfth mile-stone.

† *Shale* (or *shiver*) appears to consist of indurated clay, mica, and a little sand.

from their great heights and rocky natures, ever remain in a sterile state; but yet grass grows, and sheep pastures extend to the tops of many of considerable altitude. Where they are of a lower or secondary order, they are capable of being covered with wood, which grows freely. This kind of rock being of a fissile nature, and composed of laminas or layers, mostly arranged so nearly perpendicular as to form an angle of about 90 with the horizon, the roots of trees find a passage for their small fibres to insinuate themselves betwixt them, and thus obtain a ready vegetation on the almost naked stones. Most of the woods with which that country abounds growing upon rocky ground, where nothing else would be produced, but which flourish there spontaneously, if only fenced from cattle *; thousands of acres that would be absolutely useless under any other culture, are by this means rendered highly beneficial. The woods are in general cut every fifteen or sixteen years, and the crop is reckoned of the same value as the land. For instance, if the cutting, or *fall*, (as it is here termed,) be worth 16*l*. and the land were to be sold immediately after, it would be estimated at the same sum.

Exclusive of what comes under the denomination of timber, these woods are converted to many uses; particularly into charcoal, for smelting the iron ore from Low Furness, in the many furnaces dispersed over the country for that purpose. Poles for hoops, and the seemingly insignificant article of birch besoms, are obtained from these woods in great quantities. Bird-lime is also manufactured from the bark of the holly, which abounds in the country, and is principally exported to the West-Indies for the destruction of insects.

These woods contain all the variety of trees natural to the kingdom, but they consist mostly of oak and ash. Hazles grow in great abundance in the country north of Lancaster; insomuch that it was computed that upwards of 1000*l*. worth of nuts were sold there at the last Michaelmas fair, principally bought for the Manchester and Liverpool markets.

Immediately incumbent on the rocks generally lie beds of what in this country is called sammel, (rubble,) which is the rocky matter broken down into small particles, and forming a compact bed of a particular kind of gravel. This furnishes most excellent materials for the roads, for which the whole of this district is so justly celebrated. With

* Mr. Holt, in the Agricultural Survey of Lancashire, states, that "there are no natural woods in the county."

scarcely

scarcely a turnpike-gate in the country, they resemble more the walks in a garden or a pleasure-ground than a highway.

This mass of sammel is covered by the soil of the country, generally a light hazle mould, produced by the remains of decayed vegetables, and what manure has been added by culture. It is however in general thin and stony. Some idea may be formed of its nature, by observing the brooks and rivers after the heaviest and longest continued rains. They are augmented in bulk, but no alteration takes place in their transparency: clear as crystal, the smallest pebble may be still seen in their bottoms.

The lower grounds are fertile, and the frequent showers which are attracted by the lofty mountains contribute much to the growth of wood and grass, but render it less eligible as a corn country.

No marl is found in this district; consequently the system of manuring is reduced to lime, dung, and the ashes of turf and peat, (the principal fuel of the country,) found to be a great fertilizer. Lime is expensive, as the coals with which it is burned must be fetched from a distance of more than twenty miles by land carriage, or obtained by shipping at the rate of 25*s.* per ton. But where it has been used, it has been spoken of as highly advantageous in improving both the quantity and quality of the grass.

A coarse clay is found in some places, which has been spread with advantage upon the meadow land, improving the soil by giving it a consistency which enables it the better to retain the manure that may be spread upon it.

A *copper mine* has been worked at intervals for many centuries in the mountains at the head of *Connistone* Lake: at present it is discontinued, although lately in the hands of some spirited and intelligent adventurers.

Some of the most considerable quarries of *blue slate* in the kingdom are in this district. It is found in various places in the mountains, and brought down partly by land and partly by water carriage, and shipped either at Penny-Bridge or in the river Dudden.

II. On entering the *limestone district*, a remarkable difference may be observed in the form of the summits of the mountains; those in that which we have just quitted having a sharp serrated outline; while the limestone rocks, lying mostly in horizontal strata, have one that is flatter and smoother, with generally a precipice on one of the sides; and being of a more compact structure, they do not so readily admit the roots of trees to insinuate themselves into

into the mass, as happens with respect to the blue rock. The summits of the higher limestone rocks are mostly destitute of soil, and exhibit a desolate prospect. Fragments of the limestone rocks cover the ground to a great depth below the abrupt faces of these mountains; and the valleys are filled up by a calcareous *sammel*, forming a very compact body similar to that formerly mentioned, (p. 271,) or by the limestone fragments rounded by attrition into the form of pebbles, and mixed with others of whinstone, granite, and gritstone.

The interstices of these pebbles are frequently filled to a considerable depth with a fine mould; so that although the ground may at first sight appear so stony that little vegetation could be expected; yet, under proper management, excellent crops are produced *.

The soil is in general a brown or hazel mould, rather light, but capable of being made productive of the finest grass by the addition of the usual manures. All the poas and white clover are its natural tenants. Rushes, except on the mosses, are rarely seen in this district. A small addition of clay, or argillaceous matter, appears capable of giving it that kind of tenacity which would enable it better to retain any manure which might be spread upon or incorporated with it.

Marl, but especially *shell* marl, is found in sufficient quantity in *Low* Furness, and has been used with success as a manure in particular instances. It has not, however, become a general practice, although it promises to be of the highest utility. A proportion of clay has lately been discovered in some of the beds of *sammel*, which of course constitutes marl; and its effects on the adjoining lands will probably be soon put to the trial.

The west of Ulverstone (which is called *Low* Furness) is a fine champaign country, with little wood, and forms a perfect contrast with the mountainous district to the north-west of it, and is extremely fertile both in arable and pasture ground. From the comparative lowness of the land, the clouds frequently pass over it without depositing their contents, until they are attracted and stopped by the lofty hills.

* In cutting the various canals in this country, a very accurate discovery of the nature and quality of the soil has been made, frequently to a considerable depth; and it appears surprising how thin a stratum of mould on the surface is frequently adequate not only to the mere purposes of vegetation, but to the production of good crops both of grass and corn. In many instances a few inches in thickness of mould are incumbent on immense beds of the most sterile flinty gravel, in a part of the country where this land lets for 5*l.* per acre, (customary measure.)

hills in the interior part of the country; where it happens to be no uncommon circumstance that there is fine weather in Low Furness, whilst they are deluged with rain in the mountains.

A red freestone, composed mostly of fine sand and mica, is found to the west of Ulverstone: and a quarry of excellent millstone grit near Holker, immediately incumbent on the limestone.

Great quantities of *iron ore* are raised a few miles to the westward of Ulverstone, but principally at Whitridge, (which West, in his Antiquities of Furness, calls the *Peru* of that country.) It is found beneath the limestone rock, not in veins but masses; uncertain as to extent, and hence attended with the usual disadvantages incident to mining concerns: it is of the species called by naturalists *hæmatites* or *kidney ore*, (from its resemblance to the kidneys of certain animals.) There are three distinct species found in these mines. The account given by West is as follows: "Iron ore is found at the depth of from twenty to thirty yards: it is raised at 3*s*. 6*d*. and 4*s*. per ton, and pays 1*s*. 6*d*. per ton to the lord of the soil. It is carted and put on board vessels for exportation at 3*s*. and sells from 11*s*. to 12*s*. per ton. Great quantities of *lapis hæmatites* are raised with the ore of Whitridge, which the workmen call kidney and steel ore:—there are two kinds of it turned out with the common ore.

"First. The *lapis hæmatite, boltriodes*, or *glebosas*.

"Second. The convolved kind, described by Aldrovandus and Imperiali: it is the richest ore, and easily distinguished from the *rubrica fabrilis*, commonly called *ruddle*.

"There are other works of the same kind in Furness*."

This ore is of a very greasy and defiling nature, as Mr. Pennant has justly observed, marking every thing it touches with a red stain. The iron produced from it is of the toughest kind, and much valued where that quality is required. It is smelted not only in the furnaces dispersed over the neighbouring country, but exported in great quantities to Scotland and Wales. These mines are of great antiquity, having been worked upwards of 400 years.

Copper ore has been discovered in the rocks at Yealand and Warton, and the veins pursued at a considerable expense; but proving so thin as not to repay the cost, they are at present abandoned.

What is the base of Walney Island, is, I believe, not ascertained.

* Antiquities of Furness, p. 17.

ascertained. West speaks of it thus: "Walney Island lies upon a bed of moss; and all round the island moss is found by digging through a layer of sand and clay which covers it; and in the moss large trees have been found." It is probable, however, as limestone rocks are observed in the adjacent sands, that these extend below this island also.

In common with other limestone countries, there are many subterraneous excavations of considerable extent, such as Dunaldmill Hell-hole, near Kellet, Yardhouse-cave, near Leck, and other smaller ones near Yealand.

III. The district of millstone grit and shale contains almost every possible variety of soil and situation.

To the north and east, lofty mountains form moors covered with ling, which stretch away from the neighbourhood of Hornby, by Wyresdale, towards Rivington Pike. The western part is an uniform flat; and the intermediate country possesses the champaign properties that lie betwixt these two extremes.

The grit rock being almost entirely a flinty substance, its broken-down particles are of all others the least favourable to vegetation. Hence we find that where the rock is merely covered with a gravel composed of such fragments, (which is mostly the case on the high lands towards the moors,) ling, the hardiest of vegetables, can only find a footing; and the scanty soil produced by its decayed remains in the course of centuries, attests the slow progress of vegetation.

Planting with the hardier kinds of trees is perhaps the only improvement this kind of land admits. But it is a point not yet ascertained, at how great a height above the sea trees capable of producing timber will flourish in this country. At considerable altitudes they become dwarfish, and affect the form rather of the bush than the tree.

Where circumstances more favourable for the retention of moisture occur, mosses have been found, which being drained afford a considerable depth of a loose black soil. In many parts, at the foot of the mountains, the whole cultivated country appears to have been originally of this nature, and, being of a spongy texture, is capable of retaining moisture long; and hence, notwithstanding the looseness and lightness which it exhibits under the plough, when allowed to remain in pasture a few years it is apt to be overrun with rushes.

It frequently happens that a coarse clay covers the tops and sides of the hills and moor lands which are of a *secondary* height, which preventing the rain, or the water which flows from the higher grounds, from penetrating beyond a certain depth, such land is overgrown with moss and rushes, forming a perfect sponge, and affording scarcely any vegetation useful for pasturage. But from what has been done to particular spots in the vicinity of this mossy land, we see that by draining, and the usual modes of improvement, such lands are capable of being made very beneficial.

Where, instead of the grit rock, the shale occurs on the surface of the ground, a considerable change may be perceived in the nature and appearance of the vegetables produced ; and this latter, being a particular kind of indurated clay, is easily convertible into excellent soil.

A striking contrast offers itself betwixt the appearance of the vegetation on the mountains and hilly grounds in this district, and that of limestone. Here, they are mostly covered with ling, rushes, and moss ; there, such productions are scarcely seen ; whilst the finest kinds of grass and white clover cover their sides, and are found in the interstices of the rocks to their very summits. The same difference is observable upon passing through this mass of moor land into the limestone country in Yorkshire, which lies to the eastward. This change appears to be occasioned, as well from the decayed particles of the limestone rocks being naturally disposed to furnish a soil more favourable to the growth of nutritious grasses, as from the water and rain finding a more ready passage into the earth, in consequence of not meeting with beds of clay and marl beneath the surface.

In the lower grounds the top mould is mostly of a fine quality. A few miles to the north of Lancaster, the earth begins to have a considerable mixture of clay and marl in its composition, which continues more or less through the whole country to the southward, intermixed with extensive beds of gravel and sand. But from about five miles south of Lancaster the general mass of the country in the lower grounds is, upon digging a few feet, found to be almost entirely marl or clay, incumbent upon the rock or shale which form the basis of the district.

Where these beds of gravel occur, the top soil is in general thin, and requires frequent repetitions of manure to keep it in good condition. Where there is clay and marl beneath, it is generally of an excellent quality, but subject to the common inconvenience of this kind of substratum, the copious growth of rushes.

There is also in this district a considerable quantity of moss

moss land, which lies principally to the west of Garstang, a part of which is annually reclaimed, and converted into ground which produces good crops of corn *.

Besides the common manures, a great part of this district has also the advantage of both marl and lime, and is hence capable of being put into the highest state of cultivation. The parts of the country bordering upon the Bay of Morecombe have also two other kinds of local manures, viz. muscles and sand. The muscles are found in immense beds, which are accessible during the tide of ebb, and they are then carted away in great quantities for this purpose. They are, however, said to be a manure whose good effects are not more durable than a single year. The sand procured upon this coast (from the nature of its component parts) has not only been found advantageous to the stiffer clay lands, but also to those of a lighter texture. Besides the flinty particles usually found in sand, a great proportion of what is got here consists of particles of shells minutely divided, and is hence of a calcareous nature: a portion of clay is also mixed with it, which with some sea-salt †, and the remains of decayed animal and vegetable matters, form altogether a substance very different from common sand.

Some iron-stone is found in the shale. This district, however, is not productive of any ores which have been wrought ‡. The stone which occurs here is however extremely valuable for a variety of purposes. That which is gotten in the neighbourhood of Lancaster, and in Whittle Hills, is of the most valuable kind of freestone; and blocks of almost any size may be procured. From the great quantity of quartz or flinty particles which enter into its composition, the durability will probably be nearly equal to that of granite. Millstones of an excellent quality are gotten in Whittle Hills, and from Kellet Moor; and a fine kind of

* By means of marl, which is generally found beneath the moss, and by *paring* and *burning*, which, although disapproved by lord Dundonald, is here the source of abundant fertility; there being no fear of exhausting the vegetable matter by this process. A particular kind of clogs, called *moss pattens*, are affixed to the hinder feet of the horses employed in ploughing, which prevents them from sinking into the ground, which is frequently so soft as not to be accessible without such assistance.

† When the tide retires, it leaves many miles of these sands dry; and in the summer season the evaporation being considerable, a white crust or pellicle of salt is formed on the surface, from which culinary salt was formerly obtained, by collecting the sand containing this pellicle, and, after dissolving the saline particles in sea water, evaporating the brine, thus made, in pans.

‡ There is some lead ore in Anglezark, belonging to sir F. Standish; and carbonate of barytes.

flags and whetstones from Hutton-Roof, about six miles north-east from Lancaster.

There is a thin seam of coals, which apparently originating in a valuable but limited colliery near Ingleton in Yorkshire, extends by Hornby to within a few miles of Lancaster, when it *bassets*, (in the language of miners,) or runs out upon the common above the town. It is not found in a stratum of argillaceous stone, but (contrary to what occurs in this country, and in general in this kingdom) in a most compact quartzose or flinty one; the particles of which are so compact and hard, as not only to strike fire with steel, but to be almost inaccessible to the tool.

L. *On Smelting of Lead.* *By Mr.* John Sadler*.

Dear Sir, Most of the lead of commerce is obtained from that species of ore which is by mineralogists called *galena, potter's ore,* or sulphuret of lead. Indeed, it is the only species of lead ore which is found in sufficient quantities to be worth working.

There are many other species of lead ore met with occasionally; but these, occurring but seldom, are regarded as curiosities, and are generally carefully selected for the cabinet of the mineralogist, or as ornaments for the mantlepiece of the miner.

The ore, as it is first raised from the mine, is mixed with a considerable proportion of the matrix or gangue of the vein, from which it must be in great measure freed before it is fit for the operation of smelting.

For this purpose, the ore is delivered to the *dressers*, who either break it into small pieces with hand-hammers of a peculiar construction, which are called *buckers*, or it is passed between rollers worked by machinery, or under stampers. It then undergoes the operation of washing, to separate it from the lighter foreign matter, after which it is ready for the smelter.

Construction of the Ore Hearth.

The smelting of lead is performed differently in different districts. In most parts of the North, particularly in Cumberland, Durham, and Northumberland, smelting is performed in the ore hearth by means of bellows. In some parts of Yorkshire, in Derbyshire, and in North Wales, lead

* From Clennell's New Agricultural and Commercial Magazine.

is

is smelted in reverberating furnaces: this kind of smelting is distinguished from the other by the name of cupola smelting: each of these methods has its advocates.

The superiority of either depends much on local circumstances, and, perhaps, also on the skill of the workmen.

Ore-hearth smelting shall be first described.—To render the description intelligible, it will be necessary to commence with a description of the hearth.

Fig. 1, (Plate VIII.) is a sketch of the hearth: it is constructed principally of pieces of cast iron, which are called generally iron stones or metal stones; each different casting has a distinguishing name: they are the (*a*) pan, (*b*) back, (*c*) pipe-stone, (*d*) spark-stone, (*e*) bearers, (*f*) keys, (*g*) fore-stone, and the (*h*) work-stone.

The hearth is erected under a spacious chimney, and nearly in the centre; one side of it is called the water-side, being near the water wheel, which urges the bellows; the opposite is called the land-side.

Figs. 2 and 3 are plans and sections of the ore hearth: the same letters in the different figures are placed to the same parts.—(*i*) the floor of the smelting-house, (*k*) the back of the chimney, (*l*) the front of the chimney, (*m*) the foundation on which the hearth is constructed: it is built of rough masonry, and levelled and run in at the top with thin mortar or grout; the pan or bottom of the hearth is laid steadily in mortar on this bed: upon the posterior part of the pan is placed the back, its face being even with the inner edge of the pan.

The work-stone is next arranged; its upper edge three or four inches from the anterior part of the pan, and parallel with the back; the bearers are placed on the sides of the pan, one end of each butting against the back, the other ends resting on the upper edge of the work-stone. Two thin pieces of stone, (about half an inch thick,) generally slaty sandstone, are laid on the back, and on these is placed the pipe-stone, the inner face of which overhangs the back near an inch. The keys are set on the bearers, their faces even with them; two pieces of brick are set on edge on the bearers, next to the keys, and on these, a few inches from the keys, rests the fore-stone; the spark-stone laid on the pipe-stone completes the hearth.

Before laying the foundation, a large flat stone (*n*) called the cheek-stone is fixed firmly in the ground, and determines the extent of the land-side of the hearth; the spaces between the water-side, the back of the chimney, and the cheek-stone are filled up with pieces of sand-stone, bricks, or old

 iron-

iron-stones, and the interstices levelled up with dust. The fore-stone is wedged tight by its ends, generally against two old keys.

The space between the pan and the work-stone is filled with a mixture of bone and fern ashes well beaten in, and those between the keys and the ends of the fore-stone with stiff clay.

Care is taken in constructing the hearth to lay the bearers square, or at right angles with the back, and also to direct the blast immediately through the centre.

The hearth being completed, the operation of smelting commences with kindling the fire. The whole space between the fore-stone and back is filled with peats or chop-wood: an ignited peat or live coal being placed in the midst, the bellows are set to work: as soon as the combustion is sufficiently advanced, or that the whole are well on fire, one of the smelters (there are two to each hearth) throws a few shovels of half-smelted ore, (the remains of the last operation of smelting,) which is termed brouse, on the top of the fire, gradually adding more as the contents of the hearth settle; he also adds a few small coals occasionally to keep up the combustion: when the whole of the brouse is thrown on the hearth, the other smelter watches out; that is, with a long pointed crow-bar, called a gavel or gable-hook, he stirs up the whole of the brouse, and brings forward a great part of it upon the work-stone: this is effected by introducing the gable-hook into the hearth six different times, in the following order: he first forces it under the brouse a few inches on one side the centre, until the point touches the back; he then forces as low down as he can the end he holds in his hand; this lightens up the contents of the hearth, and as the bar is withdrawn, a part of the hot brouse comes forward on the work-stone; the gable-hook is then entered below the brouse, about the same distance from the centre, on the other side, where the same operation is performed; it is next introduced close to the side of the hearth; here the workman forces the end of the gable-hook from him, at the same time he presses it down, so as to bring the point of the bar into the middle of the hearth; this brings part of the brouse, which was next the side, into the middle, and what was in front, out on the work-stone. The gable-hook is again introduced in the same place, and the point raised close to the side, to remove any brouse that may adhere to the bearer or key. The same operation is performed at the other side, to remove the brouse from thence also. Whilst the watcher is performing his part, the man who supplied the

the hearth, and who is called the setter-on, thrusts his shovel down into the hearth, a little below the entrance of the blast, and forces the brouse sufficiently forward to allow him to place a peat or a handful of chopwood horizontally before the orifice of the bellows: this he generally gets done nearly as soon as the other has finished watching, who changes his gable-hook for a shovel; the setter-on comes to the front with his shovel, and they together throw the whole of the brouse again into the hearth, over the fore-stone, with a small quantity of coal as they see necessary, carefully separating the slags, which they throw into a corner, and breaking down the larger masses of brouse: when the whole is in the hearth, the setter-on goes again to the side, levels the top of the brouse, and covers it with fresh ore, laying this thickest against the spark-stone: the working of the hearth, after watching, is called setting-up. When a hearth is well set-up, and works properly, without an excess of coals or blast, and pretty free from slags, small reddish white flames issue from all parts of the breast, from below the fore-stone, nearly to the edge of the work-stone: these flames should not issue more than a few inches from the breast. The hearth does not continue long in this state; as the peat burns away, the blast is less equally distributed; it forces itself through more in some parts than in others; the covering at the top is perforated, or, perhaps, perfectly ignited, and the whole mass is condensed and settled in consequence of the evaporation of one part of the ore, and the separation of the metal; copious blueish flames issue from two or three parts of the hearth, as if occasioned by the combustion of some metal. The brouse must be again watched-out, a new peat put in, and more ore thrown on the top. The operations of watching and setting-up require to be repeated about every three minutes. After a few times setting-up, the metallic lead begins to flow down the channel of the work-stone, into a pot, where it is kept hot until collected in sufficient quantity to cast a pig.

It is necessary, for the easy management of the hearth, that a considerable quantity of fluid lead should remain in the bottom for the brouse to float on. The watcher, after throwing up the brouse, allows the lead to flow freely down the gutter for a short time, and then prevents any more escaping, by lightly raising up the brouse against the gutter with the corner of his shovel.

Two men will smelt about six bings of good ore a day, and from these produce 24 pigs of lead, weighing 154lbs. each.

It is advisable to draw the hearth at the end of every twelve hours, in order that it may cool; for a cool hearth works pleasanter, and makes better produce than one which has been suffered to heat. The hearth should be drawn about two watchings after throwing on the last of the six bings of ore. As soon as the hearth is watched-out the last time, the action of the bellows is stopped, and the smelters draw out the whole of the hot brouse with their shovels, and throw it on the floor to cool, picking out such slags as they may observe; they also remove whatever adheres to the sides or back.

If the hearth has been properly attended, and a due proportion of fuel used, it will scarcely appear hotter in one part than another; and, if it has been working with a free ore, should not appear hotter than a very dull obscure red heat.

With a free ore, the hearth, when fresh set-up, works as described page 280, the blast finding its way equally through all parts of the breast. The brouse, when watched-out, is dry, and mostly in small pieces, the slags firm, and easily distinguished by their cavernous appearance and brighter colour, and the lead flows from the hearth scarcely red hot. Lead ore, which contains much silver or copper, or which has not been properly cleared from the gangue with which it is mixed in the vein, requires particular attention on the part of the smelter: instead of working dry and open, it becomes soft and pasty; the slag, instead of separating in firm pieces, is diffused through the whole like a half-melted scoria, and the least inattention to the fire will set the whole contents of the hearth into a solid mass, or cause it to boil and flow down in a liquid state on the work-stone—the lead flows very hot, and the hearth appears hot and foul. The addition of lime is necessary to correct this defect in the ore, which combining with the fluid scoria, solidifies, and thus assists its collecting in masses: care should be taken not to add more lime than is absolutely necessary for the purpose intended, as all extraneous matter thrown in with the ore lessens the produce of lead.

[To be continued.]

LI. *Notice respecting Native Concrete Boracic Acid.* *By* SMITHSON TENNANT, *Esq. F.R.S. &c. Communicated by* L. HORNER, *Esq. Sec. of the Geological Society**.

THE boracic acid is not found, like the greater number of substances, in almost every country; but, as far as our present knowledge

* From the Transactions of the Geological Society, vol. i.

knowledge extends, appears confined to a few particular places. On this account, as well as the great utility of borax in various arts, the discovery of its existence in any new situation may deserve to be recorded.

Some months ago Mr. Horner was so obliging as to show me a collection of volcanic productions from the Lipari Islands, presented to the Geological Society by Dr. Saunders. They consisted chiefly of sulphur, and of saline sublimations on the lava; but among these more common substances there were several pieces of a scaly shining appearance, resembling boracic acid. The largest of these had been cut of a rectangular shape, and was about seven or eight inches in length, and five or six in breadth, as if it had been taken from a considerable mass. On one side of most of the pieces was a crust of sulphur, and the scaly part itself was yellower than pure boracic acid. To ascertain if the scaly part was coloured by sulphur, I exposed it to heat in a glass tube; and after the usual quantity of water had come over, there sublimed from it about a tenth of its weight of sulphur, and the remainder was pure boracic acid.

Mr. Horner afterwards informed me, that the late Dr. Menish, of Chelmsford, had presented to the Geological Society a specimen which he had received, with some other volcanic productions, from Sicily, but which had been collected in the Lipari Islands; the box containing them being marked "*Produzioni Volcaniche Raccolte nelle Isole Eole da Gius. Lazzari—Lipari.*" He found it to consist of boracic acid, and it perfectly resembled that I have just described, having the same yellow colour from an admixture of sulphur, and a similar crust of this substance adhering to one side.

Any future traveller visiting those countries would do well to examine them with a view to this particular object. The boracic acid may be a more extensive volcanic product than has hitherto been imagined; for in the account given of its discovery some years ago by Messrs. Hoëfer and Mascagni, near Monte Rotondo, to the west of Sienna, we can have no doubt of its volcanic origin in those places, from the substances which are there described to accompany it.

LII. *Sketch*

LII. *Sketch of the Geology of Madeira. By the Hon.* Henry Grey Bennett; *in a Letter addressed to* G. B. Greenough, *President of the Geological Society, and communicated by him to the Society**.

The following notes were taken during a short stay I made last summer in the island of Madeira. As there appears to be but little known of the structure, or of the phænomena which the strata in that island exhibit, the following observations may not perhaps be wholly unacceptable. They may be considered as furnishing directions to others, where to look for some of the most interesting objects; and may afford to future travellers a small portion of the information which my guide, Dr. Shuter, so liberally communicated to me. That gentleman having long resided in the island, had repeatedly traversed it, and was thereby able to point out to me some of the circumstances which were most worthy of examination, particularly the nature of the various strata that are exposed to view in the deep and abrupt valleys which intersect the island in all directions. These valleys are no less picturesque to the eye of the common traveller than they are deserving of the attention of the geologist. They are in general narrow and deep, the summits of the hills that form their boundaries are broken into peaks rugged and bare, while their sides are covered with the cedar and other trees peculiar to southern latitudes, and with a profuse variety of shrubs and plants, among which the *erica arborea* is the most beautiful, and in the greatest quantity.

The island of Madeira (though I believe it never has been surveyed) is said to be about 50 miles in length, and in its broadest part about 20, but the average breadth does not exceed 15 miles.

It consists of a succession of lofty hills rising rapidly from the sea, particularly on the eastern and northern extremities. The summits of many of these ranges present the appearance of what has been called a table land; yet occasionally the forms are conical, and surmounted by a peak, which in some instances I found to be of columnar basalt. Deep ravines or valleys descend from the hills or *serras* to the sea, and in the hollow of most of them flows a small river, which in general is rapid and shallow. The soil of the island is clay on the surface, and large masses of it as hard as brick are found underneath. Though there are not at present any existing volcanoes in the island, yet the remains of two craters are to be seen, one on the eastern, the other

* From Transactions of the Geological Society, vol. i.

other on the western side, the largest being about a Portuguese league, or four English miles, in circumference. Every thing around wears marks of having suffered the action of fire; yet I was unable to discover any deposit of sulphur, and was told that none had hitherto been found in the island.

The varieties of strata, which I shall term generally lava, are not numerous. I myself saw but four, and I was informed there were no more to be met with. Three of them were invariably alternating in the same order. The first or lowest lava is of a compact species, containing few, if any, extraneous substances, is of a blue colour, and of a remarkably fine grain. Upon that, the second, which is a red earthy friable lava, rests; sometimes separated by beds of clay mixed with pumice, and layers of black ash and pumice. This red lava contains minute pieces of olivine; sometimes it assumes a prismatic form, and in one place was of a moderate degree of hardness: the principal springs of water in the island issue from this stratum. On the top is the third, a grayish lava, generally compact, though at times near the surface very cellular, and containing much olivine. This lava takes principally the prismatic form of basalt. I have seen it in the most perfect prisms from 30 to 40 feet or more in height, the surface being covered with scoria, ash, and pumice. These masses of lava contain more or less of what I consider to be olivine, occasionally carbonate of lime and zeolite, which last assumes either a crystallized or globular form, or is diffused in a thin coating between the different layers.

The fourth species of lava is of a coarse grain, is used for the making of walls, and the commonest and poorest houses are built of it, the blue and gray lavas being used for the copings, &c. It works easier than the two other kinds above mentioned, is more friable and soft, and its colour is a mixture of brown and red. I observed it in a stratum by itself, and it did not seem to have any connexion with the other three kinds.

These are the principal stratified lavas that the island affords; but in the beds of the rivers, particularly in that which flows in the valley of the *Corral,* several varieties occur in isolated masses, containing olivine and zeolite in greater or less quantity, and exhibiting detached portions of strata, similar to those that are found in the *Fossa Grande* on the side of Vesuvius.

In the deep and singular valley called the *Corral,* which I had an opportunity of examining for several miles, the red and gray lava alternated five or six times. The tops of some

of

of its barrier hills are formed of columnar basalt; here and there rising to a peak, or broken into what might be termed a crystallized ridge, or tapering to a point like the granite needles in the *Mer de Glace*. The columnar strata are found here in all directions. They dip usually to the sea, but occasionally are dislocated in the most abrupt manner. Dykes of lava, rising perpendicularly to the horizon, intersect the strata at right angles. I saw one 200 or 300 feet in height, which cut through several of the alternations of the red and gray lava. This valley of the *Corral* well merits the most attentive examination; yet the journey there is one of some labour, and the walk down the river that flows in its bottom so difficult and toilsome, as almost to deter every one from the undertaking. We left the town of Funchal soon after day-break, and did not return till between eight and nine at night, having been, during the whole of that period, in a state of incessant exertion on horseback or on foot. The bed of the valley itself cannot be descended on mules or on horseback. The walk is eight or nine miles in length, and you are compelled to clamber over rocks, as there is not even a track, or wade in the bed of the river, which is rapid, and full of large and pointed stones. Some of the highest hills of the island border on this valley. Several of them rise from the bed of the river in a perpendicular height of 1000 or 1500 feet, judging only by the eye, and are what the French term *taillé à pic*. Others are broken into a succession of steep descents, and are covered with forests of wood and a profusion of plants. Down many there fall small cataracts of water, and some are hollowed into deep recesses, whence issue from the lava numerous little streams that contribute to swell the principal river in the valley.

As you arrive on the brink of the *Corral*, after a ride of about ten miles from Funchal, you find yourself suddenly on the edge of a precipice, near to which a sort of traversing stair-case is cut, with a track winding to the bottom. On the right is a wall of lava nearly perpendicular from 400 to 500 feet in depth, composed of the two species of the red and gray, alternating five or six times, and assuming in its dislocation the form of a bow, both the lavas following in a regular bend the shape of the curve.

On the left of the stairs by which you are to descend, innumerable small columns of the gray lava project from the side: they dip N.W. and their form in general is quadrangular; but I found several of them in prisms of three, five, and six sides. They are remarkably small, and as they

they lie in this bed appear almost all to break off from each other at five or six inches in length, and I never found them exceed this size. They seem to form a dyke that cuts through the horizontal beds of lava.

At the edge of the descent there is a projection or range of basaltic columns, rising like a wall, tapering to the top, and separating into large quadrangular prisms. We found no black ashes in the valley of the *Corral,* though towards the bottom there are considerable strata of pumice, great masses of scoriæ, and cellular lava, and lava in a state of semi-vitrification; the whole presenting evident marks of an eruption, anterior to that which had formed these various strata of lava, which are visible from the summit of the hill to the bed of the river.

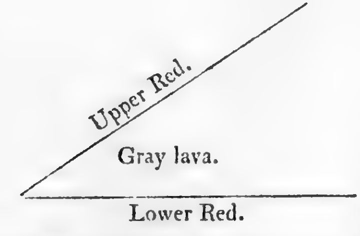
The dip of the strata is in general towards the sea. Basaltic columns shoot from the side of the ordinary strata, which are intersected by various dykes; and one of these in particular swept across both sides of the valley. There are here also rocks of about 100 feet in height, composed of a species of breccia. We examined one near the church, at the extremity of the winding stair-case, forming the descent into the valley, which was composed of large and small pieces of lava, some of them of many yards in length and depth, the angles being rounded, and the whole agglutinated together by a hard black earthy substance, that resisted all the force we could use to break off a piece of it. There are other rocks where the red lava forms the base, and these are soft.

On our road from Funchal to the *Corral* we saw a stratum of large nodules or balls of lava, composed of concentric layers similar to the coat of an onion, and lying one above another; the stratum exposed was 30 or 40 feet in depth, and appeared to go down to the bottom of the hill.

We also examined the coast to the westward of the town of Funchal. From the beach before the town to Illhoo Castle, and beyond it to the land called the *Punta de la Cruz,* the general character of the coast is as follows: The red stone is the apparent base upon which rests a bed of gray prismatic lava, the stratum being sometimes from 40 to 100 feet in depth. At times this gray lava rests upon a deep bed of ashes and pumice, agglutinated together like the *peperino* and *puzzolano* in the vicinity of Naples. The scoria at the surface is remarkably thick, and all the upper parts of the lava appear to be cellular. The general dip of the lava on the coast near Funchal is to the north, but near the fort

fort of Illhoo it forms with a mass of pumice that is intersected with slight veins of carbonate of lime and zeolite, a rapid angle or curve of declination to the east. To the westward of the fort, the lava is not found for a little distance, and there is nothing but deep beds of pumice and the agglutinated mass above mentioned. These beds of pumice are of various thickness, the deepest appearing to be about four feet, and alternating with that stratum which I have called *peperino*. In different cavities of the pumice bed, there are large deposits of black ashes. Towards the extremity of the strata the red stone appears on the surface in a more solid state, and lies in prismatic masses, the prisms being small, and not exceeding a few inches in diameter. Their substance is brittle, and crumbles with ease. This stratum of red lava is of a short continuance. Passing a small brook, it dips rapidly to the westward, and in its place the gray lava is found in a confused though sometimes prismatic form, and rises from the beach, while the red lava still runs along the surface to the height of near 100 feet, the top being covered with a thick scoria.

There is also in the vicinity of Funchal, to the eastward of the town, a fall of water, which, independent of the romantic beauty of the situation, merits being visited on account of the exposure of the two strata of lava in their relative position. The hills are composed wholly of lava, sometimes of a confused, sometimes of a prismatic formation, the red and gray lavas being visible on both sides of the valley. Near the head of it, a short distance from the cascade, the red stratum is at the bottom, and about 60 feet higher it re-appears, and again, about 200 feet higher, alternating with the gray lava. The upper red lava dips rapidly to the south, and the strata are disposed in the following manner:



The rock, down which the cascade falls, is also intersected with a red stratum of about three feet wide, that traverses it, and dips to the westward, and is broken off by a broad dyke

dyke of gray lava. It appears about 30 feet higher, and dips again to the westward. The substance of the red rock in this place is hard, and it breaks into a columnar form, being by far the most compact of the red strata I met with in the island. I saw this red lava also in the island of Teneriffe, to the eastward of Santa Cruz, as well as in the neighbourhood of Orotava.

I have thus endeavoured to give you a slight sketch of that which appeared to me most deserving of attention in the island of Madeira. The short stay I was able to make there prevented a more accurate survey of the island; yet I saw enough to induce me to recommend a careful examination of the strata to those who may have more time than I had to spare, and more knowledge to estimate the value of that which was to be seen. To my mind, the most interesting geological facts are: 1st, The intersection of the lava by dykes at right angles with the strata. 2dly, The rapid dips the strata make, particularly the overlaying of that of the *Brazen Head*, to the eastward of Funchal, where the blue, gray, and red lavas are rolled up in one mass, and lie in a position as if they had all slipped together from an upper stratum. 3dly, The columnar form of the lava itself reposing on, and being covered by, beds of scoriæ, ashes, and pumice, which affords a strong argument for the volcanic origin of the columns themselves: and 4thly, The veins of carbonate of lime and zeolite, which are not found here in solitary pieces as in the vicinity of Ætna and Vesuvius, but are *amid* the lavas and *in* the strata of pumice and tufa, and are diffused on the lava itself, and occasionally crystallized in its cavities.

LIII. *On the Progress and present State of the Practice of Vaccination.* *By* T. Bateman, *M.D.*

The objects which the general adoption of vaccine inoculation will accomplish for mankind, if time and experience shall confirm the promises of its benevolent discoverer, are so important, that every friend of humanity must have followed with anxious hope the progress of the practice, and rejoiced at the general result of the evidence in its favour. It is not easy, indeed, to calculate the sum of human misery that will cease to exist, when the prospect which vaccination holds out to us shall be realized. In its casual, or *natural* occurrence, as it is termed, the small-pox is not only the most loathsome distemper that visits the human frame, but the most fatal pestilence; sweeping off multitudes during its prevalence, and destroying the sight, corrupting the habit, or otherwise inflicting disease on great numbers of those who escape its more destructive effects. The practice of inoculation had, it is true, already diminished those evils among the individuals who resorted to it; but it had unfortunately augmented the evils among the people in general, by the perpetual infection which it disseminated, and the artificial epidemic which it constantly kept up. In London, for instance, during the first thirty years of the eighteenth century, before inoculation could yet have had any effect, the proportionate number of deaths occasioned by small-pox, as stated in the bills of mortality, was about seventy-four out of every thousand: but during an equal number of years at the end of the century, the number amounted to nearly one-tenth of the whole mortality, or ninety-five out of every thousand. So that, as far as we are able to judge from hence, the practice of inoculation, which in itself might be esteemed one of the greatest improvements ever introduced into the medical art, has actually multiplied the ravages of the disease which it was intended to ameliorate, in the proportion of above five to four*. And the extent of the mischief inflicted on the survivors is manifest from a statement published by the Society for teaching the Indigent Blind, that nearly one-fourth of the persons admitted into that charity have been deprived of their sight by the small-pox; not to mention the various forms of scrofula and other diseases which it frequently excites.

It is true, that the more intelligent classes of society, who have generally adopted the practice of inoculation, have in a considerable degree avoided the worst of these consequences of small-pox: they have seldom been deprived of the blessing of sight; and they have only been destroyed by the disease in the proportion of about one in three hundred. But the humane will shudder at the recollection, that this exemption has been obtained at the expense of so much additional misery inflicted on the people at large; and that they have but shifted a part of the evils from themselves, to be aggravated in the families of their less enlightened neighbours; while they perpetuate a plague, which would otherwise have had its periods of absolute cessation.

* See the Tables drawn up by Dr. Heberden, in his "Observations on the Increase and Decrease of different Diseases, &c." p. 36.

Such

Such is the condition in which the most *improved* state of the art of medicine had placed us, before the benefits of vaccination were discovered; and such is the condition to which some persons would advise us to return, in consequence of the alleged insecurity of this preventive. But it would seem to be only necessary to take a clear and dispassionate view of the state of the facts, relative to the efficacy of the cow-pox, up to the present time, in order to be convinced of its incalculable advantages, even were all the reported failures proved to have occurred; nay, if they had actually occurred to double the extent that has been represented. It is the purport of this paper to detail, in as brief a manner as possible, the sum of the facts which have recently been brought to light, and to point out the inference which seems to be justly deducible from them.

The National Vaccine Establishment, supported by parliament, has published two Reports during the present year, containing the evidence which they have collected from various authentic sources. The Colleges of Physicans and Surgeons at Edinburgh, and the Faculty of Glasgow, have again given their decided testimony in favour of vaccination, They assert unanimously, that the practice of vaccination is generally approved of by the profession throughout Scotland; that no bad effects can be ascribed to the practice; and that, since its introduction into Scotland, the mortality occasioned by small-pox has very greatly decreased. The Faculty of Physicians and Surgeons of Glasgow further state, that, since the middle of May 1801, they have gratuitously vaccinated in their Hall 14,500 persons; and that, as far as is known, the "vaccination in all these has succeeded*."

The accounts from several public institutions, in and near London, are equally favourable†. In the Royal Military Asylum for the children of soldiers, where between eleven and twelve hundred are now received, vaccination has been practised since its first establishment in the year 1803. From that period to the present time, but *one* instance of death

* Report from the Vaccine Establishment, 1811.

† 1811. It appears, that since the last Annual Report of the London Vaccine Institution, there have been inoculated by Dr. Walker - - 2,490
From the commencement of the Institution in 1806 - - - 8,595
By the appointed inoculators in the metropolis last year - - 1,046
From the beginning - - - - - - - - - 3,109
By the appointed inoculators in the country - - - - 20,801
From the beginning - - - - - - - - 177,474
Last year, charges of matter - - - 31,992 to 6,539 applicants.
From the commencement of the Institution, 93,080 to 18,900 applicants.

from

from small-pox has occurred; and it is worthy of remark, that the individual had not been vaccinated, in consequence of a declaration of the mother, that he had passed through the small-pox in his infancy. Vaccination was introduced into the Foundling Hospital in the year 1801; and every infant, soon after its admission, has since that period been vaccinated. From the commencement of this practice to the present time, no death has occurred from small-pox; and in no instance has the preventive power of vaccination been discredited, although many children, as a test of its efficacy, have been repeatedly inoculated with the matter of small-pox, and exposed to the influence of its contagion. A similar success has attended the practice of vaccination at the Lying-in Charity of Manchester, where, in the space of nine years, more than nine thousand persons have been effectually vaccinated, and secured from the small-pox. The officers of the Vaccine Establishment in London, through the medium of their correspondence with many similar establishments in the country, have learned, that practitioners of the highest respectability are earnestly engaged in promoting the extension of the practice; that, among the superior classes of the people, vaccination is every where generally adopted; and that, although the prejudices of the lower orders, which have been excited by interested persons, still exist, they appear to be gradually yielding to a conviction of its benefits. This inference is likewise confirmed by the fact, that 23,362 charges of vaccine matter have been distributed by the Establishment to various applicants from all parts of the kingdom, which exceeds by nearly one-third the number distributed in the preceding year.

Of the immense benefits resulting from the universal adoption of vaccination in other countries, the accounts from India have furnished the most interesting example. The number vaccinated in the island of Ceylon, from the year 1802 to Jan. 1810, amounts to no less than 128,732 persons; and the small-pox has literally been exterminated from the island. From the month of February 1808 to the last-mentioned date, the disease had not existed in any part of the island, except in October 1809, when it was carried thither by a boat from the Malabar coast: but, in this instance, the contagion spread to only six individuals, who had not been vaccinated, and was immediately arrested in its progress, and disappeared. The medical superintendant-general observes, that they have no apprehension that the small-pox will ever spread epidemically in Ceylon, while vaccination

vaccination continues to be generally practised; at the same time, that its occasional appearance there has the good effect of proving the preservative power of the vaccine pock, and of rousing the natives from their apathy on the subject. Even the Bramins are now surmounting the prejudices of their education, and submitting to be vaccinated*.

It appears from a Report of the Central Committee of the Vaccine Institution at Paris, published on the tenth anniversary of its establishment, that the benefits of vaccination, in augmenting the population of a country, have not escaped the attention of the present ruler of France, who has formed depôts of vaccine fluid in twenty-four of the principal cities, communicating with the Central Committee at Paris. In some of the departments, it is said, the zeal of the prefects has been such, that there remain none to vaccinate but the infants born in every year, and that the small-pox is already unknown. And the returns of the mortality in the city of Paris, for the year 1809, exhibit only 213 deaths by small-pox. "This number," say the reporters, "though yet too considerable, since the vaccine offered to these 213 victims a certain method of preservation, is yet extremely small in comparison of that of some years, when the epidemic small-pox has carried off, in the same city, more than 20,000 individuals." The Committee, consisting of sixteen of the principal physicians of Paris, express their conviction of the efficacy of vaccination in these terms: "Ten years of labour and success have at length decided the important question, as to the vaccine possessing the power of preserving all those, in whom it has regularly gone through its progress, from the small-pox. This has been carried to such a degree of certainty by the experiments of the Central Committee and its numerous correspondents, as well Frenchmen as strangers, that there is not at present any fact in medicine better proved, or more certain, than that which establishes the truly *anti-variolous* power of the vaccine†."

Such is the result of the progressive experience of professional men, in regard to the efficacy and preventive powers of vaccination: such is the confirmation, which the inferences, drawn from the early investigation of this subject, have received from subsequent and more extensive research! Insomuch, that the conclusion of the College of Physicians upon the subject, in the year 1807, must now be deemed

* See the Report from the Vaccine Establishment.

† A copy of this Report may be found in the Edinburgh Med. and Surg. Journal, for Jan. 1811, p. 117.

 indisputable,

indisputable, that "the truth seems to be established as firmly as the nature of such a question admits*."

The opposition to the practice, which is still but too successfully kept up by a few clamorous individuals in the medical profession, rests principally upon a mistaken view of the nature of the question. It rests upon the notion that the result of the practice should be uniform and invariable; that the *rule* should be void of all *exceptions*. But there is no such regularity in the operations of the animal œconomy: there is no disease without its anomalies; and the diversity of human constitutions is infinite. Several of these anomalies, or exceptions to the general rule, have doubtless occurred in the practice of vaccination; "but," to use the words of a judicious and experienced observer, "certainly not so often as was expected by those who considered the subject from the first dispassionately, nor have they been in sufficient number to form any serious objection to the practice founded on Dr. Jenner's discovery†." In truth, if this principle were received,—that no operation ought to be performed on the human body which was liable to occasional failure,—what medicine would remain for us to exhibit, or what surgical assistance for us to offer?

But let us examine the nature of these exceptions, or "failures," as they have been emphatically called, which have occurred in the practice of vaccination. The very sound of the word excites an alarm in the minds of many persons, as if *failure* were synonymous with *death*, or implied the certain occurrence of a desperate or mortal small-pox. But this is so far from being the case, that upon a deliberate view of the facts, we do not hesitate to affirm, that, if all the cases of alleged failure, which the opponents of vaccination have raked up, upon any sort of evidence, and often upon none, had really occurred, and that number had been doubled or tripled, its advantages over the inoculation of small-pox would still be incalculable.

In the first place, it has been ascertained by the concurring observations of almost all the practitioners who have attended to the subject, that (to use the words of the College of Physicians) "in almost every case in which the small-pox has succeeded vaccination, whether by inoculation or by casual infection, the disease has varied much from its ordinary course; it has neither been the same in *violence*, nor in the *duration* of its symptoms; but has, with very few exceptions, been *remarkably mild, as if the small-pox had been deprived by the*

* See the Report of the Royal College of Physicians on Vaccination, July 1807. † See Dr. Willan's Treatise on Vaccination.

previous

*previous vaccine disease of its usual malignity**." Dr. Willan states, that the feverishness which precedes the eruption in these cases is often considerable, but the pustules are small and hard, containing little or no matter, and begin to dry off on the sixth day†. It must not be omitted, indeed, that in a very few instances the small-pox subsequent to vaccination has assumed the confluent form, and put on a dangerous aspect (as in the recent case of the son of earl Grosvenor); but even in these rare instances, the modifying influence of the previous vaccination has been manifest, the disease, when near its height, receiving a sudden check, and the recovery being unusually rapid ‡. One case of this sort occurred to the observation of the writer of this paper, in which, on the seventh day of confluent small-pox, the child became suddenly free from constitutional complaint, and ran about at play; a circumstance, he believes, that is never known to occur in confluent small-pox where the previous influence of vaccination had not been exerted. In this statement, then, we have admitted the worst consequences that have ever accompanied the "failures" of vaccination, in any one instance.

But, in the second place, let us attend to the proportionate number of these failures. "It does not appear," says Dr. Willan, who minuted the cases as they happened, "that failures in the preventive effect of vaccine inoculation, including *mistakes*, *negligences*, and *mis-statements*, have occurred in a greater proportion than as *one* to *eight hundred* §." It is very improbable, then, that the actual failures amount to one in a thousand, or to any thing near that number. But let us suppose, for the sake of argument, that the failures amount to the proportion of one in five hundred; that is to say, that one of every five hundred persons vaccinated remains liable to be infected by small-pox: and let us further imagine, that this subsequent small-pox is not mitigated in any case, and therefore, that (as in the case of the ordinary *natural* small-pox) *one* in *six* of these will die. Then the worst result would be, that *one* out of every *three thousand* persons vaccinated would die. But we know, that *one* of *three hundred* persons, who receive the small-pox by inoculation, perishes of that disease ‖. The conclusion is therefore obvious, that the worst result that could be calculated upon from vaccine failures, would leave

* See the Report of the College. † See his Treatise, sect. iv.

‡ See the last Report of the National Vaccine Establishment, July, 1811.

§ See his Treatise, p. 23. ‖ Dr. Willan states, that "the inoculated small-pox still proves fatal in *one* case out of *two hundred and fifty*.—Ibid.

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the balance in favour of vaccination, in the proportion of *ten* to *one*. But when we consider the actual state of the circumstances;—that the number of deaths from inoculated small-pox really exceeds the number of "failures" of vaccination;—that these "failures" are, in a great majority of instances, the means of insuring a very mitigated and harmless small-pox;—and that they have, perhaps, in no instance, been followed by a fatal small-pox;—the chances of fatality from a failure of the vaccination are so trivial as to elude calculation; and the only chance of injury that ensues, is reduced to that of a temporary inconvenience.

Lastly, let us reflect on the non-contagious nature of the vaccine disease, which, while it secures the individual from blindness, deformity, or fatuity, too often consequent on the small-pox, injuries no one, and spreads no epidemic around, and we shall be compelled to admit, that, "with all its imperfections on its head," with a frequency of failure that its opponents have never yet ascribed to it, vaccination would still prove a blessing, such as few individuals have had the happiness to confer upon mankind.

We might here have terminated our observations, but the leading circumstance, communicated in the late Report from the National Vaccine Establishment, demands some notice. It is singular, that at the time when the public attention was attracted by the occurrence of small-pox after vaccination, in the sons of the earl of Grosvenor and sir Henry Martin, the second occurrence of small-pox in the rev. Joshua Rowley, miss Booth, and two other persons, should have happened. In three of these cases, the previous small-pox had been taken by inoculation, and in the fourth, in the natural way. But the truth is, that the small-pox itself, in whichsoever of these two ways it is produced, is liable to the same anomalies and exceptions as the cow-pock. There are several examples of the fact on record; one of the most striking of which is the case of Mr. Langford, related in the 4th volume of the Memoirs of the Medical Society of London. This person was so "remarkably pitted and seamed" by a former malignant small-pox, "as to attract the notice of all who saw him:" yet he died at the age of fifty, in an attack of confluent small-pox, in which he communicated the infection to five other individuals of the family, one of whom also died. It will be unnecessary here to detail the various examples which authors have described. The writer will just notice an instance which occurred under his own observation not long ago,

ago, the particulars of which will be detailed in the second volume of the "Medico-Chirurgical Transactions" about to be published*. This occurred in a woman of 25 years of age, who was considerably pitted by a former confluent small-pox, which she had suffered in her childhood. She caught the second disease, which went through the usual variolous stages in a mild way, by nursing her infant under a confluent small-pox, which proved fatal to it. It is remarkable, that her two elder children, who had been vaccinated a few years before, lived in the same apartment, during the progress of the small-pox in the infant and mother, and escaped the infection; the cow-pock in them having exerted a preventive power, which the previous small-pox had failed to effect in the mother. The poor woman had been prevented, by the terrors excited by the anti-vaccinists, from vaccinating her youngest child: a fact which should induce these opponents of the practice to reflect on the serious responsibility which they assume, in thus discouraging the adoption of this important preventive.

I am, &c.

Bedford Row, August 19, 1811. T. BATEMAN, M.D.

LIV. *Notice respecting the Decomposition of Sulphate of Iron by Animal Matter. By* W. H. PEPYS, *Esq. F.R.S. Treasurer of the Geological Society*†.

As the following circumstance, that took place in my laboratory, appears to throw considerable light on the mode whereby organic remains become penetrated by pyrites, it may not perhaps be foreign to the objects of the Geological Society, and as such, I have taken the liberty of offering it to their attention.

I was engaged a few years ago in a course of experiments on hydrogen gas, which was procured in the usual method, by the solution of iron turnings in diluted sulphuric acid. The sulphate of iron hence resulting, to the amount of some quarts, was poured into a large earthen pitcher, and remained undisturbed and unnoticed for about a twelvemonth. At the end of this time, the vessel being wanted, I was about to throw away the liquor, when my attention was excited by an oily appearance on its surface, together with a yellowish powder, and a quantity of small hairs.

The powder, on examination, proved to be sulphur; and on

* Several cases and many references will be there found, which are omitted here for the sake of brevity.

† From the Transactions of the Geological Society, vol. i.

on pouring off carefully the supernatant liquor, there was discovered at the bottom of the vessel a sediment consisting of the bones of several mice, of small grains of pyrites, of sulphur, of crystallized green sulphate of iron, and of black muddy oxide of iron.

These appearances may with much probability be attributed to the mutual action of the animal matter and the sulphate of iron, by which a portion of the metallic salt seems to have been entirely deoxygenated.

LVI. *On the Staphyloma, Hydrophthalmia, and Carcinoma of the Eye.* By JAMES WARE, *Esq., F.R.S. and Vice President of the Medical Society**.

ALTHOUGH it be too often the melancholy province of medical men to witness disorders which cannot be removed, and in the treatment of which the utmost exertions of their art can only produce a mitigation of the symptoms,—surgery nevertheless, besides affording in this way considerable benefit to mankind, is often highly serviceable in various diseases, by preventing the occurrence of greater evils than those which have already taken place. This observation is strikingly exemplified in those disorders of the eye to which the attention of the society is now requested ; for though all of them have irrecoverably destroyed vision, yet the staphyloma and hydrophthalmia indispensably require an operation that shall cause the eye to sink in the orbit, in order to obviate constant pain and uneasiness; and the carcinoma of the eye is only capable of receiving a check, in its tendency to destroy life, by the complete extirpation of that organ.

The term staphyloma is sometimes used to designate the protrusion of a part of the iris through a wound or ulcer of the cornea. This is perhaps its most correct meaning, the Greek word, from which it is derived, implying similitude to a raisin or dried grape. But various authors have also used the term to denote a projecting opaque cornea ; and in this sense I propose more particularly to employ it at this time. When the projection is very considerable, the disorder is sometimes also called proptosis ; and in those cases where the projection is not confined to the cornea, but occupies also a portion of the sclerotica, as sometimes happens, this latter appellation is peculiarly appropriate.

It has been disputed by authors, whether the projection of the opaque cornea, in the staphyloma, is occasioned by a

* From the Transactions of the Medical Society of London, vol. i. part i.

thickening

thickening of this tunic, or by a morbid accumulation of aqueous humour behind it. I believe, in general, both these circumstances combine to produce the disorder; the cornea becoming not only opaque, but both softer and thicker than in its natural texture; and in consequence of this, the aqueous humour behind the cornea pushes it forward, and thus enlarges the anterior chamber of this humour. I have sometimes seen the whole cornea sloughed off during an acute purulent ophthalmy, and a white opaque substance gradually effused from the ulcerated surface, sufficient to form a complete cover to the iris; after which this opaque body has gradually projected in a conical shape, until at length it has become so prominent as to hinder the eyelids from closing over it. I have at other times seen the projecting cornea partly opaque, ond partly transparent; the pupil being distinctly visible through the transparent part, but the power of vision wholly destroyed. Sometimes the circumference of the opaque cornea projects, its central part appearing depressed, and resembling the bottom of a plate or dish; and sometimes, near to the centre of the opacity, in the case last mentioned, there is an irregular black appearance, which a cursory observer might mistake for a pupil. No part of this aperture, however, is perceptible on a careful inspection, and the eye of course is deprived of all useful vision *.

So long as the projection of the opaque cornea can be covered by the eyelids without painfully stretching them, if it be not accompanied with an irregularity in the surface of the cornea, and the sight of the other eye continue perfect,

* The cornea not unfrequently projects without losing its transparency, assuming a conical shape instead of that which is spherical; in consequence of which change the eye at first becomes myopic; but when the projection is more advanced, it causes so unequal a refraction in the rays of light as to destroy correct vision. In cases of this description I have repeatedly discharged the aqueous humour, and endeavoured afterwards, by moderate pressure, to prevent the return of the projection; but on the reproduction of the aqueous humour the conical projection has always reappeared. If only one eye be thus affected, the sight of the other remaining perfect, all the purposes of vision will be obtained from this alone; but, if the cornea of both eyes be conical, much advantage may be obtained from wearing spectacles the rings of which are filled with an opaque substance that has a small hole in its centre, not more than the tenth or twelfth part of an inch in diameter, the smallness of which aperture, by lessening the pencils of the rays of light, will prevent the confusion that must otherwise be occasioned by their unequal refraction. Persons who have a projecting cornea should be particularly careful to avoid blows on the eye; since the projection is usually accompanied with a preternatural thinness of this tunic, which renders it easily ruptured: when this happens, the iris is liable to be involved in the wound, and the sight to be more or less injured by the derangement that takes place in the figure and size of the pupil.

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the only inconvenience the projection occasions is produced by the unseemly appearance it presents to observers. This may in some degree be prevented by wearing a pair of spectacles containing plain window glass in the ring opposite the sound eye, and glass that is ground in a slight degree opaque, or even similar plain window glass, in the ring opposite the affected eye. In some instances, however, a consciousness of the appearance produced by a projecting opaque cornea has occasioned so much distress of mind, that I have been requested to sink the eye, solely for the purpose of getting rid of the deformity. I wish I could say that milder means have been found sufficient to accomplish the object. Various applications have been proposed for this purpose at different times by different authors. By some, strong caustics have been recommended for the express purpose of producing an excoriation, and even an ulceration, on the surface of the projecting substance. Both Janin * and Richter † have said that they not only removed the projection of an opaque cornea, but even reproduced its transparency, by the application of the butter of antimony. Janin has recommended this application, for the purpose also of removing that other species of the staphyloma, in which there is a protrusion of part of the iris through an ulcer of the cornea. But I beg leave to observe that caustic applications of every kind should be used with great caution in all diseases of the eye. I have known them occasion violent and long-continued inflammations; and, so far from reproducing vision, they have very rarely reduced the prominence of the staphyloma so as to preclude the need of other means to take away the deformity. Scarpa, in his chapter on the staphyloma, expresses himself in a similar way; and has adduced several cases of this disorder in children, in whom an ulceration on the surface of the cornea was kept up by escharotic applications several weeks, and yet no diminution was obtained by it, either in the projection or opacity. If such be the result of the experiment on the eyes of children, it certainly is less likely to succeed on those of adults. The other mode which has been proposed by authors, viz. that of compressing the tumor, and thus restraining it from interfering with the motion of the eyelids, is so difficult to be accomplished with the necessary accuracy, that I remember only one case in which it afforded any advantage. In this instance a poor man who had a staphyloma of one eye many years, and

* Janin sur l'Œil, sect. 8, page 389 et sequent.
† Richter, fasciculus 2, page 105 et sequent.

could

could not be prevailed on to submit to have the eye sunk, was kept easy by wearing a bandage round his head, not unlike to the spring truss that is used for an inguinal hernia. The bolster of the instrument made a pressure on the outside of the eyelids, which kept them constantly closed, and hindered the eye from moving. In consequence of this, the projection gave no pain; and, by the aid of the other eye, the patient was enabled to work at a common handicraft business without inconvenience.

The more direct way of affording relief in the staphyloma is by removing the whole of the projecting substance; in consequence of which the humours of the eye are discharged, and the posterior part of its tunics collapse, so as to form a kind of button at the bottom of the orbit. On this button, when the wound is healed, an artificial enamelled eye is capable of resting; by which the uniform appearance of the face may be restored. Authors are not agreed on the best mode of performing the operation. Heister, St. Yves, and others, have proposed to pass a double ligature through the middle of the tumor, and then to separate the threads, and tie the tumor on each side, so that the compression made by the ligature may cause it to mortify and slough off. But this is so painful, and so indirect a mode of accomplishing the object, that I believe it has not been practised for many years. Scarpa, in more modern times, has recommended to us to remove a small portion only of the projecting cornea (agreeable to a mode first proposed by Celsus in his book De Medicina, lib. vii. cap. 7*), and to force out the crystalline and vitreous humours through the opening; after which, he says, the wound will close, and the tunics of the eye collapse to a small size, without occasioning any considerable degree either of pain or inflammation. This mode of performing the operation appears to me, however, to be liable to considerable objections. If the opening in the cornea be not larger than the size of the crystalline humour (which not unfrequently, in cases of the staphyloma, is without disease), this humour, in passing through the aperture, is very liable to bruise the iris, and to bring on pain and inflammation, that are both violent and tedious; and if, on the contrary, the opening be so large as to allow the crystalline and vitreous humours

* The words of Celsus are, "in summa parte ejus ad lenticulæ magnitudinem exscindere," Scarpa proposes to make an opening "two, three, or four lines in diameter, according to the size of the staphyloma;" but the largest of these dimensions being only one third of an inch, is barely sufficient to allow the crystalline to come through it, without forcibly compressing the iris.

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to be discharged, without doing violence to the iris, though the pain and inflammation consequent on the operation may not be considerable, yet the place of the evacuated humours will be supplied by a watery humour, which will speedily distend the tunics of the eye to their former size, will do away the possibility of inserting an artificial eye, and will hazard the return of all the old symptoms. Scarpa, aware of these circumstances, mentions expressly, that he has been obliged to irritate the wound three or four different times, after the operation, in order to bring on a sufficient degree of inflammation to cause the eye to collapse. Influenced by these considerations, I have never performed the operation according to this method; and having uniformly succeeded in a considerable number of cases, during a practice of more than thirty years, by performing it in the following manner, I trust that I am justified in recommending my mode of operating to the attention of this Society.

The operator will find it more convenient to stand behind the patient than before him; and the patient should be placed on a chair sufficiently low to allow the operator to carry his hand with ease over the patient's head. A large crooked needle, armed with a strong thread, should then be passed through the opaque projecting cornea, and, after separating the needle from the thread, a knot should be tied in the latter, at a small distance from the eye, in order to hinder the thread from slipping. The operator having thus obtained by means of the thread a secure hold of the eye, a knife similar to that which is used to divide the cornea in extracting the cataract, or, if this be not at hand, a long sharp-pointed lancet, should be pushed through the sclerotic coat, about a quarter of an inch from its connection with the cornea, and be carried quickly but accurately round the cornea, as nearly parallel to it as can be accomplished. Sometimes, as soon as a puncture is made through the sclerotica, so large a portion of the vitreous humour escapes, as to cause the cornea to become flaccid; in consequence of which the operator may find it difficult to complete the incision round this tunic with either the lancet or the knife; and in this case a curved blunt-pointed scissars will be found useful to finish the operation. The only objection to the use of the scissars is drawn from the additional pain which it is supposed to give; but the duration of the operation is so short, that the difference between the pain produced by the instruments is scarcely worthy to be named. The hæmorrhage that succeeds is seldom considerable;

derable; and the less the eye is examined afterwards, the less danger will there be of pain and inflammation. A compress wet with a saturnine lotion should be applied over the eye, and it should be moistened with this liquor, without being removed, as often as it becomes dry; but no lint or any other application should be put within the lids, since this has been known to give great pain, and in one instance to occasion alarming symptoms. An anodyne should be given after the operation, of greater or less strength according to the age of the patient; but it is seldom necessary to repeat this medicine, since the patient has usually more sound and quiet sleep after the operation than he had for a long time previous to its performance. At the end of about a fortnight, that part of the sclerotica which remained in the orbit will be found to have collapsed, and sometimes a small fungous substance will then protrude through the wound. This in the course of time would subside of itself; but, as the delay may be irksome, the fungus may be easily removed, and with very little pain, by snipping it off with a pair of sharp scissars. The fungus is usually smaller in its neck where it joins the sclerotica than in its top; in consequence of which its removal is effected with very little difficulty; and though it sometimes reappears, it may be snipped off again and again, until at length the wound will completely close, the inflammation cease, and the orbit become fit to receive an artificial eye. This, however, ought not to be introduced until the inflammation be perfectly removed; and when such an eye is used, it is advisable to withdraw it every night and replace it in the morning, which may be effected with ease by the patient himself, after a short experience. In the choice of the artificial eye, it is not only important that the colour of the iris resemble accurately that of the sound eye, but the size of the eye should be well adapted to that of the orbit, and the dimensions of the cornea be rather smaller than that of the natural eye. If these rules be not regarded, the artificial eye will give an unsightly stare to the countenance; it will not move, as it ought to do, in unison with the sound eye; and it will be liable to occasion both pain and inflammation. It is of consequence also to know that an artificial eye is apt to irritate after it has been used about a year and a half or two years, and must then be either disused entirely, or its place be supplied by a new one: and it may not be improper to remark, that when an eye has been sunk, if an artificial eye be not introduced, the appearance of the countenance may be much improved by wearing a pair of spectacles with either plain

window

window glass in the circles, or glass that is tinged in a slight manner with a green or blue colour. The reflection from the glass in the spectacle frame will prevent the deficiency from being noticed, or will only give rise to the supposition of the eye being weak.

[To be continued.]

LVI. *Facts relating to the Nautical Almanac and the* Connoissance des Tems.

It is well known that the Nautical Almanac has contributed more essentially to the improvement of navigation and of practical astronomy in general, than any work of the kind ever published. It was begun in the year 1767, and has been continued up to 1816 inclusive, making in the whole 50 volumes.

This most important work was planned, and has been constantly conducted, by the Rev. Dr. Maskelyne, the late Astronomer Royal, whose name must for ever stand high in the annals of science, both as a profound mathematician and a most accurate and able astronomer; and particularly as the founder of the lunar observations, by which the longitude at sea is now accurately determined; and for this great national purpose the Nautical Almanac was established. The astronomers of France have since modelled their *Connoissance des Tems* on the plan of this publication, and they have been suspected of copying many of its most valuable and laborious calculations, although they pretend that all their articles are from original computation. A recent discovery, however, seems to remove all doubt on the subject. The Nautical Almanac has been always conducted with such accuracy, that there is no instance of any error of importance having been discovered in it. A trivial exception, however, has lately occurred. It has been observed that in the volume of the ensuing year, 1812, the obliquity of the ecliptic is assumed about 9 seconds too little, which has probably originated in making a double correction, in reducing the mean obliquity to the true; which inaccuracy pervades all the columns of the sun's right ascension and declination.

This error was discovered by Dr. Kelly of Finsbury Square, who took an early opportunity of shewing it to Mr. Pond the Astronomer Royal; and that gentleman, after examining the Almanac, and comparing it with others, agreed as to the existence of the error and the propriety of having it corrected. Dr. Kelly mentioned it to other scientific

entific persons, among whom was the Earl of Rosse: and this communication led to a correspondence between the Lords of the Admiralty and the Astronomer Royal; the result of which is understood to be, that the Almanac is to be corrected in the second edition, which it is expected will be wanted in the course of the year. This delay will not be attended with any inconvenience to seamen, as the error is too delicate to affect their calculations. It is only perceptible to Astronomers at land, and especially in Observatories; and they may be supposed able to correct for themselves.

Dr. Kelly is now computing tables of the sun's right ascension and declination, for the use of his own Observatory; and he will probably publish them in the beginning of the year, if others are not brought out before that period. In this operation he avails himself of the improved Solar Tables in Mr. Vince's third volume of Astronomy, which were not published when the Almanac of 1812 was printed; but the effects of their superior accuracy may be observed in all the Almanacs that follow.

It is no way extraordinary that this mistake in the Nautical Almanac should have escaped the notice of our Computers and Examiners of the longitude, or of our other Astronomers, as their attention might not have been immediately directed to the subject. It seems, however, a little curious that the American Astronomers, who have printed this volume with additions and pretended improvements, should have left the error in question with all its consequences wholly uncorrected. But the most extraordinary circumstance relating to this mistake is, that the French have actually copied it into their *Connoissance des Tems.* They have, it is true, given the obliquity of the ecliptic correctly in their fifth page; but in the columns of right ascension and declination of about nine months, they have inserted our inaccuracies (only making their usual allowance for the difference of meridians); and even at the solstitial points, where the mistake is most obvious, they have copied it exactly, and have thus made the sun's greatest declination 9 seconds less than the obliquity of the ecliptic in the fifth page, though both should be the same.

If any thing could add to the fame of Dr. Maskelyne, it is the entire confidence thus placed in his calculations by the great Astronomers of France. It is, besides, highly honourable to his memory, that in the Nautical Almanacs of half a century, only one error should be found, and even

 this

this too small to be felt in nautical practice. It is a curiosity in science! and it is likewise worthy of being recorded as a most gratifying instance of the powers and persevering energies of the human mind.

LVII. *Proceedings of Learned Societies.*

LITERARY AND PHILOSOPHICAL SOCIETY, HACKNEY.

The first year's Report of this Society has just reached us; and it is but justice to the members to observe, that they have shown commendable zeal and assiduity in promoting the objects for which the Society was instituted.

The Report, after stating the regulations of the Society, presents an account of its origin, establishment, and labours.

At the meetings, (held the first Tuesday of every month,) besides routine business and conversations on objects connected with the pursuits of the Society, the following papers were read:

On the Process of Tanning in England; and the Mode of Rearing Black Cattle in South America: by Mr. David Booth of Newbuigh, in Fifeshire, author of an Introduction to an Analytical Dictionary of the English Language.

An Account of the Wahabees, a Sect of Mahomedans, generally accounted Deists under the Faith of the Arabian Prophet: translated from the French by Mr. John Ellis junior.

On the Advantages of Mutual Intercourse among Literary Institutions: by Mr. Clennell.

A Literary Portraiture of France in the eigheenth Century: a Translation from the French by Mr. William Fox jun.

A Translation of part of the Preliminary Discourse to the Account of "Arts and Manufactures," in the *Encyclopédie Méthodique*, by Mr. John Peters.

Observations made in visiting a large Copperas Work in Northumberland: by "A Friend to Science and the useful Arts:" together with some Account of Muriatic Acid, drawn up by Mr. John Sadler.

On the Quadrature of the Circle: by Mr. James Clark of Newport, Isle of Wight.

A Memoir of the Life of the late Professor Beattie of Aberdeen; drawn up for the Society by an intimate friend of the Professor; with some Introductory Observations by Mr. Clennell.

Several of these papers have since been published in the New

New Agricultural and Commercial Magazine, conducted by Mr. Clennell, one of the Secretaries of the Society—a practice which we hope to see continued, at least till the Society may think it necessary to publish their papers in regular volumes.

LVIII. *Intelligence and Miscellaneous Articles.*

THE COMET.

On the Comet. By Mr. Firminger, *late Assistant Astronomer at the Royal Observatory, Greenwich: in a Letter to the Editor.*

Sir, Having always considered the Philosophical Magazine one of the first channels of periodical scientific information, it has given me great pleasure in being able from time to time to add to its valuable contents, either by registering in it such productions as were my own, or those of my friends, (of whom I had permission,) as appeared to me worthy of public attention.

In the month of February last, I noticed, when giving an account of the position of the newly discovered planets Ceres, Pallas, Juno, and Vesta, the valuable observations made on Ceres at its last opposition, (which happened about that time,) by Stephen Groombridge, esq. of Blackheath. The same gentleman has done me the honour of communicating to me his equally valuable observations on the comet which now graces with splendour the concave vault of the starry hemisphere that surrounds us, and which has been for some time past an object of general interest and admiration. The observations with which I have been favoured, were not reduced, owing no doubt to the multiplicity of objects in which Mr. Groombridge is continually engaged. I have therefore, in uniformity with the valuable observations you lately published, added the latitude and longitude of each observation, these being the parts useful to astronomers, either for determining the elements of the comet's orbit, or for comparing such elements, when obtained, with its actual situation. The elements which have been given by M. Burckhardt, and which are found to agree with subsequent observations, enable us to represent the position and motion of the comet as seen from the earth, in a very clear and interesting point of view. I have lately made for the use of my pupils * a model, in which the vari-

* Mr. Firminger gives private lessons in Astronomy, Geography, Mathematics, and the various branches of Natural Philosophy.

 able

able situation of the comet, and the course it has moved over since its first appearance, are at once comprehended by persons almost wholly unacquainted with the most simple and popular principles of astronomical phænomena: a drawing and description of this cometarium, with the method of laying down the place of the comet upon its orbit, will be the subject of a future communication. As the perihelion distance of the comet is a little greater than the earth's mean distance from the sun, its motion at its perihelion is only about one and half time the earth's mean motion in its orbit; on which account, notwithstanding the earth and comet are now moving in almost opposite directions, its disappearance will be very gradual, and it may be expected to adorn our hemisphere for six weeks or two months longer before it vanishes to the eye unassisted by the telescope. With the telescope it may probably be traced till nearly the end of January, when it will again be so near the sun as to be lost in his beams; and as its descent below the plane of the earth's orbit will take place about the beginning of April, just after its conjunction with the sun, and being at that time between three and four times the distance from us that it is at present, it will be too faint to be any longer visible; so that we may conclude that its disappearance to the inhabitants of this earth will be about the middle of January 1812.

OBSERVATIONS OF THE COMET.

1811.	*M. Time.*	*A.R.*	*Decl N.*	*Long.*	*Lat. N.*
Sept. 8	$9^h 40^m$	162° 32′ 37′	41° 12′ 35″	4^s 26° 48′ 52″	30° 37′ 41″
12	10 6	167 15 4	42 5 48	5 0 2 25	33 20 55
Oct. 2	10 34	204 6 20	49 31 51	6 5 45 46	53 27 20
12	11 24	229 39 28	46 59 26	6 22 39 2	61 19 43
15	10 50	236 46 9	45 11 30	7 3 45 59	62 19 1
17	11 23	241 26 54	43 43 46	7 11 37 51	62 29 11
23	11 52	253 48 10	38 44 10	8 3 34 48	60 43 27

From the reduced geocentric latitudes it will appear that the comet made its nearest approach to the earth between the 17th and 23d of October. On the 25th, about half-past six o'clock in the evening, the comet was so near to the star marked π 67 Herculis, that this star appeared to the naked eye to be the nucleus of the comet: the phænomenon must have been highly interesting to gentlemen who happened to view it with a good telescope. To me it appeared that the star had actually suffered an occultation; but being in a situation where I had no telescope at hand, I was unable to make any further observations upon it.

Somers Town, Oct. 26, 1811. THOMAS FIRMINGER.

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The following observations on the comet were made at Gottingen, and published there on the 20th of September: "The comet which is now visible on the horizon in the northern part of the heavens, is one of the most remarkable which has ever been observed. None has ever been so long visible, and, consequently, none has ever afforded such certain means of information with respect to its orbit. Accordingly, since the end of March last, when it was first perceived by M. Flauguergues in the south of France, its course has been regularly traced; nor shall we lose sight of it till the month of January 1812. Its train, which occupies a space of 12 degrees, exhibits several curious *phænomena.* It is not immediately connected with the comet, as if it were an emanation from it, but forms, at a distance from the *nucleus,* a wide belt, the lower part of which girds without coming in contact with it, much in the same manner as the ring of Saturn; and this belt extends itself in two long luminous *fasces,* one of which is usually *rectilineal,* while the other, at about the third of its length, shoots forth its rays with a slight curve like the branch of a palmtree; nevertheless this configuration is subject to change. It has been observed that the space between the body of the comet and its train is occasionally filled, and of the two *fasces,* that which is generally *rectilineal* sometimes arches its rays, while those of the other assume the form of right lines. Finally, rays, or, as it were, plumes of ignited matter, have been seen to issue from the lower extremities of the *fasces* or flakes, and again unite.

"Such fluctuations and accidents in that sort of luminous atmosphere which must occupy in the regions of space a scope of about eight millions of leagues, are immense, and may well impress the imagination with astonishment. The celebrated astronomer of Lilienthal, Mr. De Schrœtter, remarked variations of the same kind in the tail of the last comet of 1807, and inserted, in the work he published with respect to it, plates of the successive configurations.

"Professor Harding has also observed and delineated with care the present comet under its various aspects, and his design will appear in one of the succeeding numbers of the 'Geographical and Astronomical Correspondence,' edited at the observatory of Gotha by the chamberlain De Lindenau.

"They will show that when the comet first appeared, and was yet at a distance from the sun, the two flakes of its train were separated so as to form a right angle; but as

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that distance decreased, they approached each other till they became parallel.—This phænomenon, however, may be nothing more than an optical illusion.

"As to the *nucleus*, or the comet itself, it has been found impossible, as yet, even with the aid of the best telescopes, to make observations on its disk, as on that of a solid body and of determined circumference. We can only discern a vague circular mass, more luminous than the train, particularly towards the centre; but the verge of which is doubtful, furnishing to the eye no fixed line of demarcation.

"This mass is without doubt composed of a very subtile substance, as is probably that of all comets. This hypothesis receives much support from the fact, that one of these stars, of very considerable magnitude, (the first comet in 1770,) passed and re-passed through the very middle of the satellites of Jupiter without occasioning amongst them the slightest disorder. There is every reason to believe, that the *nucleus* of the present comet is nothing more than a conglomeration of vapours of very little density, so little perhaps as to be transparent. Whether this be the case or not, might be easily ascertained, if those who are in the habit of observing it would watch the moment of its *transit* athwart the disk of some star, the rays of which would have sufficient power to perforate it, if transparent. Such a body might very possibly be an incipient world, just past its *gaseous* state, and which was to derive solidity from the precipitation and condensation of the matter surrounding it. The successive observation of some comets, in which it may be possible to distinguish the different stages of chaos and progresswe formation, can alone furnish any knowledge with respect to this point." *Moniteur*, 4th October.

To Mr. Tilloch. Glasogw Observatory, Oct. 7.

Sir,—I hope the following facts relative to the comet will not be unacceptable to your readers:

Since my communication to you of the 4th, relative to the comet, announcing the determination of the elements of its orbit made at this establishment, I am happy to perceive in the London papers which arrived to-day, the result of Burckhardt's second approximation. The talents of this gentleman as a computer are well known, and highly appreciated by the learned world. Between his time of the perihelion passage and ours there is a difference of no more than three days, and the whole period of the comet's revolution, I am satisfied, exceeds considerably 100 years. It is to be remarked too, that Burckhardt never ventured to give

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to the public his first trials; and therefore, whatever differences exist between his numbers and ours may have been obtained at his second calculation. The inaccuracy of the first he expressly admits in his letter to the editor of the *Moniteur*, which begins in the following manner: "Having been requested to correct my first determination," &c. I wish it to be understood, however, that the appearance of his statement has not shaken, in the least degree, the confidence I humbly conceive due to our own results. The observations from which these are derived were performed with the instruments of Troughton; instruments unquestionably superior to any other in the world. But we have still more direct assurance of the accuracy of our observations, by comparing them to the numbers which have been published from the highest authority (the astronomer royal) in the Philosophical Magazine of last month. The longitudes of the comet, determined at Greenwich and Glasgow Observatories, coincide to the fraction of a minute.

The time of the perihelion passage may be considered as pretty accurately fixed, either for Sept. 12 or 9, or, as is more probable, at some intermediate period. From this we can fully explain some of the phænomena generally remarked. From the 9th, as stated in the Glasgow papers by a correspondent, the comet was observed to increase considerably both in brilliancy and in the apparent magnitude of the coma, but particularly of the tail, in the course of eight days. This verifies very happily the observation of Sir Isaac Newton, that it is not till immediately after the perihelion passage, that comets acquire their maximum of lustre and of size. The enlargement therefore uniformly takes place at that time, whether the comet is coming nearer us or moving in the opposite direction. The quantity of increase due to its approximation alone, in six or eight days, can be calculated, and we know that there is no instrument in Scotland capable of measuring the change of apparent magnitude produced by this cause. Whether the exquisite micrometer of Troughton, applied to our great Herschelian telescope, may show any difference, I shall be able to ascertain in a few days, as that instrument is lately dispatched from London for us.

I must acknowledge, however, that I entertain very slender hopes of success in this kind of observation on a minute body surrounded with such a nebulosity, and at a distance from us much greater than that of the sun. It has been said, that this comet was ascertained to be the same with that of 1661. The two are as different as can be imagined

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in every respect. Hence we may see how much safer, in the event, scientific investigation is than vague conjectures. I subjoin the elements of the comet of 1661, and those now given by Burckhardt:

	COMET 1661.	BURCKHARDT.
Long. of node.....	82 deg. 30 min.	140 deg. 13 min.
Inclination	32 deg. 35 min.	72 deg. 42 min.
Place of perihelion,	115 deg. 58 min.	74 deg. 12 min.
Perihelion dist.	42,600,000 miles.	96,000,000 miles.

I am, sir, your obedient servant,

ANDREW URE.

Glasgow Observatory, Oct. 16.

SIR,—In the Glasgow Courier of October 5, I had the honour of submitting to the public the results of the joint labours of Mr. Cross and myself, for the preceding month, on the comet, at the Glasgow Observatory. In The Star newspaper of October 11, appeared for the first time the elements of the orbit, as determined by the celebrated Burckhardt, member of the National Institute.

It is a duty which I owe to the skill and the unwearied exertions of my associate Mr. Cross, to this patriotic establishment, and also to this country, hitherto considered by the French mathematicians and astronomers unequal to the primary solution of this difficult problem, to state the following facts:—On October 8, at eight hours fifteen minutes, by observations made here, with every precaution to insure the utmost accuracy, the comet had deviated 42 degrees 18 minutes from the longitude which Burckhardt's elements assign for that instant. On October 14, at two o'clock in the morning, the longitude, as deduced from a most satisfactory transit, was 206 degrees 42 minutes. By the French computation it ought to have been 248 degrees 1 minute, differing from nature by 41 degrees 19 minutes. By our elements, which have received a partial correction from my observations since the 5th, the coincidence on the 8th, at the same time, was within 15 minutes, and on the 14th, within 13 minutes. Our computed latitudes on the 13th agree to a minute with observation, while those of Burckhardt differ by 3 degrees or 180 times that quantity.

The examination of both has been made by the excellent tables of the parabola, constructed by Delambre, imperial observer at Paris. It is in the longitude of the perihelion that the chief discordance exists between the French elements and ours, and this amounts to about 31 degrees; the

the former being, in our judgement, too small by this quantity.

The comet has been continually approaching the earth for many weeks. From September 15th till October 14th, its decrease of distance amounted to 25 millions of miles, yet its brilliancy and the magnitude of its tail have gone on diminishing, as Burckhardt properly remarked. Persons ignorant of astronomy would naturally infer from this diminution the recedure of the comet from us, as, from its increase they conjectured its approach. Astronomers laugh at such idle dreams when applied to a demonstrative science, in which conjecture has found no place since the days of Newton. Its first principles teach, that these phænomena arise from the comet's varying distance from the sun. At the period of the perihelion passage these bodies are known uniformly to attain their maximum of size and brightness.

If Glasgow has been justly ridiculed on the present occasion for the dexterous appropriation of every floating error about the comet, I trust this communication will, in some measure, redeem its former scientific character. It is to be hoped that some of our eminent philosophers will take the trouble of comparing Burckhardt's elements with our observations, and that they will speedily give the result of their comparisons to the public. Nothing could have induced us to enter the lists against so able a competitor, but the conviction of the justice and importance of the cause which public duty assigned. I am your obedient servant,

ANDREW URE.

Mr. T. Leybourn, of the Royal Military College, editor of the Mathematical Repository, intends to publish, by subscription, A Collection of all the Mathematical Questions and their Answers, which have appeared in the Almanack called *The Ladies' Diary*, from its commencement in 1704 to the present time. The editor of the Diary (Dr. Charles Hutton) published a similar work in 1773, but comprehending both its mathematical and poetical parts down to that period. Mr. Leybourn's publication will comprehend only the Mathematical Part; and, with Dr. Hutton's permission, will contain all the valuable Additions given in his Edition, as far as it extends. He also hopes to be able to give other Additions by the assistance of some of the ingenious Mathematicians who have for a number of years past contributed to the Mathematical Repository.

The work will be printed in 8vo, and will be published in in half volumes, one of which will appear every three months. The Diagrams will be printed in the text from figures cut in wood. It will be put to press as soon as such a number of subscribers can be obtained as shall give the editor a prospect of being indemnified for the expense which must attend its publication.

Mr. Parkinson's Third Volume of the Organic Remains of a former World will be published in November.

ANTIQUITIES.

While the workmen were lately opening some ruins in the venerable mansion of John Floyd, esq. near Redburn, Oxfordshire, they discovered below the foundation of an old wall a leaden box, measuring three feet in length by two feet and a half in breadth, in perfect condition, and perfectly secured by an antique kind of padlock, which was not forced but with great difficulty. When opened, it contained 72 copper medals, each weighing three ounces and one quarter, all in a high state of preservation. The devices on them, which are throughout the same, are, on one side, the figure of a dying warrior supported in the arms of two men in complete armour, and several others standing weeping round. In the back-ground, a battle raging; the motto "*Dulce et decorum est pro patriâ mori*" surrounding the whole. On the reverse a Roman triumph, with no less than 115 figures. Along with the medals were four beautiful lamps, made of a composition chiefly silver; two small daggers, most curiously wrought; five human figures in solid gold, supposed to represent the penates.—There was also a wooden box, contained in the leaden, 14 inches in length, apparently solid, which when exposed to the air crumbled into dust. A mutilated scroll was discovered, but too much disfigured by time for any of its contents to be legible, save a few detached sentences which are of an amatory description.

A REMEDY FOR APOPLEXY.

M. Sage has lately stated in a memoir read to the National Institute at Paris, the efficacy of flour volatile alkali in cases of severe apoplexy. "For at least 40 years," says he, "I have had opportunities of witnessing the efficacy of volatile alkali, taken internally, as an immediate remedy for the apoplexy, if employed on the first appearance of the disease. One of the keepers of my cabinet, aged 72 years, robust, though thin and very sedate, was seized, while fasting, with an an apoplexy. He fell down deprived of sense. When raised up, he had the rattles in his throat; his eyes were closed, his face pallid, and his teeth fixt together. I drew out his under lip so as to answer the purpose of a spout, into which was poured a spoonful of water, containing 25 or 30 drops of flour volatile alkali. At the same time two slips of paper, the edges of which were wetted with volatile alkali, were introduced into his nostrils. The teeth were speedily separated, and the eyes opened. A second dose of alkali was instantly poured down the throat. The rattles ceased; speech and recollection returned. In the course of an hour the patient recovered sufficient to proceed without assistance about 300 paces to his own chamber. In another hour he got up, asked for something to eat, and has since experienced no return of the disorder." He reports another instance in the person of one of his friends, who was a great eater, and was struck with the apoplexy while at table. "The volatile alkali excited a vomiting; and after that had abated, the patient took 20 drops of volatile alkali in half a glass of wine. His senses returned, and in two hours he was able to walk in his garden."

LECTURES ON MANUFACTURES.

Mr. Clennell, of Homerton, F.S.A. Edinburgh and Perth, &c. &c. conductor of the New Agricultural and Commercial Magazine, or General Depository of Arts, Manufactures and Commerce, will deliver a Course of Six Lectures on Manufactures, at Mr. Cowland's, the New Inn, Stratford.—The Lectures will commence at 7 o'clock on Friday Evening, the 1st of November, and be continued at the same hour on the Thursday Evenings of the five following weeks.

LIST OF PATENTS FOR NEW INVENTIONS.

To William Taylor, of Gomersal, in the county of York, merchant, for his machine or apparatus to be attached to the axle-tree and nave of wheel carriages, whereby their motion may be gradually checked and stopped, and also again loosened or unstopped at the pleasure of the driver or passengers, during the progress of the carriage.—August 7, 1811.

To James Mallovy, of the state of New York, but now residing in the city of London, hatter, in consequence of a communication made to him by a certain foreigner residing abroad, for a machine for cutting or shearing the nap or wool from all kinds of broad and narrow cloths.—August 7.

To

To William Davis, of Royal Oak Yard, Bermondsey Street, in the county of Surrey, engineer, for his machine for chopping meat for sausages and other like purposes.—August 7.

To John Stubbs Jorden, of Birmingham, patent copper window frame manufacturer, for his new method of glazing hot-houses, green-houses, and all horticultural buildings.—August 20.

To William Good, of Coleman Street, London, plumber, for his improvement in valves for various purposes.—September 9.

To Walter Rochfort, of Bishopsgate Street, London, grocer and tea-dealer, for his improved method of preparing coffee.—September 9.

To William Frederick Collard, of Tottenham Court Road, in the county of Middlesex, musical instrument maker, for certain improvements upon an upright piano forte.—September 9.

To John Barton, of Tufton Street, Westminster, engineer, for a sawing machine upon an improved construction.—September 9.

To William Walter Jenkins, of Birmingham, brass-founder, for his improvement in the method of manufacturing drawer and other knobs of different shapes and forms, used with or affixed to cabinet and other furniture and things, whereby much labour and expense will be saved in the manufacturing of the same.—Sept. 9.

To John Jones, of Beverton, in the county of Glamorgan, gent., for a new method or methods of applying the expansive force or pressure of atmospheric air, condensed air, or steam, in or upon a wheel, so as to be the first mover of machinery.—Sept. 9.

To Michael Logan, of Paradise Street, Rotherhithe, engineer, for an instrument for the generation of fire, and various purposes in chemical and experimental operations.—Sept. 9.

To William Strachan, of Pool Cottage, in Poolton cum Seacombe, in the county of Chester, chemist, for a new method of preparing the ore of cobalt for the various purposes to which it is applicable in trade, manufacturing, and painting.—Sept. 9.

To John Chancellor, of Sackville Street, Dublin, watch- and clock-maker, for his mechanical musical instrument, on a new construction, applicable to clocks and other kinds of machinery.—Sept. 9.

To Thomas Marsh, of King Street, in the parish of St.

St. James's, Clerkenwell, watch-maker, for his improvements in the construction of watches.—Sept. 9.

To George Kitchen, of Sheffield, silver plater, for his method of making portable sconces or branches.—Sept. 14.

To William Fothergill, of Greenfield, in the parish of Holywell and county of Flint, copper forger, for his new method of making copper rollers for printing.—Sept. 23.

Metereological Observations made at Clapton in Hackney, from Sept. 24 to Oct. 23, 1811.

Sept. 24.—A clear morning was followed by abundance of *cumuli* floating along in the wind; this was the only cloud which prevailed through the day: in the evening large sheets of the *cirrostratus* obscured the setting sun, and were followed by strong west wind and frequent showers through the night, with a falling barometer.

Sept. 25.—Wind and rain from the west all day. The mercury kept sinking till about four o'clock, when it began to mount again, and continued rising all night.

Sept. 26.—Early the sky was clouded, with strong wind from the west; afterwards it cleared, when *cirrostratus* appeared in a lower region while *cirri* were abundant above: in the afternoon followed *cumulostratus*, the wind became calmer, and in the evening showers came on.

Sept. 27.—Cloudy morning, rainy day, and clear evening with clouds at different altitudes: wind W. and N.W.

Sept. 28.—A thick *stratus*, followed by gentle showers and fair evening.

Sept. 29.—An overcast morning: soon several strata of clouds appeared: when the day cleared large petroid *cumuli* and *cumulostrati* were seen, among others small and floccose, while in a region more lofty, light flimsy clouds showed features of *cirrus*, *cirrocumulus*, and *cirrostratus:* large denser and wavy sheets passed over and threatened rain, but the night became fair.

Sept. 30.—Early the linear *cirrus* ranged south-east and north west, and showed a tendency to *cirrocumulus* and *cirrostratus*; presently *cumuli* curling inwards floated below and inosculated, and gentle showers were the consequence.

Oct. 1.—Small rain, then showers, in the intervals of which light flimsy clouds were in a high and calmer region, while large *cumuli*, as yesterday, floated along in the west wind below.

Oct. 2.—Thin *stratus* followed by clear morning, afterwards *cumulostratus:* about four o'clock drops of rain fell from

from a light *nimbus*: fine evening: about six o'clock *cirrostratus* ranged N.W. and S.E. from which cirrose tufts pointed to S.W.; from which quarter I saw numerous little linear *cirri* ranging by moonlight.

Oct. 3.—Thin *stratus* with strong dew: *cirri* passed over from N.W., and in a lower region long spreading beds of *cirrostratus* in some places becoming *cirrocumulus*. Soon after noon rain set in with a south-east wind. In the evening, which was warm and muggy, I observed the clouds to pass over from W.S.W.

Oct. 4.—Two strata of cloud produce showers at times; wind south.

Oct. 5.—Windy and showery, with frequent rainbow. In the clear intervals fleecy *cumuli* flew along at different heights, the lower ones moving fastest: in a region still higher, features of *cirrus*, *cirrocumulus*, and *cirrostratus* frequently appeared; while large petroid *cumulostratus* rose in the horizon.

Oct. 6.—Clouded morning with calmer west wind: in the evening abundance of *cirrus* followed by *cirrostratus* and *cirrocumulus* ranging from the south.

Oct. 7.—Misty, with a breeze from south-west; then clouded, with showers: fair intervals by night.

Oct. 8.—Light *stratus* early: abundance of *cirrus* pass over gently from the west, some of its tufts bent obliquely upwards at right angles: *cirrocumulus* and *cirrostratus* also formed, and *cumuli* sailed under: afterwards *cumulostratus* alone increased through the day: clear by night, with a few *cirrostrati*. Comet very bright.

Oct. 9.—Calm clouded day, various clouds; the *cirrostratus* prevails; wind S.W.

Oct. 10.—Calm and cloudy; features of all the modifications followed by slight rain by night. S.W.

Oct. 11.—Chiefly clouded; in the breaks several strata observed: fleecy *cumuli* flew rapidly along, though the wind below was gentle S.S.W.

Oct. 12.—Gentle showers from the union of two strata of clouds; towards evening the wind got up with hard showers, and a clear night: some small meteors left long trains.

Oct. 13.—Clear strong wind from N.W. fleecy *cumuli* flew along in it; higher up various *cirri*; in some places cirrose fibres transversely intersected by tufts became a beautiful reticular *plexus*: about five in the evening a *solar halo* appeared, with spreading and low *cirrostratus*.

Oct. 14.—Damp day with small rain; clear intervals by night;

night; a little brilliant meteor descended into a fleecy *cumulus*. Wind S.W.

Oct. 15.—Misty morning with *cumuli*, followed by clear and very warm day with a south-west wind.

Oct. 16.—Fair warm day, *cirrus* and *cirrocumulus* with some *cumuli*, followed by all the modifications variously mixed in different altitudes and interfused with mistiness; fine orange colour at sun-set; flashes of lightning by night.

Oct. 17.—Misty and overcast morning; afterwards fair with *cirrus, cirrocumulus*, and others; *stratus* by night. S.W.

Oct. 18.—Thick *stratus* followed by fair warm day with multiform *cirrus, cirrocumulus, cumulus, cumulostratus*, and afterwards *cirrostratus*: rain occurred in the night. Wind gentle from S.S.W.

Oct. 19.—A wet mist, followed by much cloud and mistiness: towards evening it was fair, when much *cirrus* was observed scattered about ahead of *cumulus* and *cumulostratus* which appeared lower. Thick *stratus* at night. Wind gentle from S.S.W.

Oct. 20.—A mist followed by damp and chiefly cloudy day, with some intervals of sunshine: *stratus* by night. Wind S.S.W.

Oct. 21.—Cloudy morning followed by fair day with *cirrus* and abundance of *cirrocumulus** above large and lowering fleecy *cumuli* flying beneath in the wind: very warm night, with a brisk gale from the south; distant flashes of lightning.

Oct. 22.—Fair morning with south wind, *cirrus* and *cirrocumulus* above; large low and spreading *cumulus* and *cumulostratus* below: afterwards sky became clouded all over, rain followed, and the wind became west. Clear again by night.

Oct. 23.—Filiform *cirri* range from N.W. to S.E. and become a fine veil; *cumuli* float below in W.S.W. wind: afterwards much spreading cloud and misty horrizon. Towards evening breeze from N.W. with light showers; very clear by night, with some small meteors and cooler air.

Five Houses, Clapton, THOMAS FORSTER.
October 24th, 1811.

* Some *cirrocumulus* appeared whose *nubeculæ* were smaller than those of a bed of *cirrocumulus* in a *higher* region; which is an unusual inversion of order.

METEORO-

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For October 1811.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Thermometer. Noon.	Thermometer. 11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Sept. 27	46	50°	48°	29·15	0	Rain
28	45	60	50	·20	10	Showery
29	50	62	50	·50	29	Fair
30	50	63	55	·65	15	Showery
Oct. 1	55	62	52	·41	0	Showery
2	54	61	49	·72	33	Fair
3	44	56	52	·62	0	Rain
4	54	68	60	·50	21	Fair
5	59	66	56	·66	32	Stormy
6	56	64	57	·89	42	Fair
7	57	67	58	·93	30	Cloudy
8	57	67	56	·99	41	Fair
9	56	64	57	30·05	37	Fair
10	57	64	60	·06	32	Fair
11	60	66	60	29·84	29	Fair
12	60	63	56	·67	26	Cloudy
13	57	60	58	·82	35	Fair
14	58	60	57	·83	0	Small rain
15	60	70	63	·80	35	Fair
16	60	70	63	·97	32	Fair
17	60	69	60	30·09	30	Fair
18	55	68	61	·18	25	Fair
19	60	64	54	·22	21	Cloudy
20	55	63	56	·22	10	Cloudy
21	55	62	60	29·82	27	Fair
22	60	64	54	·54	26	Cloudy
23	52	59	52	·56	29	Fair
24	51	52	47	·48	0	Rain
25	46	56	46	·34	18	Fair
26	45	51	47	28·69	0	Stormy

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

LIX. *The Reports of Mr.* William Smith, *and Mr.* Edward Martin, *to the Bristol and Taunton Canal Company, on the State of the Collieries at and near Nailsea, in Somersetshire.*

The Committee of Proprietors under an Act of Parliament passed last sessions, for the Bristol and Taunton Canal, in Somersetshire; having resolved, in June last, to take the opinions of two eminent mineral surveyors, whether the coal-field around Nailsea, across which their line of canal is to pass, was likely to furnish such a supply of Coals, as by the tonnage on them, to pay interest to the proprietors for the expense of executing this part of their line, about eight miles in length from the river Avon at Morgan's Pill, near Bristol, with a branch to the eastward of about two miles in length, to Nailsea collieries: I am happy in being able to present the Reports of these two gentlemen, conceiving that they will be read with interest by a considerable class of my subscribers: and I beg to solicit the communication of similar documents, from time to time, respecting other coal and mining districts. Editor.

To the Committee of Management of the Bristol and Taunton Canal.

Bristol, July 1, 1811.

Gentlemen,—Agreeably to your order of the 13th of June, requesting my assistance to examine and report on the probability of a sufficient quantity of coal at Nailsea, and the neighbourhood, to induce the Company to proceed with the canal from Morgan's Pill to Nailsea, immediately; I am happy to state that my Survey of those works has been highly satisfactory; and, that a sufficient quantity of coal may thence be obtained, is more than probable.

This coal district is of much greater extent than is generally imagined, and, like the great coal-field at Newcastle-upon-Tyne, becomes flatter in the deep than at the outcrops. From this favourable position of all the coal (which I have most clearly ascertained), and from the great difficulties which were likely to happen, with respect to water, being successfully encountered by the engines lately erected, there can be no doubt of the permanency of the works. From the extraordinary hardness of the roof, and the easy working of the coal, I have no doubt but the Nailsea Pits will produce the quantity stated.

In my Survey of the Backwell Common Works, I also found many favourable circumstances belonging to those veins

veins of coal, which cannot fail to make the collieries established on them, of long duration. The veins are of sufficient thickness to produce a great quantity of coal, without going over much ground. There are also a sufficient number of veins lying one beneath another, within a moderate depth from the surface, so as not to require the too frequent repetition of the great expense of new pits and machinery. The veins also lie so moderately inclined, as to be for a long period of years within the reach, of such shafts as may be sunk, by the help of steam-engines, which, from the small quantity of water, in a great extent of coal already working, have not yet been found necessary. As the sinkings through the strata lying over these veins of coal are all soft and mostly impervious to water, it may be reasonably expected that the veins which lie under these will have still less water.

The surface of the land, to a great extent around these collieries, (at Blackwell Common) is a tenacious clay, quite unabsorbent, and altogether unlike the land at Nailsea; and although the veins are thinner, there are more of them, and the coal is of a harder and better quality. The disadvantages which these works have experienced, from the quantity of timber required, will lessen with the depth to which the veins are worked, and the expense of procuring such timber will be lessened by making the canal. Although these veins of coal have been worked for a long time, the works have been carried on in such a small way, as not materially to have reduced the quantity of coal, or to render the working of the deep coal anywise dangerous, from water contained in the old hollows. The whole of the water between the pits and the outcrops is known, and daily exhausted, without the aid of pumps, and, in fact, all the coal that has ever been worked out of these veins, has been merely along the outcrops; and instead of exhausting the veins, or of rendering the deep works dangerous, they have most satisfactorily proved the great extent to which such works may be carried.

These veins appear to underlay the Nailsea veins; and it is highly probable that other veins between them remain undiscovered. At these pits, (Mr. Walters's and Mr. White's) there are large stacks of good coals on hand: if these works in their present state are capable of thus overstocking the sale, there can be no doubt of what they will produce, when all of them are in full working. Besides the pit which is now working at Nailsea, there is another nearly down to coal, and old ones are kept open, which may

be

be very readily cleaned up, and brought into use, either by opening gangways from the bottoms of them to the deep coal, or by sinking them deeper, to work the under vein, which is quite unwrought, except by a few shallow pits along the outcrop, and which were drained by a level through Mr. Davis's tan-yard.

From the singularly advantageous position of the Nailsea coal, which I have clearly ascertained, and the quantity of water which can come to the pits, being also clearly known, either from the hollows of old workings, or the vast extent of coal which can be worked, and that quantity of water being kept down, and rapidly decreasing in all parts of such an extensive coal-field, by the power of the present engine, proves most clearly that such a colliery, worked to its utmost extent, can never be drowned.

From duly considering all these circumstances, I am fully satisfied, of the great extent to which these collieries may be worked; and have no hesitation in stating, that the Bristol and Taunton Canal Company may, with the greatest safety, proceed with the immediate execution of their canal to these collieries, with full confidence of thence obtaining tonnage sufficient, to pay a good interest on their expenditure.

15, Buckingham Street,
York Buildings, London.

WM. SMITH, Engineer.

To the Committee of Management of the Bristol and Taunton Canal.

Gentlemen,—In consequence of your application, requesting my assistance in investigating the state of the collieries in and about Nailsea, in order to ascertain whether there was a sufficient prospect of coals, to induce the company to cut the canal from Morgan's Pill to Nailsea immediately; I attended at Nailsea on the 22d and 23d instant, and proceeded to the investigation, and I now send you the following observations, delineations, and report.

[Vide rough Sketch of Nailsea Colliery, in annexed Section No. 1, Plate IX. the thickness of the veins or seams being as follows; viz.

1st vein ...... $4\frac{1}{2}$ feet
2d ditto 2

$6\frac{1}{2}$ feet of coal, in all.

From which it may be seen, that the position of the strata and veins of coal, which are near the north crop, as at (*a. a.*), rise out very rapidly, but which moderate materially

 in

in going down southward in the deep; for at the 50 fathoms Engine Pit, which is the deepest point this vein has ever been sunk to, and but very little coals have been worked at that depth, the fall or dip of the strata is only from seven to eight inches in the progressive yard, or as near as may be, a dip of one in five; and I have no doubt but that the strata and veins of coal dip less and less as they run southward, as down (*b b.*), and soon afterwards rise gradually southward, as up (*c c.*). I am confirmed in this opinion from two corroborating circumstances.

1st. On examining the ground from Nailsea Colliery, along the surface line (*d d.*) southward to Chelvy, Youngwood, &c. I observed in the Quarries, and other places where the rock was bare, that the strata rose southward; and if the common strata do so, the veins of coal must do so too, for they are parallel beds.

2d. It has been observed in the neighbourhood of (*e e.*), that the water in some of the Wells has been drained off at times, when the water in the 50-fathom Engine Pit was quite out; and that when an accident happened to the engine, so as to occasion the water of the colliery to rise a considerable height in the pit, the wells were also filled again. This proves a subterraneous communication, and which is effected through the means of a very hard rock (intersected with chinks and chasms) which lies in a great thickness, immediately above the main vein of coal, through which the water is perpetually flowing in great abundance, to the large engine in the 50-fathoms pit. This thick hard rock, at the same time that it forms a most substantial strong roof to the main vein of coal, certainly lets loose an immoderate quantity of water to the fire-engine, the pumps of which are 18 inches; but I do not wonder at this large flow of water, at the depth of about 50 fathoms; for I am well aware, that the surface water in coal countries, in general, is let in from gravel banks, or from some loose matter connected with the surface, which often overpowers the means opposed to it, and which cannot be conquered but by additional machinery. This once done to a considerable extent, as to depth, greater depths may with facility be sunk to and worked, without much risk of pricking water. The principal colliery at Whitehaven, the property of Lord Lonsdale, dipping directly to and under the sea, is drained close to the sea, at the depth of 80 fathoms; and the same vein is pursued to, and worked under the sea, to the depth of 150 fathoms, where so much water is not met with, as would suffice for the wetting of the underground tram-roads, which

which is the case in all other situations where collieries are worked to very great depths. I merely mention these circumstances, on account of the above engine being heavily laden with water, at the depth of 50 fathoms only, and to show that it is no uncommon case. When other collieries are open on the same vein to the westward, viz. on Nailsea and Kenn Moors, they will partake of this colliery water, which seems to follow a particular stratum of very hard jointy rock.

As to White's Colliery, which lies to the north and north-east of Messrs. Grace's Colliery, at Nailsea; the veins which are known here, break out to the north of the Nailsea Colliery, and naturally lay perpendicularly under the same. The following are the number and thickness of the veins, with the depth to which they have been sunk down and worked.

[Vide White's Colliery, in annexed Section No. 2;] where the thicknesses are as follows, viz.

1st, King's-hill vein	2 feet
2d, Main vein	$3\frac{1}{2}$
3d, Dungey vein	2
	$7\frac{1}{2}$ feet, in all.

As to Backwell Colliery, [vide No. 3, in annexed Section,] the property of Mr. Teague, partly his own estate, and partly that of the Marquis of Bath; the thicknesses are as follows, viz.

1st, Vein (of Smith's coal) .	$2\frac{1}{2}$ feet
2d, Vein	3
3d, Ditto	$1\frac{1}{2}$
4th, Ditto	3
5th, Ditto	2
	12 feet, in all.

As to the veins of coal which lie parallel one under the other in a limestone basin, (which is the case with all the veins of coal in the coal countries that I am acquainted with*,) [vide annexed Section No. 4; where G. W. and T. show the places of the supposed southern crops of Grace's, White's, and Teague's veins, respectively.]

* This gentleman, in the year 1806, communicated to the Royal Society a most interesting map and account of the great coal-field in South Wales, lying there in a limestone basin, which are printed in the Philosophical Transactions of that year: from what I have since read of Coal-fields, in Williams's "Mineral Kingdom," Westgarth Forster's "Treatise on a Section of Strata," Farey's "Derbyshire Report," vol. i. &c. I am inclined, however, to think, that some at least of the coal-fields in the middle and north-eastern parts of the island, do not agree with those of the western side of it, to which Mr. Martin here alludes, in basseting on all sides from out of a limestone basin.—EDITOR.

This

This section shows the position in which the sundry veins of coals lie in the ground, and exactly in a parallel direction with the substratum, the limestone. I have already pointed out two proofs at Nailsea Colliery, of the veins and strata becoming flat in the deep, and afterwards rising southward; and if the Nailsea veins really do so, there can be no manner of doubt of all the other veins of coal and strata, acting and laying in the same manner. There is also positive proof, that the limestone takes a dip from the north side, (where the same is observable at Belmont, Wraxall, and Clevedon-Court) to the southward; and from the south side to dip northward, observable at Backwell Village, at Chelvy, Mr. Piggott's, &c.; which proves the uninterrupted continuity of the limestone all the way underneath.

No. 5, is a rough sketch Plan of the north and south limestone, which contains the veins of coal, in the range from Backwell collieries to Nailsea, Nailsea-Moor, Kenn-Moor, and the inclosed lands westward, from thence to the sea,—where

a a. are Messrs. Grace's veins of coal at Nailsea, dipping southward.
b b. are the supposed crops of the same veins, dipping northward.
c. is the east end or crops of the same veins, dipping westward.
d d. are the crops of White's veins, dipping southward.
e e. are the supposed south crops of the same veins, dipping northworth.
f. is the supposed east crop of the same veins, dipping westward.
g g. are the Backwell veins, dipping southward.
h h. are the supposed south crops of the same veins, dipping northward.
i. is the supposed east crop of the same veins, dipping westward.
And the centre or hollow of the supposed basin is along the dotted line *l l l.*

The north, the south, and the east outcrops of these veins are here delineated; but the west crops cannot be ascertained, for the basin seems to *widen* very much going westward. It is not a mile wide on the surface at A. There is no doubt, I think, of each having a west outcrop, but it may be at many miles distance, under the sea, where the veins of coal may have increased in number as the basin became more extended; and as the level course of the veins and strata

strata run nearly west or down channel, they cannot take land and be seen again in Glamorganshire, &c.

Very large new collieries may be opened on Nailsea Moor, the property of Sir Hugh Smyth, Bart.; on Kenn-Moor, the estate of Lord Paulett: under the inclosed lands from Kenn-Moor to the sea; and under the lands of Sir Abraham Elton, Bart., near Clevedon church, where the Backwell veins, worked by Mr. Teague, must range. The proposed collieries on Kenn-Moor, which is about the main line of Canal, will be two miles nearer to Morgan's Pill than the Nailsea, for the canal branch to Nailsea appears to be about two miles in length.

There are ten veins or seams of coal already discovered within this limestone basin; which added together make 26 feet of solid coal. These ten veins worked in the usual way, will yield 30,000 tons of coal per acre; but as some of them are thin, being under two feet, I will only calculate upon 20,000 tons per acre, and on working 400 tons per day, and on 300 working days in the year.

Consequently 400 tons × by 300 days, make 120,000 tons per annum.

And again, I will only estimate upon 1,000 acres, containing upon an average the whole of the ten veins, though I have no doubt but there are 2 or 3,000 acres. Therefore 1,000 acres × by 20,000 tons per acre, give 20,000,000 of tons, and 20,000,000 divided by 120,000 tons per annum, give 166 years, which is the length of time the colliery would last, at 120,000 tons per annum.

The distance from Morgan's Pill to Nailsea Collieries I understand is ten miles, and the tonnage authorised to be received by the act for making the Canal is 2*d.* per ton per mile; so that every ton is 20*d.* which on 400 tons per day, and on 300 days in the year, amounts to 10,000*l.* per annum, being equal to 10 per cent. upon a capital of one hundred thousand pounds. This is about the sum that the estimate is made for; but from what I observed of the line of canal, it is all good ground and easy cutting (excepting the Tunnel), and I think the whole ten miles should be completed for, from 70,000*l.* to 80,000*l.*

All the Nailsea Colliers are much interested in promoting the canal scheme; that is to say, in getting the canal brought from Morgan's Pill to the vicinity of their works; for till that is done, their sales of coals are so small, that the moneys arising from what is sold to the country, will hardly pay for carrying the works on, particularly where heavy engines and machinery are required.

The Committee of Management should require the Nailsea colliers to put their works in such a state and condition, as to insure the working of 500 tons of coals per day at the least, as a sort of guarantee to the canal proprietors for the risk of laying out immediately about 100,000*l*.

Messrs. Grace and Co.'s Colliery is heavily loaded with water, and by pricking additional feeders it would be drowned out; another Engine should be put up, to place them on a tolerable certainty. White's Colliery seems to be better off in regard to water, and a new engine of considerable power is erecting on the deep work.

At Teague's Colliery very little water has hitherto been met with, but in sinking deeper and in extending the work in every direction, feeders of water will surely be pricked, and that colliery cannot be considered out of danger till a fire engine is erected upon it.

The proposed collieries on Kenn-Moor, on Nailsea-Moor, and also on the lands of Sir Abraham Elton, Bart., to the southward of Clevedon church, should be opened.

As to the Prospect of Collieries from Clevedon Hill to Morgan's Pill.

A colliery was some years ago worked at Clapton, near the church, by virtue of a level which was brought up from the low grounds. The last pit upon the level head, was, I heard, nearly 40 fathoms deep, and the main vein when left was full six feet in thickness. Why it was abandoned, I know not; and coal strata appear all along from Clapton church to Portbury church, where sundry veins of coal I have no doubt exist. A canal once opened into that vicinity, will certainly prompt persons to open those collieries, and with a fair prospect of success, after a canal is made ready to their hands.

(Signed) EDWARD MARTIN, Colliery Surveyor.

Morriston, near Swansea, July 31, 1811.

LX. *On the Cultivation and Manufacture of Woad. In a Letter to the President of the Bath and West of England Agricultural Society. By Mr.* JOHN PARRISH*.

WOAD is a plant which, combined with indigo, gives the best and most permanent blue dye hitherto discovered. It is of great importance to our commerce, as well as to agriculture, being in nature one of the best preparers of

* From vol. xii. of the Society's Letters and Papers.

land

land for a corn crop that has hitherto been discovered; and, if the land is properly chosen for it, and well managed, will be found very profitable, more particularly at this time, when its price is advanced to almost an unprecedented degree: therefore I conceive that in rendering its cultivation and preparation better known and understood, it may be greatly beneficial to the nation.

I have the honour to be a member of the Bath and West of England Agricultural Society, where many noble and exalted characters unite their talents to promote the public benefit. And to one of its earliest and most respectable members I presume to address this information.

I have been many years a considerable consumer of woad, and have also cultivated it with much success: and though I am well experienced in the usual method of its preparation, I was induced to depart from it in consequence of the great waste of its juices in the old method of grinding and balling. But I shall endeavour to give instructions for carrying on each process, and leave those who shall undertake it to proceed as they think best.

This plant is cultivated in different parts of England for the use of the dyers, as well as in France, Germany, &c. It is best to sow the seeds in the month of March, or early in April, if the season invite, and the soil be in condition to receive it; but it requires a deep loamy soil, and is better still with a clay bottom, such as is not subject to become dry too quickly. It must never be flooded, but situated so as to drain its surface, that it may not be poisoned by any water stagnant upon it.

If (at any reasonable price) meadow land to break the turf can be obtained, it will be doubly productive. This land is generally freest from weeds and putrid matter, though sometimes it abounds with botts, grubs, and snails. However, it saves much expense in weeding; and judicious management will get rid of these otherwise destructive vermin. A season of warm showers, not too dry or too wet, gives the most regular crop, and produces the best woad.

If woad is sown on corn-land, much expense generally attends hoeing and weeding: and here it will require strong manure, though on leys it is seldom much necessary, yet land cannot be too rich for woad. On rich land dung should be avoided, particularly on leys, to avoid weeds. Some people sow it as grain, and harrow it in, and afterwards hoe it as turnips, leaving the plants at a distance in proportion to the strength of the land: others sow it in ranks by a drill-

plough,

plough; and some dibble it in, (in quincunx form, by a stick with a peg crossways, about two or two and a half inches from the point, according to the land,) putting three or four seeds in a hole, and these holes to be from twenty inches to two feet apart, according to the richness of the land: for good land, if room be given, will produce very luxuriant plants in good seasons; but if too nearly planted, so that air cannot circulate, they do not thrive so well: attention to this is necessary in every way of sowing it. I have been most successful in this last process. Woad very often fails in its crop, from the land not being in condition, or from want of knowing how to destroy the botts, snails, wire-worms &c. that so often prey upon and destroy it, as well as from inattention to weeding, &c. Crops fail also from being sown on land that is naturally too dry, and in a dry season; but as the roots take a perpendicular direction, and run deep, such land as I have described (with proper attention to my observations) will seldom fail of a crop: and if the season will admit sowing early enough to have the plants strong before the dry and hot weather comes on, there will be almost a certainty of a great produce.

These plants are frequently destroyed in the germination by flies, or animalculæ, and by grubs, snails, &c. as before observed; and in order to preserve them, I have steeped the seeds with good success in lime and soot, until they began to vegetate; first throwing half a load or more of flour lime * on the acre, and harrowing it in. Then plant the seeds as soon as they break the pod, taking care not to have more than one day's seed ready; for it is better to be too early, than to have their vegetation too strong before it is planted, lest they should receive injury; yet I have never observed any injury in mine from this, though I have often seen the shoot strong. Either harrows or rollers will close the holes. If the ground be moist it will appear in a few days; but it will be safe, and a benefit to the land, to throw more lime on the surface, when, if showers invite snails and grubs to eat it, they will be destroyed, which I have several times found; particularly once, when the leaves were two inches long, and in drills very thick and strong, but the ground was dry. When a warm rain fell, in less than two hours I found the ranks on one side attacked by these vermin, and eaten entirely off by a large black grub, thousands of which were on the leaves, and they

* If the seeds are not sown within a day after the time, it will lose much effect.

cleared

cleared as they went, not going on until they had destroyed every leaf where they fixed. They had eaten six or seven ranks before I was called by one of my people to observe it. Having plenty of lime, I immediately ordered it in flour to be strewed along those ranks which were not begun. This destroyed them in vast numbers, and secured the remainder. Another time, having had two succeeding crops on four acres of land, I considered it imprudent to venture another. However, as the land after this appeared so clean and rich, I again ventured, but soon found my error. On examining the roots (for after it had begun to vegetate strong, it was observed to decay and wither) I found thousands of the wire-worm at them, entwined in every root. I immediately strewed lime, (four loads, of six quarters each, on the four acres,) and harrowed it; when rain coming on soon after, washed it in, and destroyed them all, and gave me an extraordinary crop; but the first-sown side of the field, where they had begun, never quite recovered like the rest. And I am fully satisfied, that when the grub is seen in wheat, &c. the same treatment (if the weather suited) would destroy them all, as well as change the nature of the land. I need not enter on the wide and extensive field of observations on the causes of weeds, grubs, &c. (which so often counteract the labours of the husbandman,) that occur so differently in different seasons, and after different treatment and improper crops,—further than to observe that when your land has *not a proper change*, then it is that these are experienced in a more destructive degree.

Further, it is in vain to expect a good crop of woad, of a good quality, from poor and shallow land. The difference of produce and its value is so great, that no one of any experience will waste his labour and attention on such lands upon so uncertain a produce. Warm and moist seasons increase the quantity every where, but they can never give the principle which only good land affords.

In very wet seasons, woad from poor land is of very little value. I once had occasion to purchase at such a time, and found that there was no possibility of regulating my vats in their fermentation; and I was under the necessity of making every possible effort to obtain some that was the produce of a more congenial season. I succeeded at last; but I kept the other three and four years, when I found it more steady in its fermentation; but still it required a double quantity, and even then its effect was not like that from good woad.

At this time several dyers experienced much difficulty, and

and one of eminence in the blue-trade suffered so much by woad of his own growth, that he declared his resolution to decline the trade altogether. When I pointed out to him that it was the woad that occasioned his bad blues, and that I had from the same defect purchased such other woad as would do, and informed him where he could get it,—he succeeded as usual. His own he disposed of to a drysalter, who sold it again somewhere in the country; and it occasioned such a cause of complaint, as I believe rendered the claim of payment to be given up, or partly so: of this I am not certain, having it only from report. I mention this in order to give those who wish to become growers of woad, such information as may properly direct them.

The leaves of woad on good land in a good season grow very large and long, and when they are ripe show near their end a brownish spot inclining to a purple towards its centre, while other parts of the leaves appear green, but just beginning to turn of a more yellowish shade; and then they must be gathered, or they will be injured.

Woad is to be gathered from twice to four and even five times in the season, as I once experienced (it was an early and a late season), and for the next spring I saved an acre for seed, of which I had a fair crop. I picked the young seedling sprouts off the rest, and mixed with my first gathering of what was newly sown; this was very good. During one season I let these shoots grow too long; the consequence was, that the fibrous parts became like so many sticks, and afforded no saponaceous juices. When you design to plant woad on the same land the second season, it should be as soon as your last gathering (before winter is finished) be ploughed; that is, as soon as the weather will permit, and in deep furrows or ridges, to expose and ameliorate it by the vegetative salts that exist in the atmosphere, and by frost and snow. This, in some seasons, has partly the effect of a change of produce; but if intended for wheat, the last gathering should not be later than September.

The land, after woad, is always clean, and the nature of the soil appears to be greatly changed in favour of the wheat crop; for I have always experienced abundant increase of produce after woad, and observed that it held on for some time, if proper changes were attended to, and good husbandry. Keeping land clean from weeds, certainly produces an increase of corn; but in the hoeing and gathering woad (for hoeing and earthing up the plants often renders them abundantly more prolific, even if there are no weeds), many nests of animalculæ are destroyed, as well as grubs and

and insects, which are destructive to vegetation. All this is favourable to corn; but I am disposed to believe that woad in itself furnishes such a principle of change in favour of corn (and wheat in particular), as in a high degree to merit the attention of that Society who are so honourably united to promote and encourage the first interests of the British empire.

Having said all I conceive necessary on the cultivation of woad, I now proceed to say something on its preparation for the use of the dyer.

Woad, when gathered, is carried to the mill, and ground. I need not describe this mill, because they are to be seen in open sheds in several parts of England, only that I conceive some improvement might be made in their construction, so as not so much to press out and waste the sap, which contains the very essence of the dyeing principle. These mills grind or cut the leaves small, and then they are cast into heaps, where they ferment, and gain an adhesive consistence*; they are then formed into balls, as compact as possible, and placed on hurdles lying horizontally in a shed one over the other, with room for air between, to receive from the atmospheric air a principle which is said to improve them as a dye, as well as to dry them to a degree proper for being fermented; but in summer these balls are apt to crack in drying, and become fly-blown, when thousands of a peculiar maggot generate, and eat or destroy all that is useful to the dyer. Therefore they require attention as soon as any are observed to crack, to look them all over well, close them again, so as to render them as compact and solid as possible; and if the maggot or worm has already generated, some fine flour lime strewed over it will destroy them, and be of much service in the fermentation. These balls, if properly preserved, will be very heavy; but if worm-eaten, they will be very light, and of little value. They are then to be replaced on the hurdles, and turned, not being suffered to touch each other, until a month or more after the whole that is intended for one fermenting couch is gathered in, ground, and balled, and often until the hot weather of summer is past, to render the offensive operation of turning it less disagreeable, and not so apt to overheat; and though temperature herein is necessary, yet a certain degree of heat must be attained, before it is in proper condition for the dyer's use. This is easily distinguished by a change of

* In a dry place, if these leaves remain a fortnight, being occasionally turned, they will become more adhesive, and have less juices to squeeze out in balling. The balls must be compact.

smell—

smell—from that which is most putrid and offensive, to one which is more agreeable and sweet, (if I may be allowed the term,) for few people at first either can approve of the smell of woad, or of a woad-vat; though, when in condition, they become quite agreeable to those whose business it is to attend them. Woad is in this state of fermentation more or less time, according to the season and the degree of heat it is suffered to attain, whether at an early period, or according to the opinion of those who attend the process; but the best woad is produced from a heat temperately brought forward in the couch until at maturity, and turned, (on every occasion necessary,) which a proper degree of attention will soon discover.

These balls, when dry, are very hard and compact, and require to be broken to pieces with a mallet, and put into a heap, and watered to a due degree, only sufficient to promote fermentation, but not by too much moisture, which would retard it; and here is a crisis necessary to be attended to. When the couch has attained its due point, it is opened, spread, and turned, until regularly cooled, and then it is considered in condition for sale: but the immediate use of woad new from the couch is not advised by dyers who are experienced; for new woad is not so regular in its fermentation in the blue vat. This is the common process. Woad oftentimes is spoiled herein, by people who know nothing of the principles of its dye, following only their accustomed process of preparing it; and hence the difference in its quality is as often seen, as it is in the real richness or poverty of the leaves, from the quality of the land. The process for preparing woad which I have followed, and which I consider beyond all comparison best, is as follows:

Gather the leaves, put them to dry, and turn them, so as not to let them heat, and so be reduced to a paste; which, in fine weather, children can do. In wet weather, my method was to carry them to my stove, and when I had got a quantity sufficiently dry, I proceeded to the couch, and there put them in a large heap; where, if not too dry, they would soon begin to ferment and heat. If too wet, they would rot, but not properly ferment, nor readily become in condition for the dyer. These leaves not having been ground, nor placed in balls on the hurdles, their fermenting quality was more active, and required more attention; and also the application of lime occasionally to regulate the process with the same kind of judgement as used in the blue dying woad vat. When the heat increases too rapidly, turning is indispensably necessary, and the application of very fine flour lime

lime regularly strewed over every laying of them; or, if the couch is getting too dry, lime-water instead of common water, applied by a gardener's watering-pot, may have an equal effect*, without loading the woad with the gross matter of the lime; though I conceive that the gross dry flour lime, and the oxygen in the air, will furnish more carbonic acid gas to the woad, and retain such principles as are essential, to a better effect. For I have experienced, that woad which requires the most lime to preserve a temperate degree of fermentation, and takes most time, is best, so that at length it comes to that heat which is indispensable to the production of good woad.

In this couch it is always particularly necessary to secure the surface as soon as the leaves begin to be reduced to a paste, by rendering it as smooth as possible, and free from cracks: this prevents the escape of much carbonic acid gas, (which is furnished by the lime and the fermentation,) and also preserves it from the fly, maggots, and worms, which often are seen in those parts where the heat is not so great, or the lime in sufficient quantity to destroy them; it is surprising to observe what a degree of heat they will bear. This attention to rendering the surface of the couch even and compact is equally necessary in either process, and to turning the woad exactly as a dung-heap, digging perpendicularly to the bottom. The couching-house should have an even floor, of stone or brick, and the walls the same; and every part of the couch of woad should be beaten with the shovel, and trodden, to render it as compact as possible.

The grower of woad should erect a long shed in the centre of his land, facing the south, the ground lying on a descent, so as to admit the sun to the back part; and here the woad should be put down as gathered, and spread thin at one end, keeping children to turn it towards the other end. In the course of a week, every day's gathering will be dry for the couch, which should be at the other end; therefore it will be necessary to calculate how long the shed should be; but this can be erected as you gather, and then it will soon be known.

I never used the thermometer to discover or determine the heat which is necessary to produce that change of smell which finishes a couch of woad properly for the dyer†, but I am convinced it cannot be regularly obtained but by temperance and time.

* There is in lime-water so little of its salt, that its effect is proportionably small, and water will take up but a certain quantity.

† I suppose from 100 to 120 degrees.

Good

Good woad, such as the richest land produces, if properly prepared, will be of a blackish green, and mouldy; and when small lumps are pulled asunder, the fracture and fibres are brown; and these fibres will draw apart like small threads, and the more stringy they are, and the darker the external appearance and on the green hue, the better the woad; but poor land produces it of a light-brownish green. The fibres only serve to show that it has not suffered by putrefaction.

Considerable fortunes have been acquired by the culture of woad in the North of England, and those who have not in possession land sufficient of proper staple, will give an extra rent for leave to break pasturage; and such as is old, and its sod worn out and full of ant-hills from long feeding, is equally good, when lime is applied to destroy these and other insects, which here exist more than in such as is in full proof to bear grass; for here they generate and become destructive, so as often to render it very necessary to plough such land, corn it, and form a new turf; and though this is so often prohibited, yet it is often consistent with the best principles of husbandry. Here woad is every thing, and corn after it to a certain degree, which experience will determine, according to the kind of land. Those who grow woad in large quantities, have moveable huts for their work-people; and also all their apparatus so easily put together, as to be of little expense except in carriage.

A friend of mine in London took a large quantity of land whereon had been wood just grubbed up. He planted woad on it, and engaged a person from the North to manage it; and the produce was so abundant as to afford immense profit. I believe he only woaded two years, and then let it. His tenant's produce did not by any means equal his, because the land began to want change. I know not how he succeeds in corn, but I presume he did well, as it is a fine preparative for it.

LXI. *Geological Remarks and Queries on Dr.* CAMPBELL'S *Map and Account of the Stratification of Lancashire, in our last Number*, p. 268. *By Mr.* JOHN FAREY, *Senior, Mineral Surveyor.*

To Mr. Tilloch.

SIR, I WAS gratified, as I doubt not great part of your readers would be, at seeing a beginning made in your Philosophical and Geological Magazine, to sketch out the stratification

tification of the different Counties in England, a work which is so much wanted, that I hope gentlemen in other counties will be induced to imitate the excellent example of Dr. Campbell, whose statements are highly valuable contributions towards the geology of the British islands, and I am induced to hope, that that gentleman will take in good part, a few remarks which I am about to make on his paper, and be induced, as soon as opportunities will permit, to furnish the information further wanted, either through the medium of your useful work, or in a letter to me, as may be most agreeable to himself. First, I would remark, that the most important of the four geological divisions of the county, (IV. page 270), coloured blue in the map, is omitted at page 278; I should hope, through an accidental separation of the manuscript sent to the Bath Society, and that Dr. Campbell has it in his power, without delay or material trouble to himself, to supply the account of this district, wherein abundance of coals, and sand-stone, and some red marl, gypsum, and salt springs*, and some lead-mines occur, as well as an interesting bed of limestone in the coal-measures near Ardwick, and thence towards Stockport. It would be extremely desirable to obtain sections, or accurate accounts of the sinkings of the pits or driving of long levels, at as many of the Lancashire Collieries as possible, noticing the direction and quantity of *dip*, and the large *faults* which have been proved, in the several Collieries, with an account, as particular as possible, of the situation and nature of the junction between the red marl and the coal-measures, and whether a large fault effects this separation? as I suspect to be the case, (passing near Stockport, Manchester, Newton, Prescot and Liverpool, perhaps,) or whether the coal-measures dip under the red marl, or *vice versâ?* It is not less important to learn, the exact boundary line of the Coal-measures to the north, whether at a large fault? ranging from near Keighley in Yorkshire, near to Colne, S. of Clitheroe, N. of Blackburn, S. of Preston, and N. of Ormskirk, as I have conjectured, in a paper read before the Royal Society in March last (now lately published), or whether the shale and millstone grit, coloured brown in Dr. Campbell's map, underlays the coal-measures, or *vice versâ?* Whether the limestone, coloured yellow, overlays, or covers, the shale and grit coloured brown, or *vice versâ?* Whether, as there is a thin coal seam ranging out of York-

* See Mr. Henry Holland's paper on the Salt-District of Cheshire and Lancashire, lately published, in the first volume of The Geological Transactions.

shire near Ingleton, past Hornby to Lancaster Common, (p. 278), the Limestone Rock of Chipping and Clitheroe, is not a different one from that of Dalton (not Datton as engraved), Cartmel, Warton, &c.? having this coal seam, and perhaps nearly the whole of the shale and grit (brown) between them? or are they one rock and the connexion complete through Yorkshire and Westmoreland, having the brown upon it W., in a sort of trough or basin? Does not the Limestone of Kendal, Cartmel, and Dalton lay upon the Slate, &c. (green) where these districts adjoin each other across Lancashire? and does not this Limestone Rock, after crossing the Duddon River, proceed somewhere near Ravenglass, Egremont, Cockermouth, &c. in Cumberland? overlaying the Slate, &c. to its East? or does the slate in any part of this course, by approaching the coast, push the Limestone out to sea? does not the Coal-field of Whitehaven and Workington, &c. overlay this Limestone Rock, to the East of these places? and how far South and North, and through what villages, can this junction of Coal-measures *on* limestone be traced? and where does the Limestone Rock, by approaching the coast, push these Coal-measures out to sea?

By consulting the lists at page 433 of your 35th volume, and page 188 of the 1st volume of my *Derbyshire Report*, the concise general view of the Derbyshire and Lancashire Coal-field, which is given at page 172 of that work, and my paper in the recent Philosophical Transactions above alluded to, Dr. C. will be enabled to comprehend more fully the objects of my several inquiries, and which I trust will appear of such importance to him, and others among your readers, as soon to procure answers to them.

The Maps and Sections, and other mineral documents in the possession of the late Duke of Bridgewater's agents at Worsley, and those in possession of Francis Astley, Esq.*, of Duckingfield Lodge, Cheshire, are unusually extensive and complete, I believe; and I doubt not, but on proper application by Dr. C. or any other gentleman, who would kindly undertake the task of collecting and arranging these important materials, that the gentlemen alluded to, and the other Lancashire coal-owners and their leasees, would permit the necessary extracts and copies to be taken, with the same readiness which I have uniformly experienced, in all

* Some of which were made by Mr. Thomas Bartley, mineral surveyor, (formerly an assistant to Mr. William Smith, the father of mineral surveying,) of No. 104, Chancery Lane, London, as I am informed.

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my extensive inquiries, among the coal-masters of the adjacent counties to the south and east: and the services to science would be great indeed, as well as an important favour conferred on

Your obedient servant,
JOHN FAREY, Senior.
Mineral Surveyor.

12, Upper Crown Street, Westminster,
November 6, 1811.

LXII. *On the Staphyloma, Hydrophthalmia, and Carcinoma of the Eye.* *By* JAMES WARE, *Esq., F.R.S. and Vice President of the Medical Society.*

[Continued from p. 304.]

I NEXT proceed to consider the disorder called Hydrophthalmia. By this term authors do not in general mean an accumulation merely of the aqueous humour, but so great an enlargement of the whole eye, produced by an increase of the vitreous humour as well as the aqueous, as to cause the eye to occupy an undue portion of the orbit, and to occasion difficulty and pain when the eyelids are closed over it. Thus defined, it may perhaps with more propriety be denominated Exophthalmia than Hydrophthalmia*. In describing this disorder, a greater discrimination is required than seemed necessary in the former part of this paper. In the staphyloma, for instance, the opake projecting cornea designates the nature of the disorder in so plain a manner, that it seems impossible to make a mistake with regard to its nature. But in the hydrophthalmia, which implies an universal enlargement of the eye, some examination is requisite in order to ascertain what occasions the enlargement: whether there be an equal enlargement of all the different parts of the eye; a morbid enlargement of one particular part only; the formation of an adventitious body within the eye; or a projection of the eye in consequence of a substance formed behind it.

* Scarpa is of opinion that an accumulation of water between the choroid coat and retina is a common cause of the hydrophthalmia, and he minutely describes a case of this kind which occurred in a child three years and a half old, in which the eye was a third larger than its natural size, the cornea partaking of the increase, in the same proportion as the sclerotica. I have several times observed, on dissecting the eye after death, that there has been an effused fluid between the choroid coat and retina, the vitreous humour being wholly absorbed, and the retina collapsed into a cylindrical, or rather a conical, chord like substance, its apex arising from the optic nerve, and its basis surrounding the crystalline humour; but, though this effusion had produced a fixed dilatation of the pupil, an opacity of the crystalline, and sometimes a violent deep-seated pain in the eye, I have never known it to occasion an enlargement of this organ.

 Infants

Infants are sometimes born with eyes remarkably large and prominent. But if they do not give by their pressure, nor interfere with the free motion of the eyelids, and if at the same time the cornea be transparent and the sight perfect, the mere circumstance of their prominence does not call for any particular attention. Sometimes, however, the eyes of infants, at the time of their birth, are not only remarkably prominent, but the cornea of one or both is universally opake, without any accompanying inflammation in the conjunctiva, or any morbid discharge from the eyes. Of this I have seen several instances, three of which happened in one family. These were more directly under the care of Mr. Farrer, a surgeon, resident at that time at Deptford. He has described them with accuracy in the second volume of Medical Communications, page 463, published in London in 1790. The opacity gradually diminished; and in less than a year, in two of them, it was quite removed. In the third the cornea did not resume its transparency until the end of the second year. The amendment in these instances cannot be attributed to any particular remedies, since none were used; but it was owing to the *vis naturæ medicatrix*, which in infants, in this disorder, as it also is in many others, is often effectual to restore a healthy state. Mr. Farrer does not mention any particular prominence in the eyes of these children; but, having seen two of them shortly after the time when Mr. Farrer drew up the account of the cases, I find by a minute I then made, that the cornea appeared to me remarkably prominent; and that, though the children had recovered a distinct vision, they were all short-sighted.—Another case of a similar kind came under my notice about three years ago, in the new-born infant of a respectable farmer in Essex. Both corneæ were completely opake, and both were large and prominent. In this instance, as in those last mentioned, no applications were used with sufficient steadiness to allow me to attribute any considerable degree of efficacy to them; notwithstanding which, when about four months ago the child was again brought to me, I had the satisfaction to see the left cornea sufficiently clear to allow the perception of all large objects; the opacity of the right cornea being also diminished round its outer edge, though the greatest part of the pupil was still obscured. I was consulted in a fifth case of the same kind about a year ago. It occurred in the infant of a gentleman in Portman-square. Here, as in the other instances, the corneæ of both eyes, at the time of birth, were large and prominent, and they were at the same time completely

completely opake; the child, in other respects, being healthy, and suffering no pain from the state of the eyes. Sanctioned by the successful issue of the preceding cases, no particular remedies were employed; and at the time of my writing this paragraph, which is just a year from the birth of the child, the cornea of one eye is not only perfectly transparent for a considerable space round its circumference, but the pupil can be seen through the diminished opacity that remains in its centre; and though the cornea of the other eye has improved less in its appearance, the transparency of this also is evidently increased, and the iris is visible through it, for the space of a line at least round its rim.

In all these instances, the enlargement of the eye was not sufficient to be of serious consequence independent of the opacity of the cornea; and, when this opacity was dissipated, the power of vision was restored. But when, on the contrary, the enlargement is not confined to the cornea, but extends to the sclerotica, and is so considerable that the eyelids cannot be closed without difficulty, the patient being not only blind, but unable to sleep without the aid of opiates; the prospect of restoring sight is wholly lost, and the only question is, in what way ease may be obtained, and deformity obviated. It does not appear possible to do more than this; nor can even this be accomplished by any other mode than that of diminishing the size of the eye: and the best manner of doing it I believe to be by means of the operation which has been recommended above in cases of the staphyloma.

Before an operation of so much importance be performed, it is, however, essentially requisite to ascertain that the disease consists solely in an enlargement of the different parts of the eye; and that it is not produced by the formation of purulent matter within the eye; by a morbid alteration in the structure of either its coats or humours; nor by the undue accumulation of adeps, or of any other substance, behind this organ.

When purulent matter is accumulated within the eye, the inflammation and pain, which both precede and accompany the enlargement, seem fully sufficient to distinguish the peculiar nature of the disorder; and they at the same time point out the necessity of procuring an adequate aperture in the tunics of the eye, through which the matter may be discharged. In a case of this kind, which I was desired to see at a small distance from London, in which a young lady, nine years of age, had suffered agonizing pain several days, the sight of the eye having been lost many years, and

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the cornea being both opake and prominent, an aperture had taken place spontaneously on the side of the eye next the temple, just in that part where the cornea is joined to the sclerotica, and through it a small portion of matter had escaped; but the tension of the eye continued, and the wound was only large enough to admit the blunt end of a probe. The propriety of enlarging the aperture naturally suggested itself; and as the eye had not been useful for a long time as an organ of vision, a small blunt-pointed bistoury was immediately introduced through the wound, to the depth of at least a quarter of an inch, and the incision was carried three quarters of an inch in a direction towards the temple, dividing at the same time the sclerotica choroides and retina, and making a large opening into the body of the vitreous humour. No part of this humour, howover, nor any sort of fluid, issued through the wound at the time of the operation. The eyelids were immediately closed, without any pressure being made on the eye, and directions were given to apply an anodyne fermentation, in the same way in which it had been frequently before used. An anodyne draught was intended to be given; but within half an hour the patient fell into a sound sleep, which lasted several hours. She awoke much refreshed and perfectly easy. The wound discharged more or less of matter for a fortnight; the pain did not return; and the eye gradually diminished, so that in a short time it did not appear to be more than one half of its natural size.

Purulent matter is sometimes also formed behind the eye in the adipose substance that supports this organ in the orbit. If the suppuration be quick in its progress, and be not situated deep, the fluctuation of the matter may be easily felt, and the propriety of discharging it be determined at once; but if, as I have occasionally found, the suppuration be slow, and the matter lie considerably below the surface, the eye will be protruded before any fluctuation can be discovered; and the existence of the matter will only be learned by paying attention to the accompanying symptoms, such as a quick pulse, white tongue, shiverings, &c. In a case of this kind, which occurred in a child six years old, which was attended also by Mr. Hill in Bedford-row, I passed a lancet, on the side of the eye next the nose, a little below the commissure of the eyelids, at least an inch into the orbit, before I reached the matter. On withdrawing the instrument its point was evidently marked with pus. I therefore enlarged the aperture with a blunt-pointed bistoury, and discharged a considerable quantity, which was thick and putrid,

putrid. It was necessary to preserve the opening by the insertion of a small dossil of lint; on the removal of which, a vent was given daily to new matter, for a fortnight. Its quantity gradually decreased, together with the prominence of the eye; and at length it wholly ceased, the wound healed, and the child became well. The motion of the affected eye, however, was not quite free toward the nose for several months afterwards.

Encysted tumours are sometimes also found in the adipose substance that supports the eye. A melancholy instance of this kind came under my notice a short time ago. The tumour was first perceived between the orbital process of the os frontis and the globe of the eye, and it gradually increased in size. An attempt had been made to extirpate it; but the greater part was situated so deep, that it was not possible wholly to remove it; and, after a short period, it reappeared, and in a few months completely pushed the eye out of the orbit; after which vision was destroyed, and the eye and the tumour became so blended, as to render it impossible to distinguish one from the other. The united mass increased continually in size, until, before the child's death, it was literally larger than his head*. Another case of this kind came under my care, about the same time, in a girl about five years of age, who was a patient of Mr. Drew in Gower-street. The tumour had been perceived several months, and, when I first saw it, projected under the upper and outer edge of the orbit, and began to push the eye out of its place. In this instance I made an incision through the eyelid, parallel to the edge of the orbit, sufficiently deep to expose the whole of the fore part of the cyst. I then separated the cyst from the orbit, and, embracing it with a hook, drew it forward, and, dissecting it from all its attachments, brought it away entire. The sides of the wound were aftewards kept together by the use of adhesive plaster, and the cure completed in a few days.

In some instances, again, a projection of the eye appears to be occasioned solely by a morbid accumulation of the substance on which the eye rests in the orbit. The repeated application of leeches on the temple and forehead, has been found of great use in subduing this morbid tendency. In one case, that came under my own care, the projection was speedily diminished by opening the temporal artery; and,

* I presume that this may be considered a case of fungus hæmatodes; though it originated in an encysted tumour situated between the bony orbit and globe of the eye, and did not affect the sight until the eye was thrust out of the orbit.

after the hæmorrhage had ceased, by converting the orifice into an issue, the discharge from which became soon very considerable. In another case, in which the protrusion occasioned great pain, and nearly destroyed vision, a perfect cure was accomplished by the application of a large caustic behind the ear. The discharge which it occasioned, when the eschar separated, was profuse; and it was kept up, nearly a month, by the insertion of a dozen peas daily.

Another disorder of the eye, which gradually occasions its enlargement, has by some been called fungus hæmatodes; and by others medullary sarcoma, spongoid inflammation, and soft cancer. This differs so much, both in progress and appearance, from the hydrophthalmia, that it cannot easily be mistaken for it. It more nearly resembles the disorder which I proposed to consider last in this paper, the Carcinoma of the eye, having many symptoms in common with it. The fungus hæmatodes seldom attacks the eyes of adults, and is most commonly discovered at an early period of an infant's life. The first symptom that is noticed is a white shining substance in the posterior part of the eye, visible through the pupil in some particular positions of the head, but not in all. One eye is generally attacked some time before it appears in the other. As soon as the whiteness is perceived in the eye, the sight is impaired, and in a short time it is wholly lost. At its commencement it bears a slight resemblance to a cataract; but an attentive person will at once discover the difference between the two disorders; the opacity in the cataract lying close behind the pupil, whilst in the fungus hæmatodes it is situated deep in the posterior part of the eye. In the cataract, the pupil retains the power of dilating and contracting in different degrees of light; but in the fungus hæmatodes the pupil never varies its size, and is usually dilated. When the disorder has so much advanced as to destroy the figure of the eye, and to make it protrude beyond the rim of the orbit, it is more difficult to distinguish it from what has usually been called a carcinoma of this organ. There is still greater difficulty, when, after extracting an eye that contains a fungus hæmatodes, a fresh tumour arises from the bottom of the orbit, which fills this cavity, and continues increasing, until it becomes, as has sometimes happened, as large as the whole head. This difficulty of distinguishing between the carcinoma of the eye and the fungus hæmatodes is, however, the less to be regretted, since the proper treatment of both disorders seems nearly alike; the only known mode of checking the progress, in both, ap-

pearing

pearing to be the complete extirpation of every part that is diseased. Before recourse be had to the operation, it is necessary to ascertain, as far as possible, that every such part is capable of being removed; since, in both disorders, if the smallest portion that has been contaminated remain, whether it join the organ that is extirpated, or be at a distance from it, the diseased part will infallibly increase, and all the old symptoms be reproduced. The fungus hæmatodes is not always confined to one eye, nor even to both, but sometimes occupies a large portion of the orbit exterior to the tunics of the eye. It is also accompanied not unfrequently with abscesses and tumours in different parts of the head; sometimes between the pericranium and cranium, and at other times between the cranium and dura mater. These abscesses are not confined to the fore part of the head, having sometimes been found both on the outside and inside of the os occipitis. Distinct portions of matter, and sometimes hard tumours, have also been formed in the dura mater, and even in the substance of the cerebrum; and sometimes under the anterior lobes of the cerebrum, making a compression on the thalami nervorum opticorum. A disease of this kind is by no means new. It has occasionally come under my notice ever since I was a boy; and it has been described by many of our ancestors under the common name of carcinoma, or cancer. It may be more correct, however, to distinguish it by the term fungus hæmatodes, or medullary sarcoma, though it does not appear to me to be always easy to ascertain the difference between the two disorders. It has been said that carcinomatous affections are always preceded by a hard circumscribed tumour, and that, after an ulceration has been produced, if it be followed by a fungous excrescence, this is of a cauliflower figure, and a hard firm texture; but such cannot be admitted to be the universal progress of these affections, nor is it unlike to that which the fungus hæmatodes sometimes assumes. It may be said with greater correctness, that the carcinoma of the eye is a disease to which persons are most subject in the middle or latter part of their lives, whereas the fungus hæmatodes appears in early life, and most commonly in infancy.

The following is the progress of a disorder which I have also repeatedly seen in persons advanced in life, but do not remember in any who were young. By some it may be called fungus hæmatodes, and by others carcinoma; but I shall content myself with describing it. The sight is lost before

before any change takes place in the appearance of the eye: after this the pupil becomes dilated without any visible opacity in the crystalline humour. This description designates a gutta serena; but the disorder does not stop here. After a little time the crystalline humour becomes opake; and soon afterwards shooting pains are experienced, which dart suddenly through the eye in different directions, rarely continuing long at one time. At this period, if the sclerotica be carefully examined, a blueish, or rather a dusky leaden-coloured spot, of greater or smaller extent, will be discovered in it, on one side of the cornea, and sometimes on both. These blueish- or leaden-coloured spots gradually spread; the eye enlarges either partially or generally; and in a short time it pushes forwards the eyelids, and fills the whole of the orbit. In some instances the blueish enlargements appear as if they were affections of the outer surface of the sclerotica, and only covered by the tunica conjunctiva. In others they are evidently produced by a distention of the whole substance of the sclerotica, which is pushed out and thinned, where the projection appears by the accumulation of a morbid substance within the eye. A few of the blood-vessels of the conjunctiva are usually enlarged, and have a purplish-red appearance, very different from that which is produced by a common inflammation. On examining the internal state of these tumours, after their extirpation, the whole of the eye has been found full of the leaden-coloured substance I have described; divided, in an irregular manner, by membranous laminæ into separate cells, the contents of which have varied much, even in the same eye, in their degrees of consistence. They are usually firm and solid, but sometimes contain pus in separate cysts, and sometimes also osseous particles that differ much in their shape and size. These tumours are in general produced by an irregular enlargement of the whole eye, involving both its coats and humours; but sometimes the humours are very little altered, the disease seeming to originate in an affection of the tunica sclerotica, which spreads outwards rather than inwards. Sometimes the tumour is confined to one side of the eye, its other side being unaffected. At other times it occupies both sides; and, occasionally, there have been three tumours annexed to the eye, one on each side and one above, all as large as the eye; this organ being unaltered in size, though deprived of sight.

The progress which the disorder makes is very various. Sometimes a prominence of a leaden colour has continued

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in the substances of the sclerotica, on one side of the cornea, many years, without giving pain or occasioning any sort of trouble; and, on the contrary, it has at other times increased rapidly, and the enlarged organ in a few months has completely filled the orbit*. It does not appear that medicines or applications have the power of checking or controlling this malignant disorder; and whenever its nature can be clearly ascertained, the only question is, whether it be possible to extirpate completely every part that partakes of the poison. Although it be a melancholy truth that the operation has too often failed, this does not lead to the conclusion that its performance is always improper, since it certainly has not unfrequently succeeded; and I have the satisfaction to say, though I have sometimes failed, I have several times performed it with complete success.

With regard to the mode of performing the operation, I would advise it to be done in the following manner.

The patient should be seated in a clear light, on a chair of a suitable height to bring his eye on a level with the breast of the operator; and the operator should either sit or stand before him, as is most easy to himself. The patient's head should rest against the breast of an assistant, whose left hand should support the upper eyelid by means of a double blunt-pointed hook, the points of which are seven-eighths of an inch distant from each other, and his right hand should be at liberty to do any thing that may be desired by the operator. The hands of the patient should be held by two assistants that sit one on each side, and an assistant should be ready to give the operator instruments, sponges, &c. A crooked needle armed with a strong thread, and well waxed, should then be passed through the whole of the

* Since these papers were put together, I have extirpated an enlarged eye from a gentleman, thirty years of age, who had lost the sight of it many years; but it occasioned no pain or inconvenience until about six months ago, when it began to enlarge, and an increase in its size had afterwards been perceived almost every week. The enlargement of the eye was universal; the blood-vessels had a purplish red appearance; there were three blueish spots on the sclerotica, one of which was as large as a sixpence; and the pressure of the eye against the eyelid kept up a constant uneasiness. In a consultation with Mr. Cline, it was judged advisable, as the increase of the tumour was rapid, to recommend the extirpation of it without delay; and, the patient giving his consent, I performed the operation, in presence of Mr. Cline, a few days after the consultation. Nothing unusual occurred at the time; and on examining the tumour afterwards, the humours of the eye were found to be no otherwise affected than by their enlargement, the blue appearances being occasioned solely by an affection of the sclerotica. No accident happened after the operation, and in less than a month the wound was healed, and the patient returned, perfectly well, to his home in Kent.

the cornea; after which, the needle being cut off, a knot should be tied in the thread, at the distance of about an inch from the eye, to hinder it from slipping. This thread is more useful in cases where the eye is so much enlarged as nearly to fill the orbit, than when it is smaller; the finger alone, in the former case, being insufficient to incline the tumour from one side to the other, so as to make the room that is required for the proper use of the knife. If the tumour be considerable, the upper and lower eyelids should next be separated, by dividing with the knife the integuments which unite them on the side next the temple. This will give much additional room for the introduction of the knife to dissect the diseased organ from its attachments. The conjunctiva should then be divided round the whole globe of the eye; and afterwards the knife be carried downwards, on that side where it passes with the greatest ease. It is not possible to give precise directions, as to the mode in which the dissection should be conducted; but great care should be taken to avoid wounding the tumour until the point of the instrument has reached the bottom of the orbit. If it be possible, the operator should introduce his finger with the knife so as to feel the optic nerve, which, together with the muscles of the eye, should be divided as close to the foramen opticum as the instrument can be carried. In general the common straight scalpel may be so directed as to perform this part of the operation with accuracy; but if the tumour completely fill the orbit, it may be useful, in this part of the operation, to substitute for the straight scalpel one that is a little curved. As soon as the optic nerve and muscles of the eye have been divided, the tumour becomes loose, and may be easily drawn out of the orbit, either by the fingers, or by the ligature that was passed through the cornea at the beginning of the operation. The tumour, when removed, should be carefully examined, in order to ascertain if it be entire, or if it be wounded in any part. In the latter case, the orbit should be carefully examined, both with the eye and the finger; and if any portion of the tumour be seen or felt, it should be dissected away. The state of the nerve should also be examined. If this appear white, and of its natural size, a hope may be entertained that the operation will prove successful; but if it appear of a leaden colour, or be altered in shape or size, there is too much reason to fear that the disease has passed beyond the part which has been removed, and that, sooner or later, a fungus will arise in the orbit, and all the old symptoms

ptoms be reproduced. The hæmorrhage consequent on the operation is seldom considerable. The arteries that supply the eye with blood are not large; and if a little time be allowed, those that are wounded will contract of themselves. It is desirable to avoid the application of lint or of any other substance within the lids, since it sometimes has given considerable pain; and, in one instance, in which the operation was performed by an eminent surgeon, it was supposed to occasion violent convulsions by its pressure against the divided end of the nerve. It is sufficient to apply over the eyelids a compress of old linen, folded six or eight times, and moistened with the liquor plumbi acetatis dilutus; and to direct the compress to be remoistened, without removing it, as often as it becomes dry. If by accident the eyelid be wounded during the operation, care should be taken to bring the divided ends together, and to confine them in their natural position either by means of sticking-plaster, or of a suture with a small needle and thread. Care should also be taken, before the compress be applied, to adjust the edges of the upper and lower eyelids, so as to hinder one from lopping over the other. If, after the operation, the pain continue violent, an anodyne should be given; and, if necessary, it should be repeated after three or four hours; but its repetition, I believe, will seldom be required. Sometimes, after a week or ten days, the upper eyelid is observed to tuck in under the lower; in consequence of which the upper lashes, by rubbing against the inside of the lower lid, have been known to keep up a painful irritation. This may be obviated by fixing the end of a slip of adhesive plaster on the upper lid, and continuing it lengthways on the forehead, sufficiently tight to make a fold in the skin and hinder the edge of the lid from turning inwards. Cooling medicines, and a spare diet, are necessary for a few days; but afterwards a light preparation of cinchona, together with a nutritious diet, will be required. As the wound heals, an adhesion usually takes place between the inside of the eyelid and the bottom of the orbit; and when this happens, it is not possible to give the patient the benefit of an artificial eye, as is done after the operation for the staphyloma or the hydrophthalmia; and he must be contented either to wear a compress, bound by a ribband over the orbit, or a pair of spectacles, having plain glass in the ring before the good eye, and glass that is either plain, or in a slight degree opake, in that before the affected eye.

If, unfortunately, after a careful extirpation of a carcinomatous eye, a tumour again arise in the orbit, it is vain to expect

expect benefit from a second operation, and applications of a painful kind should be avoided as much as possible. Art does not appear to be capable of doing more than to palliate the violent symptoms; by anodyne remedies, by evacuations local or general, and by tonic medicines, when the state of the general health renders these expedient.

LXIII. *Some Speculations on the Nature of Instinct.* By ARTHUR MOWER, *Esq.*

[No. II.]

THAT great portion of nature which we call the animal kingdom, may be considered as consisting of two great divisions; the one embracing human beings destined by their Creator for another state of existence, and the other comprehending every other living creature, whether beast or bird, fish or insect;—the lion and the ephemera, the eagle and the smallest of the finny tribe;—beings, whose consciousness we suppose to cease when they cease to breathe. When we consider the different varieties of the human species, and reflect on the different manners and customs of each nation and tribe of men who dwell upon the earth, we shall find, that however great the difference produced by civilization between one man and another, those constitutional laws by which we are governed are in every clime invariably the same. The natural wants of the European are the same with those of the most untutored Indian; they have the same appetites, the same passions; but in one, those passions are so modified, coloured, and refined by luxury, as almost to lose their original simplicity; whilst in the other we see nature undisguised by art, and controlled only by her own laws. Between savages and Europeans, as between different individuals in every nation, there appears to exist some natural mental inferiority. Attempts which have been made to civilize individuals of the Hottentot nation, have, as it is well known, completely failed. Their minds, unusually contracted, seem to be incapable of cultivation; or only fitted to receive such simple ideas as are conducive to the wants and comforts of a savage life. On a first view, indeed, there is apparently very little difference, or mental superiority, between individuals of the brute and the human species: their actions seem to be regulated by the same laws, and their desires and appetites to be produced by one common principle. If we compare together the savage, whose pleasures and desires are confined

confined to the gratifications of sense; the polished European, pampered by luxury, and abandoned to every species of civilized depravity; and the faithful dog, whose days are spent in the service of his master,—to which of these animals shall we allow the claim of superiority over the other two? In point of useful respectability, I believe no one will hesitate to prefer the dog: but, however low and degraded the human species may be found, the signs of a superior intelligent principle will be sufficiently plain to mark, distinctly, a barrier between man and the brute creation;—the glimmerings of an immortal spirit will sometimes break through the gloom, and "render" not only "darkness" but greatness "visible."

Metaphysical speculations are so frequently and necessarily founded on theory and conjecture, that it perhaps is little wonderful that there should exist so few established principles respecting the striking analogy which exists between the actions of our species and those of other animals. It is not to be expected that bricklayers and labourers should argue on the sentient principle;—but that the generality of well-educated men should have no determinate idea either of themselves or of the animals which surround them, is at least a proof that almost every other science is found more engaging, and thought more useful, than that of metaphysics. Ask a man of common abilities, and who has received what is termed a liberal education, what is the mental difference between himself and the dog which lies at his feet, and he will tell you thus:—"A man is composed of a body, and a spiritual principle which we call the soul, and which (from Revelation) we know to be immortal. The province of the soul is to reason, and it is our business to act according to the conclusions we are enabled to draw from a comparison of our ideas. The actions of that dog frequently appear to be the effect of reason; but reason implies an immortal soul, which to imagine for a moment existing in brutes, is in the highest degree shocking and absurd. I therefore believe that brutes are guided by a principle called Instinct; or, by a certain law which they can neither alter nor control. The operation of this law is as unerring and irresistible as the law of gravitation; and we may as well attempt to prove why two drops of water adhere together, as to account for the impulse which leads my dog to follow me, and to bark when he sees a stranger. Of the nature of instinct or the human soul I pretend to know nothing. Metaphysical conjectures are 'weary, stale, flat, and unprofitable;' the business of life can be carried on

as

as well without them, and they neither serve to make us really wiser, nor more contented."—That such is a common opinion among those who think at all upon the subject of instinct, will, I think, not be denied.

Instinct, according to Dr. Reid, is a natural blind impulse to certain actions, without having any end in view; without deliberation, and very often without any conception of what we do. It has again been defined to be a tendency implanted in the mind of animals, when under the influence of certain feelings or sensations, to perform spontaneously, unerringly, independently of all teaching and experience, and without any determinate view to consequences, certain actions necessary for the preservation of the individual, and the continuance of the kind(1). The latter definition appears to be a very just one; and there is not the least doubt, that many actions, both of men and brutes, are performed under this mysterious tendency. No action, perhaps, is more decidedly instinctive, than the sucking of the new-born lamb. This little animal is never thought to have any distinct idea that its mother's milk is necessary for its existence, nor is it ever suspected to know by inductive philosophy that if it does not suck it will inevitably die; but it evidently acts under the uncontrollable influence of a certain law, which impels to the teat the young of every animal, whether a lion's cub or the offspring of a woman. If the analogy between a wolf and a man could only be traced in their alike acting from a principle whose operation is in both involuntary, and as unerring as the law of gravitation, the common opinion of men concerning the brutes would undoubtedly be just. Exclusively distinguished by the gift of reason, we might then look down from an immense height on the lower animals; deny, with Descartes, that they possess a soul; and with him consider them as mere automatons, moved by clock-work. But are we authorized by the mental phænomena which the actions of brutes daily exhibit, to deny that they possess a soul as well as ourselves? Soul is a word by which we understand that principle in man which possesses consciousness, a power of perception, memory, intelligence, and volition(2). That faculty of the soul which chiefly distinguishes man from the brute creation, is that of reason, or a power of drawing conclusions, from a comparison of our ideas, by which to regulate our actions. But a very short observation of the manners of the brute creation would, one should imagine, convince every one, that the lower animals act from reason as well as ourselves. To prove this, innumerable instances might

might be mentioned. The following, from Dr. Rees's Cyclopædia—article *Instinct*,—is well worth quoting:—"A lady, with whom we were acquainted, had a tame bird, which she was in the habit of letting out of its cage every day. One morning, as it was picking crumbs of bread off the carpet, her cat, who always before showed great kindness for the bird, seized it on a sudden, and jumped with it in her mouth upon a table. The lady, alarmed for the fate of her favourite, on turning about observed that the door had been left open, and that a strange cat had just come into the room. After turning it out, her own cat came down from her place of safety, and dropped the bird, without injuring (if we may so express it) a hair of its head." Will any one assert that this cat acted from "a natural blind impulse, without having any end in view," or "without any determinate view to consequences?" We might as well say, that to plunge into the water to save the life of a fellow-creature, is an instinctive involuntary action. This sagacious animal perceiving the entrance of a strange cat into the parlour of her benefactress, by the recollection of her own predilection for bird's flesh, had sufficient intelligence to know that the bird on the carpet was in danger from a similar appetite in a strange individual of her own species. Here was the exercise of reason in a very high degree; complex ideas, both of observation and experience; the latter, recalled by an act of memory, must have passed through the mind of the cat with inconceivable rapidity. She acted from a certain motive, which we may naturally suppose was that of attachment to the bird; and she certainly appears to have had in view a determinate end, which was the preservation of her little companion. She perceived, she felt, she recollected, she willed, and she acted. That the cat in this instance was a free agent, and exercised the power of volition, cannot be denied; for powers and feelings to be properly instinctive, must be in common to the species; and the free agency of this animal is incontrovertibly established, if we can suppose that any other individual of the feline race would not have acted with the same feeling and intelligence, in a similar situation. If, then, we observe one of the brute creation displaying all those powers and faculties which are used for the definition of that principle which we call soul,—consciousness, a power of perception, memory, intelligence, and volition,—why should we hesitate to ascribe similar phænomena to a similar cause? why should we refuse to believe that brutes are animated by a sentient principle, analogous in its essence

 to

to that of the human species?—It is, no doubt, easy to mention numberless instances to prove that many of the lower animals constantly act under the direction of instinct; and that many others, who in some cases appear to act from reason, at other times, when we should most expect them to reason, are mere idiots. But this merely proves that there exist different degrees of intelligence amongst brutes as well as men;—but who ever doubts that a dunce has a soul as well as a man of genius? The phænomena which we may daily observe in the animal kingdom, exhibited by men and brutes in their actions, their manners, and their customs, bring us, I think, to this conclusion;—that in some cases they both alike act under the involuntary influence of a certain principle, which is called Instinct. For the preservation of the individual, and the propagation of the species, Providence has ordained that certain feelings and sensations shall invariably impel all his creatures to certain actions, which shall contribute to those two great ends: and this, I think, is the truest explanation we can give of instinct. But from observation and experience, we may likewise conclude, that the lower animals are in many instances free agents, as well as men; that both possess a soul, or sentient principle, analogous in its essence, but differently modified; and that the inferiority of brutes to the human species, the disparity of intellect which naturally exists between individuals of both kinds, is owing to this difference of modification in that cogitative substance called the soul.——I will hazard a few more remarks on this subject, in your next or the following number.

LXIV. *An Account of the Growth and Processes of Mealing, Malting, and Brewing, of the Northern naked Barley. In a Letter to the Bath and West of England Agricultural Society.* By R. Flower, *Esq.**

Marden, near Hertford, April 1, 1810.

Gentlemen, When I had the honour of being present at a meeting of your Society in February 1809, amongst the many subjects then discussed were the qualities and merits of the Northern naked barley.

As no accurate statement was brought before you of any experiment by which its value could be ascertained, I beg leave to recommend to your attention the following account of the growth and processes of mealing, malting, and brewing of the Northern naked barley.

* From vol. xii. of the Society's Letters and Papers.

On

On the 12th of May, 1809, I sowed five acres of it after a mixed crop of turnips and cabbages, which were fed off by sheep in the latter end of April and the first week in May. This crop, being very abundant, kept the sheep longer on the ground, which was on this account in some degree better manured than my other land.

Although this barley was so late sown, it was ready to cut a week sooner than my English barley, and came to maturity a month sooner; which is doubtless an advantage to the husbandman, as the crop of barley on the latest-fed turnip land often suffers.

Of the produce I can only speak comparatively, as it was not large; a long drought in the summer burnt our light-land crops, and this suffered with the rest. I had but two quarters of English barley per acre; of the naked, four quarters one bushel. It came up well, and had a luxuriant appearance during the dry season.

I sent a bushel of each sort of barley to a neighbouring mill, requesting each might be ground and dressed into one sort of flour; the bran only being taken out; and an accurate account of the weight of each sent to me, which was as under:

		Pecks.	lbs.		lbs.	
Foreign—	Flour	2	8	or	36	
	Bran	1	3		17	
		Total			53	when returned from the mill.
English—	Flour	1	10	or	24	
	Bran	1	6		20	
		Total			44	when returned from the mill.

Each bushel of barley lost 4 lbs. in the process of its manufacture. It will be observed, that the foreign barley made 12 lbs. more of flour per bushel than the English, which is within 2 lbs. of seven pecks per quarter; and at the computed value of 2*s*. 6*d*. per peck, amounts to 17*s*. 6*d*. worth of flour more per quarter than was obtained from English barley of the last year's growth.

In the course of the winter I malted six quarters: it worked but indifferently on the floors, having many hard corn amongst it; but this I consider as the defect of almost all the barley of the year 1809. Its swell in the cistern was much greater than English barley, being from six quarters equal to our usual steeping of twenty quarters. I had also a large increase in the making, having nearly two bushels in six quarters, which is much more than it is usual to obtain from the best barley on our plan of making malt.

 On

On brewing this malt, I had the satisfaction to find the wort tasted much richer than that brewed from English malt*. My instrument (Richardson's saccharometer) confirmed my observation, having extracted 12 lbs. more of saccharine matter per quarter than from the English malt.

The result of these different experiments appears to be in favour of the Northern naked barley as follows:

	s.	d.
Nearly seven pecks of flour per quarter more than obtained from English barley, at 2*s.* 6*d.* per peck	17	6
In its malted state, 12 lbs. more of saccharine matter per quarter extracted than from English malt, at 1*s.* 6*d.* per lb.	18	0

From this account it may be fairly presumed, that the Northern naked barley is worth from 17*s.* to 18*s.* more than the English, for the purposes of mealing, malting, and brewing.

Wishing this communication may prove useful to agriculturists in general, and acceptable to this Society,

I remain your obedient servant,

RICHARD FLOWER.

LXV. *On the Impropriety of assigning new Meanings to the established* MARKS *used in Science: and on observing the Directions and Dips of* STRATA. *By a* CORRESPONDENT.

To Mr. Tilloch.

SIR, ON reading the printed proposals for *a Mineralogical Survey of the County of Salop*, issued in May 1810, by Mr. Arthur Aikin, I was much struck, on examining the specimen of his intended Maps, which is annexed to this prospectus, to find that small *arrows* are there used, not to denote by their heads or points, the *dip* or fall of the measures, as has on all previous occasions, I believe, been the case, and as is particularly recommended on geognosical Maps in that highly important volume, the first of the Wernerian Transactions, not to mention their invariable use on Maps in general, to mark the *descent* of the streams, to find that Mr. A. proposes, to apply them to mark the *rise* of the Strata, or turns their barbéd points the contrary way to what has ever before been done: I hope that this was a mere oversight, and that Mr. A. will be induced to restore the

* Mr. Flower has since informed the secretary, that the beer proves excellent.

known

known meaning of these marks in his Mineral Maps, which are so anxiously expected.

In examining the directions and dips of the Strata, it is hoped that Mr. A. will be particularly careful to discriminate in all cases, between the *folia* or stratula of thick beds, or of unstratified masses (or their accidental parallel fissures), and the regular strata-seams or partings of the strata: which, it is thought, could not always have been done by M. De Luc, in his 2d and 3d volumes of Geological Travels in England, in so invariably stating the dip of the strata to be, to or from, and never across the numerous valleys and combes, he describes, as *his theory* of their formation requires.

LXVI. *On the Solar Eclipse which is said to have been predicted by* Thales. *By* Francis Baily, *Esq.**

There is probably no fact in ancient history that has given rise to so many discussions, and to such a variety of opinions, as the solar eclipse, which (according to Herodotus) is said to have been predicted by Thales; and which, owing to a very singular coincidence, put an end to a furious war that raged between Cyaxares king of Media, and Alyattes king of Lydia.

According to the account given by that celebrated historian, "the contest had continued during five years, with alternate advantages to each party: in the sixth, there was *a sort of nocturnal combat*. For, after an equal fortune on both sides, and whilst the two armies were engaging, *the day suddenly became night*. Thales, the Milesian, had predicted this phænomenon to the Ionians; and had ascertained the time of the year in which it would happen. The Lydians and the Medes, seeing that the night had thus taken the place of the day, desisted from the combat; and both parties became desirous of making peace." —*ἐν τοῖσι πολλάκις μὲν οἱ Μῆδοι τοὺς Λυδοὺς ἐνίκησαν, πολλάκις δὲ οἱ Λυδοὶ τοὺς Μήδους· ἐν δὲ καὶ νυκτομαχίην τινὰ ἐποιήσαντο. διαφέρουσι δέ σφι ἐπὶ ἴσης τὸν πόλεμον, τῷ ἕκτῳ ἔτεϊ συμβολῆς γενομένης, συνήνεικε ὥστε τῆς μάχης συνεστεώσης, τὴν ἡμέρην ἐξαπίνης νύκτα γενέσθαι. τὴν δὲ μεταλλαγὴν ταύτην τῆς ἡμέρης Θαλῆς ὁ Μιλήσιος τοῖσι Ἴωσι προηγόρευσε ἔσεσθαι, οὖρον προθέμενος ἐνιαυτὸν τοῦτον ἐν ᾧ δὴ καὶ ἐγένετο ἡ μεταβολή. οἱ δὲ Λυδοί τε καὶ οἱ Μῆδοι ἐπεί τε εἶδον νύκτα ἀντὶ ἡμέρης γινομένην, τῆς μάχης τε*

* From the Philosophical Transactions for 1811, part ii.

ἐπαύσαντο, καὶ μᾶλλόν τι ἔσπευσαν καὶ ἀμφότεροι εἰρήνην ἑωϋτοῖσι γενέσθαι.—*Herodotus*, lib. i. § 74.

The fact is here very clearly (and probably very justly) related: but, unfortunately, there is nothing, either in the statement itself or in the contiguous passages of the work, that will enable us to determine, with any degree of accuracy, the exact *time* wherein this singular phænomenon took place. And this is the more to be regretted, because the dates of several other events, recorded by the same historian, might be more easily ascertained, if the æra of this eclipse were correctly known; but which are now involved in much obscurity.

Deprived of all information from the body of the work itself, chronologists have called in the aid of astronomy to assist them in fixing the date of this remarkable appearance. For it must be evident, that if we could ascertain, by this mean, that in any solar eclipse, which happened about that period, the centre of the moon's shadow passed over the country bordering on the two contesting empires where the battle was probably fought (for Herodotus has likewise omitted to mention the *place* where the action occurred), we may reasonably and very fairly conclude, that that eclipse only was the one alluded to by the historian.—In this attempt, however, a great diversity of opinion has arisen; the origin of which it may be useful and entertaining here to trace. But, in order to render my subsequent remarks the more intelligible, I shall previously state the various dates that have been assigned to this event by the several authors above alluded to.

Pliny places this eclipse in the fourth year of the forty-eighth olympiad; which answers to the year 585 B. C. (*Hist. Nat.* lib. ii. cap. 12.) A similar opinion has been advanced, among the ancients, by Cicero (*De Divinat.* lib. i. § 49), and probably by Eudemus (*Clement. Alex. Strom.* lib. i. p. 354). And, among the moderns, by Newton (*Chron. of Anc. King. amended*), Riccioli (*Chron. Refor.* vol. i. p. 223), Desvignoles (*Chronol.* liv. iv. chap. 5, § 7, &c.), and Brosses (*Mém. de l'Acad. des Belles Lettres*, tom. xxi. Mém. p. 33.)

Scaliger, in two of his writings (*Animad. ad Euseb.* p. 89, and in Ὀλυμ. ἀναγραφή), has adopted the opinion of Pliny; but in another work (*De Emen. Temp. in Can. Isag.* p. 321), he fixes the date of this eclipse on the 1st of October, 583 B. C.

Calvisius, who was contemporary with Scaliger, thinks that it took place in the year 607 B. C. (*Opus Chron.*)

Petavius

Petavius says that it happened July 9, 597 B. C. (*De Doct. Temp.* lib. x. cap. 1): and he has been followed by Hardouin *Dissert. de* lxx *Hebdom. Dan.* § 3), Marsham (*Chron. Canon.* p. 561), Bouhier (*Recher. et Diss. sur Hérodot.* p. 42), and Corsini (*Fast. Attic.* tom. iii. p. 68); together with M. Larcher, the French translator of Herodotus (tom. i. p. 335).

Usher is of opinion that it happened on the 20th of September, 601 B. C. (*Annal. Vet. et Nov. Testam.*)

Bayer has shown, from the astronomical tables then in use, that this eclipse ought to have taken place May 18th, 603 B. C. (*Com. Acad. Scient. Imp. Petrop* tom. iii.): and he has been supported in this opinion by the two English astronomers, Costard and Stukeley. (*Phil. Trans.* for 1753, pages 17 and 221.)

Lastly, M. Volney has attempted to show, in a recent publication (*Chronologie d'Hérodote*) that the eclipse, mentioned by the historian, could be no other than the one which happened February 3d, 626 B. C.

Thus we find a distance of no less than forty-three years between the extreme periods that have been assigned for this eclipse: an interval which, however, may be somewhat abridged; since there are other facts recorded by the same historian which enable us to reduce these limits, and yet leave the narration consistent with itself.

For, according to Herodotus, the two kings of Media, that immediately preceded the conquest of that country by Cyrus, were Cyaxares, who reigned forty years, and Astyages, who reigned thirty-five years: and it is admitted by all the chronologists, that Cyrus conquered Astyages in the year 560 B. C. Consequently (if the numbers given by Herodotus be correct) the reign of Cyaxares extended from 635 B. C. to 595 B. C. And, since the battle of the eclipse was fought in the sixth year of a war which *began after* Cyaxares had ascended the throne, it could not happen earlier than 629 B. C. nor later than 595 B. C. If therefore we can find, within this short space of thirty-four years, a solar eclipse that was central and *total* in that part of Asia bordering on the two hostile empires, where this battle was probably fought, we may justly conclude that it was the one alluded to by the historian.

I say that this eclipse must have been a *total* one, because no *annular* eclipse (and much less a *partial* one) could have produced that degree of obscurity alluded to by Herodotus. The celebrated Maclaurin, in his account of the *annular* eclipse which happened at Edinburgh, February 18th, 1737, observes (Phil. Trans. vol. xl. p. 177), that "during the "appearance of the annulus, the direct light of the sun was "still *very considerable;* but the places that were shaded from "his light, appeared gloomy:"—that "day-light was *not* "*greatly obscured;* appearing only so much dimmer than "usual, as that of the sun is, when seen through a gentle "mist in a fine morning in April or May." And, as a further proof of the trifling alteration this phænomenon made, he observes, that "there was little notice taken of this "eclipse by the populace in the country: and I cannot but "add, that several gentlemen of very good credit, and not in "the least short-sighted, assure me, that about the *middle* of "the annular appearance they were *not able to discover the* "*moon upon the sun,* when they looked without a smoked "glass, or something equivalent." In another account likewise of this eclipse, in the same volume, by sir John Clerk, bart. it is observed that there "was *no considerable darkness;* "but the ground was covered with a kind of dark-greenish "colour." And M. Le Monnier (who came over from France on purpose to observe the *annular* eclipse of the sun, which happened July 14th, 1748) says, "that when he "looked at the sun with his naked eyes, during the middle "of the eclipse, he could observe *nothing upon the sun,* but "saw the sun *full,* though faint in his light." (Phil. Trans. vol. xlv. p. 588.)

In the account also which is given, in the *Mémoires de l'Acad. Roy. des Sciences* for 1724, of the *total* eclipse which happened on the 22d of May in that year, it is stated that, at the moment when the *last* portion of the sun was covered by the moon, "la clarté a diminué *tout d'un coup;* de sorte "qu'on a eû besoin de lumière pour compter à la pendule: "on voyoit les personnes au grand air, mais on ne distinguoit "pas bien les visages à quelques pas de loin." In another account, in the same volume, it is stated, that the darkness came on "*dans un instant;*" and that, after an interval of two minutes and sixteen seconds, "le soleil *commença* à repa-"roitre *comme un éclair,* qui dissipa *sur le champ* les tenèbres "dans lesquelles on étoit plongé." M. Desvignoles, likewise (in his *Chronologie de l'Histoire Sainte,* vol. ii. p. 253), gives an extract of a letter from M. Abauzit of Geneva, who, at the close of his remarks on the calculation of Petavius respecting this very eclipse, observes; "il ignoroit "que *le moindre* raion, qui commence à poindre, est assez "fort pour dissiper les tenèbres: *comme je l' ai observé deux* *fois.*"

fois." All which may serve to explain the remarkable expression of Herodotus, who says, τὴν ἡμέρην ἐξαπίνης νύκτα γενέσθαι, " the day *suddenly* became night:" a passage which has been ignorantly censured by some of his commentators.

It appears to me, that an inattention to these singular facts has been the principal cause of the various opinions that have arisen respecting the time when this eclipse happened. For each chronologist, having a system of his own to support, has satisfied himself merely with ascertaining that a solar eclipse did take place in the year that he had assigned for it; and which eclipse he supposed might be visible in that part of the world bordering on the two hostile countries: but without taking into his account the *magnitude* of the eclipse at the place where the battle is supposed to have been fought. Now, since the territories of the two belligerent powers were probably separated by the river Halys (which was the case in the subsequent reign, although we have no authentic information that it was so at the period now under consideration), and as the battle was probably fought on the confines of these two empires, I think it will be evident from the preceding extracts, that no solar eclipse could be the one mentioned by Herodotus, unless it was central and *total* in some part of Asia Minor; that is, the centre of the moon's shadow, in such total eclipse, must have passed over that part of Asia Minor where the contending armies were engaging. Consequently the fact is capable of being verified or disproved by the present state of our knowledge in astronomy.

M. Th. S. Bayer is the first who seems to have fixed the attention of the public to this point, in a paper entitled *Chronologica Scythica*, inserted in the Petersburg Memoirs for the year 1728. He consulted his friend Fred. Chris. Mayer on this subject, who has shown, from the astronomical tables *then* in use, that neither the eclipse mentioned by Pliny, Scaliger, Calvisius, Petavius, or Usher, could possibly be the eclipse alluded to by Herodotus. For, the first two (he says) happened between the hours of sun-set and sun-rise in Asia Minor. In the third, the centre of the moon's shadow passed too near the equator, and in the last two it passed too far to the north of Asia Minor, for it to cause any remarkable obscurity there. In order, however, to set the question at rest, he calculated all the solar eclipses that could possibly be seen in Asia Minor from the year 608 B. C. to 556 B. C.; and he found that the one which took place May 18, 603 B. C. was the only one that was

was at all likely to be that mentioned by Herodotus. According to his computation, the centre of the moon's shadow in that eclipse entered the earth's disk about N. lat. 1° 40′, and E. long. 23° from Ferrol. It proceeded then towards the mouths of the Nile; and, traversing the Mediterranean, crossed Cyprus, Cilicia, and Cappadocia, and passed over to Trebizond.

The rev. George Costard, without knowing what M. Mayer had done, has drawn nearly the same conclusions; and has likewise entered into a calculation (from Dr. Halley's tables) of all the eclipses which have been assigned to this event by preceding authors: which may be seen, at length, in the Philosophical Transactions for 1753. In a subsequent paper, in the same volume, Dr. Stukeley has given a map containing the path of the moon's shadow in this eclipse, deduced from the calculations of a Mr. Weaver; and which correspond nearly with Mr. Costard's. But Mr. Costard has suggested an important correction in his computation, by allowing for the moon's *acceleration*; which does not appear to have been attended to either by M. Mayer or Mr. Weaver: and which throws the route of the moon's shadow too far to the southward to pass over any part of Asia Minor. For, on this supposition (he observes) the umbra of the moon will leave Africa near Damietta; and, after traversing the south-east corner of the Mediterranean, will enter Syria between Tripoli and Tyre; and, proceeding across Mesopotamia, between Nisibin and Mosul, will enter the Caspian Sea near Ardebil. Notwithstanding this circumstance, however, the date here assigned has continued to be received as the true date of the battle of the eclipse by all succeeding chronologists; although it must be evident, even from these data, that such eclipse could not be total any where near the place where the battle was probably fought.

But none of these calculations can have much weight at the present day, since they must have been formed from tables which the subsequent improvements in astronomy have shown to be exceedingly defective and incorrect. Even the *mean* motions of the sun and moon are not given with a sufficient degree of accuracy, either in the Rudolphine or Halleian tables, to enable us to determine, with any tolerable correctness, their true *mean* place of conjunction at so remote a period: neither can the lunar *equations*, there given, be safely depended upon. The *secular variations* also are wholly omitted: and these must have an important effect in all inquiries of this kind, since they increase in proportion to the period of time elapsed.

Under

Under these circumstances, and in order to set this question at rest, as far as it can now be done by the aid of astronomical science, I have been induced to re-calculate the elements of the several eclipses, above alluded to, from the new *Tables Astronomiques*, lately published by the *Bureau des Longitudes* in France. In these tables, the mean motions of the sun and moon are given with the greatest exactness for the most distant periods: and, by the successive labours of Mayer, Mason, and Burg, the lunar equations are carried to an astonishing correctness; which, together with the secular variations deduced from the formulæ of M. Laplace, enable us to determine the true place of the sun and moon with considerable accuracy for many centuries prior to the Christian æra. These calculations, at full length, together with a map containing the paths of the moon's shadow in the several eclipses there alluded to, are sent with this paper for the inspection of the Members of the Royal Society, should they be desirous of entering more fully into the detail. The substance of those inquiries I shall now proceed to lay before them.

The eclipse, which is supposed to have been that alluded to by Pliny, happened May 28th, 585 B. C.: and the time of the ecliptic conjunction was at $2^h\ 38'\ 22''$ in the afternoon, *mean* time at Greenwich, or $2^h\ 46'\ 24''$ *apparent* time. The elements were as follow:

True longitude of the luminaries	1^s	$29°$	$41'$	$4''$
Sun's declination, north		20	23	17
—— semi-diameter			15	45
Moon's semi-diameter			16	43
—— equatorial parallax			61	13
—— horary motion from the sun .			35	29
—— true latitude			12	39
—— horary motion in latitude ...			3	30

By a projection of this eclipse, I find that the sun was centrally eclipsed on the meridian, about the middle of the Atlantic ocean, in N. lat. 33¼° and W. long. 43°. The centre of the moon's shadow then proceeded to the parallel of N. lat. 40°, in W. long. 13°; where, turning to the southward, it crossed Spain, and traversed the course of the Mediterranean. By a trigonometrical calculation I have ascertained that the sun set centrally eclipsed on the borders of the Red Sea in N. lat. 28° 1′, and E. long. 35° 2′. So that at no time was this eclipse central in or near any part of Asia Minor. It happened likewise ten years *after* the death of Cyaxares, according to the received chronology.

With respect to the eclipse which happened October 1st, 583 B.C.

583 B. C. it is sufficient to observe that, as the ecliptic conjunction of the sun and moon did not take place till after four o'clock in the afternoon at Greenwich, it is evident that the sun must have set, centrally eclipsed, to the *westward* of any meridian line that can be drawn through any part of Asia Minor: and consequently the eclipse could not have been central in that peninsula.

Calvisius does not come much nearer the truth, in supposing that the eclipse mentioned by Herodotus is the one which occurred in 607 B. C. For in that which happened July 30th, the ecliptic conjunction took place at $8^h\ 26'\ 18''$ in the morning, *mean* time at Greenwich, or $8^h\ 25'\ 55''$* *apparent* time: and the elements were as follow:

True longitude of the luminaries	$3^s\ 29^\circ\ 6'\ 54''$
Sun's declination, north	20 38 39
—— semi-diameter	15 54
Moon's semi-diameter	15 10
——— equatorial parallax	54 33
——— horary motion from the sun .	27 41
——— true latitude, south	2 17
——— horary motion in latitude ...	2 46

By a trigonometrical calculation, I find that the sun rose centrally eclipsed off the coast of Sierra Leona in N. lat. $8^\circ\ 13'$ and W. long. $12^\circ\ 33'$. The moon's umbra then crossed the continent of Africa between the 10th and 20th degrees of north latitude: and the sun became centrally eclipsed on the meridian in Arabia Felix, in N. lat. $18\frac{1}{3}^\circ$ and E. long. $3^\circ\ 24'$. It is evident, therefore, that this eclipse (independent of its being *annular*) was not central in any part of Asia Minor. The other eclipse in this year, which took place February 2d, happened when it was near midnight in Asia Minor.

The eclipse mentioned by Petavius took place July 9th, 597 B. C. The ecliptic conjunction happened at $4^h\ 29'\ 25''$ in the morning, *mean* time at Greenwich, or $4^h\ 29'\ 58''$ *apparent* time: and the elements were as follow:

True longitude of the luminaries	$3^s\ 9^\circ\ 16'\ 32''$
Sun's declination, north	23 28 18
—— semi-diameter	15 49
Moon's semi diameter	14 50
——— equatorial parallax	54 23
——— horary motion from the sun .	27 32
——— true latitude	41 59
——— horary motion in latitude ...	−2 44

* [In the *Phil. Trans.* it is $8^h\ 35'\ 45''$: but we have the authority of the writer of this paper to make the above correction.—Ed.]

By

By a trigonometrical calculation, I find that the sun rose centrally eclipsed to the inhabitants of Holland in N. lat. 51° 45′ and E. long. 5° 39′. The moon's umbra then proceeded across Denmark, Finland, and the northern provinces of Russia: and the sun became centrally eclipsed on the meridian in N. lat. 74½° and E. long. 113° 35′. This eclipse, therefore, could not possibly be the one mentioned by Herodotus. And yet his translator, M. Larcher, without taking the slightest pains to verify the fact, or even to ascertain its probability, has adopted it as the most likely one, "parcequ'elle s'accorde mieux avec la chronologie que "toutes les autres:" an opinion as unfounded, as the circumstance to which it relates; and an assumption which puts the visionary speculations of the antiquarian in competition with the immutable laws of nature. It is scarcely necessary to add, that this eclipse likewise was *annular*.

In the eclipse alluded to by Usher, September 20th, 601 B. C. the ecliptic conjunction took place at 7h 25′ 18″ in the morning, *mean* time at Greenwich, or 7h 31′ 35″ *apparent* time: and the elements were as follow:

True longitude of the luminaries	5s	20°	46′	50″
Sun's declination, north		3	42	27
—— semi-diameter			16	8
Moon's semi-diameter			16	43
—— equatorial parallax			61	14
—— horary motion from the sun .			35	24
—— true latitude			52	1
—— horary motion in latitude ...			−3	27

From a projection of this eclipse, it will be seen that the centre of the moon's shadow entered the earth's disk very near the north pole; and that the sun became centrally eclipsed on the meridian in N. lat. 73¾° and in E. long. 72° 10′. The umbra then passed over Siberia and the eastern parts of the Chinese empire: and consequently this eclipse was not central in any part of Asia Minor.

The eclipse first suggested by Bayer, and hitherto generally received as the true one, happened May 18th, 603 B.C. The ecliptic conjunction took place at 7h 12′ 13″ in the morning, *mean* time at Greenwich, or 7h 19′ 36″ *apparent* time: and the elements were as follow:

True longitude of the luminaries	1s	19°	15′	44″
Sun's declination, north		17	48	24
—— semi-diameter			15	46
Moon's semi-diameter			16	43
—— equatorial parallax			61	16

Moon's

Moon's horary motion from the sun .	35′ 32″
——— true latitude	17 15
——— horary motion in latitude ...	3 30

By a trigonometrical calculation, I find that the sun rose centrally eclipsed in S. lat. 5° 9′ and E. long. 0° 46′. The moon's umbra then passed over the continent of Africa in a north-easterly direction; and, crossing the Red Sea, entered Arabia near Mecca, continuing its course over the provinces of Kerman and Segistan in Persia. The sun afterwards became centrally eclipsed on the meridian in N. lat. 35¼° and E. long. 68°. Consequently this eclipse could not be central in any part of Asia Minor: and yet it has generally been considered, of late years, as the only one that could be reconciled to the fact.

Lastly, I shall notice the eclipse proposed by M. Volney, which happened February 3d, 626 B. C. The ecliptic conjunction took place at 4^h 19′ 27″ in the morning, *mean* time at Greenwich, or 4^h 0′ 35″ *apparent* time: and the elements were as follow:

True longitude of the luminaries	10^s 7° 47′ 47″
Sun's declination, south	18 35 50
—— semi-diameter	16 7
Moon's semi-diameter	15 16
——— equatorial parallax	55 56
——— horary motion from the sun .	29 13
——— true latitude	44 28
——— horary motion in latitude ..,	−2 53

By a trigonometrical calculation, I have ascertained that the sun rose centrally eclipsed to the inhabitants of Great Buccharia in N. lat. 40° 17′, and E. long. 61° 35′: and the moon's umbra then proceeded in a south-easterly direction across Thibet and China. Consequently this eclipse (which, moreover, was an *annular* one) could not possibly be central in any part of Asia lying to the west of the Caspian Sea: and M. Volney ought to have taken some steps towards ascertaining this fact, before he ventured to set up his own opinion in opposition to all preceding chronologists.

I have thus shown, from the most correct evidence which the present state of astronomical science affords, that not one of the eclipses, mentioned by either of the authors above alluded to, could possibly be that which is recorded in so singular a manner by Herodotus. In order, however, that I might not leave the subject in the same degree of doubt in which I found it, I have taken the pains to calculate all the solar eclipses that were likely to have been visible in

Asia

Asia Minor, from the year 650 B. C. to 580 B. C.: but, out of this period of seventy years, I have found only one that was central in, or *near*, any part of that peninsula.

The eclipse here alluded to, happened September 30th, 610 B. C. The ecliptic conjunction took place at 8^h 12′ 51″ in the morning, *mean* time at Greenwich, or 8^h 21′ 41″ *apparent* time: and the elements were as follow:

True longitude of the luminaries	5^s 29° 59′ 40″
Sun's declination, north	0 8
—— semi-diameter	16 10
Moon's semi-diameter	16 36
——— equatorial parallax..........	60 50
——— horary motion from the sun .	34 53
——— true latitude..............	29 57
——— horary motion in latitude ...	3 26

Since the sun's declination in this eclipse was only eight seconds, it may safely be neglected in the calculation; and it may then be found very easily by plane trigonometry that the sun rose centrally eclipsed in N. lat. 47° 34′, and W. long. 11° 55′; that it was centrally eclipsed on the meridian in N. lat. 31° 6′, and E. long. 59° 33′; and set centrally eclipsed in N. lat. 11° 13′, and E. long. 122° 36′. The centre of the moon's shadow crossed the parallel of N. lat. 42° in E. long. 34° 45′; and the parallel of N. lat. 36° in E. long. 50°; and consequently passed nearly in a straight line over the north-eastern part of Asia Minor, through Armenia and Persia, where the sun became centrally eclipsed on the meridian, as above mentioned. This eclipse, therefore, was central and total to part of Asia Minor, Armenia, and Media: and the path of the moon's umbra lay in the very track where the two hostile armies probably met. For it passed over the very mouth of the Halys, just at the point where Crœsus, the immediate successor of Alyattes, crossed that river in order to attack the Median empire.

It would appear from the order of events belonging to the reign of Cyaxares, as related by Herodotus, that the battle of the eclipse happened *prior* to the invasion of the Scythians, who kept possession of his kingdom twenty-eight years; and that, after the expulsion of those barbarians, he besieged and took the city of Nineveh, and thereby put an end to the Assyrian empire. This, however, will not accord with the date here assigned: neither indeed will it suit any of the systems above alluded to; except it be that of M. Volney, which may lay claim to some ingenuity. But his system

system is too much at variance with the astronomical fact to be entitled to any credit.

It has been remarked by Dr. Halley (Phil. Trans. vol. xxix. p. 245), that "though twenty-eight eclipses of the "sun happen in eighteen years, and eight pass through the "parallel of London, yet since March 20th, 1140, no *total* "eclipse has been seen in that metropolis." Indeed, so rare is this phænomenon in any particular country, that its occurrence, when well authenticated, may be considered as an æra which is less liable to mistakes or confusion, than any other event recorded in history. All attempts at imposition or deceit are easily detected by our knowledge of astronomy: and the unintentional errors of the historian are soon rectified and adjusted. On this account, and as the fact of the eclipse is so confidently related by Herodotus (indeed, its singular coincidence with the battle will ever render it memorable in history), I would place the termination of the war between Alyattes and Cyaxares, in the year 610 B.C.: and, if the other events of that period, as related by the historian, cannot be reconciled to this date, I should attribute the confusion to the want of authentic documents and information at the time that the history was written.

I have before observed, that all these calculations have been made from the *Tables Astronomiques*, lately published in France: which tables have since been adapted to the meridian of Greenwich, and to astronomical time, by Mr. Vince, and inserted by him in the third volume of his System of Astronomy*. In these tables are given the *secular variations* in the moon's mean longitude, mean anomaly, and mean distance from her node, as deduced from the formulæ of M. Laplace. It is with much deference that I presume to question the accuracy of the results obtained by means of those formulæ; but, as the present subject is in a great measure connected with that inquiry, I shall briefly state my reasons for offering a doubt upon that point.

* It is to be regretted, that Mr. Vince did not adapt his tables to the English system of *chronology* likewise. For the years before Christ, according to the *English* mode of computation, exceed by unity the coresponding years given by the *French* chronologists: since they make the year of Christ equal to 0, whereas the English reckon it as 1 B. C.—The French also assume the year 1582 as the date of the reformation of the calendar; whereas, in England, that event did not take place till the year 1752.

Without a proper attention to these circumstances, we may be led into an error of one whole year, in the calculation of the places of the heavenly bodies for any period prior to the Christian æra; and into an error of ten or eleven days in our calculations for that space of time which is included between October 5th, 1582, and September 14th, 1752.

It

It is well known that Agathocles, king of Syracuse (when besieged in that city by Hamilcar the Carthaginian general), undertook the bold design of invading Africa, and thereby moving the seat of war from Sicily. He accordingly embarked a numerous army, and set sail for the continent. The *day after* he left Syracuse, the fleet was terrified at an eclipse of the sun; which was so great, that, in the words of Diodorus Siculus, lib. xx. ὁλοσχερῶς φανῆναι νύκτα, θεωρουμένων τῶν ἀστέρων πανταχȣ̃, "it seemed *exactly* like night, "the stars *every where* appearing." This eclipse was therefore evidently *total* in the place where it was seen by the fleet of Agathocles. It happened on August 15th, 310 B. C. The ecliptic conjunction took place at 8^h 10′ 23″ in the morning, *mean* time at Greenwich, or 8^h 9′ 6″ *apparent* time: and the elements were as follow:

True longitude of the luminaries	4^s	16°	41′	32″
Sun's declination, north		16	2	38
—— semi-diameter			15	57
Moon's semi-diameter			16	39
—— equatorial parallax			61	0
—— horary motion from the sun .			35	9
—— true latitude			14	42
—— horary motion in latitude ...			3	28

From these elements I have found, by a trigonometrical calculation, that the sun rose centrally eclipsed to the inhabitants of the western coast of Africa, in N. lat. 24° 57′ and W. long. 14° 9′. The centre of the moon's shadow then, crossing the Desert, proceeded towards the Mediterranean, near to, but rather to the southward of, Tripoli; and crossed the parallel of N. lat. 33° in E. long. 20° 44′. But in no part of its course did it advance more northerly than N. lat. 33° 55′ 36″, which I find by a trigonometrical calculation to be its maximum of latitude, and the parallel of which it reached in E. long. 35° 21′ 8″. It then turned to the south; and the sun became centrally eclipsed on the meridian in N. lat. 30¼° and E. long. 59° 45′.

Let us now compare this result, with the fact as related by Diodorus. It is stated by this author, that Agathocles was six days on his passage, from Syracuse to the coast of Africa; although he used the utmost expedition, being, in fact, closely pursued by the Carthaginian fleet. The place where he landed was called Λατομίας, the *Quarries*; whence he proceeded to the neighbouring cities of Μεγάλην πόλιν, *Megalopolis*, or the *Great City*, and Λευκὸν Τύνητα, *White Tunis*. The position of these cities is not handed down to us; all that we know is, that the latter place (which must not be confounded with the present Tunis) was two thousand stadia, or two hundred and twenty-nine English miles, distant from Carthage. Agathocles, therefore, probably landed near the Syrtis Minor, or Gulf of Cabes, about three hundred miles in a direct course from Syracuse: whence we may reasonably conclude that he performed one-sixth of his passage, or about fifty miles, in the space of one day; which, I am aware, is not so much as the mean rate that has been attributed to the ships of the ancients (see Herodotus, lib. 4, § 86). Syracuse lies in N. lat. 37° 3′ and E. long. 15° 14′; and, consequently, on the day after the sailing of Agathocles from that port (being the day on which the eclipse took place), the fleet would be in about N. lat. $36\frac{1}{4}$°: at all events, it could not (from the direction of its course) be much further south than this point; which is all that is required in the present instance; and a few miles, either way, not being of any material consequence. It follows therefore, that in the meridian of Syracuse, the northern part of the moon's umbra ought to extend as far north as that parallel of latitude. But, from the calculations above adduced, it will be found that the centre of the moon's shadow, on that meridian, had only reached the parallel of about N. lat. $32\frac{1}{2}$°: and as the semi-diameter of the umbra was not more than forty-seven and a quarter English statute miles, or about two-thirds of a degree, the eclipse could not *there* be total to the northward of N. lat. $33\frac{1}{4}$°. Now, since the place where Agathocles landed in Africa was probably not situated below the parallel of N. lat. 34°, it is evident that he did not, in *any part* of his course (and much less at the *commencement* of it), come within a considerable distance of the moon's umbra.

I much doubt whether, according to our present computation, this eclipse was total even at Tripoli: and, although it was unquestionably of considerable magnitude, both there and as far north as Syracuse itself, yet (for the reasons already given in this paper) I do not think that, at any intermediate place between these two cities, it could be so great as to produce that degree of obscurity, which is recorded by Diodorus and confirmed by Justin. In order that the phænomenon should accord with the fact, as related by these historians, the centre of the moon's shadow ought to pass over, or very near to, Malta: that is, the latitude of the moon ought to be, at least, three degrees greater than our present tables make it.

Since the latitude of the moon depends on her true distance from the node, these observations (if correct) will show

show the necessity of some alteration in the table of the secular variation of the moon's mean distance from her node, which (agreeably to the rule given by M. Laplace) is deduced immediately from the secular variation of her mean longitude. These remarks, however, are thrown out merely as hints to those who are more conversant with, and better informed on, the subject: and I regret that I have not more time to pursue the inquiry further.

Such an alteration, as is here suggested, would somewhat vary the position of the route of the moon's umbra, in all the eclipses which have been the subject of this paper; but, in none of them would it alter the conclusions which have been drawn from them, except perhaps in the one (September 30th, 610 B. C.) which I have supposed to be that mentioned by Herodotus. In this particular case, the path of the moon's umbra might, by such a correction, be thrown so much further north as to prevent the eclipse being total in any part of Asia Minor. But still it would remain the only one that can be at all adapted to the account given by Herodotus; since there is no other that could possibly be central in, or *near*, any part of Asia Minor from the year 650 B. C. to 580 B. C.: a period which far exceeds the probable limits of time wherein this singular phænomenon must have taken place, so as to be reconcileable to any received system of chronology.

November 1810. F. B.

LXVII. *An Account of the Smelting of Lead. By Mr.* John Sadler.

[Concluded from p. 282.]

The substances which are found to render the ores of lead refractory, when mixed with them, are cawk, (*sulphate of barytes,*) black jack, (*blend* or *sulphuret of zinc,*) sulphur, (*iron pyrites,*) and silver, or copper, when they are contained in the ore in larger proportions than usual.

I have always considered that these substances render an ore refractory by the extra quantity of sulphur they bring with them. I do not think the earth or metals alone would produce any visible effect in the smelting; and I am almost confirmed in this opinion, by repeatedly observing the effect produced by roasting the ore previous to smelting; it works more pleasantly, requires less lime and fuel, and gives a better produce.

The quality of the coals materially affects the working of the

the hearth and the produce of lead; those which are free from sulphur, and which leave but little residuum after combustion, are the best fitted for smelting.

The lead, which is separated directly from the ore, is called ore lead, or common lead, to distinguish it from that which is the result of a subsequent process.

Slag-hearth Smelting.

The slags or scoria separated in the process of ore-hearth smelting, consist of the infusible part of the ore, the ashes of the coals, peats, &c. semi-vitrified and agglutinated by a quantity of oxide of lead produced by the action of the blast; they contain also particles of metallic lead dispersed through their substance, and not unfrequently unreduced ore.

These scoria, which are technically named *gray slags*, vary considerably in the quantity of lead they contain, but the poorest hold a sufficient quantity to pay the expense of smelting.

As it is necessary to bring these slags to a perfect fusion to separate the lead, a furnace capable of producing a more intense heat than the ore-hearth is requisite. Plate X contains plans, sections, &c. of the slag hearth, in which the same letters are applied to the same parts in the different figures. Fig. 1 is a perspective view of the hearth; Fig. 2 a plan; and Fig. 3 a perpendicular section; (*a*) a cast-iron plate, which forms the bottom of the hearth: an old workstone is generally used for this purpose; what it wants in dimensions is supplied by other old castings, refuse of the ore-hearth: the bottom is laid in fine dust which has been damped a little, and well rammed; on the bottom is placed the back (*b*) which is formed of three or four old bearers laid on each other; on the centre of the back is placed the tuyre, or as it is generally called the *tue-iron*. The pipe-stone (*c*) is bedded in tempered clay on the back; it is a block of free-stone about 15 or 18 inches square, and 30 long, hollowed out on the underside to fit the tuyre. Two old bearers (*d d*) about 18 inches apart, and placed at right angles with the back, against which their ends butt, form the lower part of the sides: on these, two blocks of free-stone (*e e*), about 15 by 18, and in length equal to the height of the pipestone, are placed on end; the front is built entirely with old castings, the lower one resting on the ends of the side bearers.

The spaces between the back of the chimney, the side and the cheek stone, are filled with old castings, bricks, or pieces of stone, and the joints filled up with dust or ashes; the

the space left between the bottom of the hearth and the lower front bearer is called the breast;—(*f*) is the sump or pot to contain the lead, as it flows from the hearth; the space between the breast and the sump is paved with old castings imbedded in dust, and the joints filled with thin mortar grout; near the sump is a mould for casting the lead into pigs.

The hearth is prepared for working, by slightly ramming into the bottom a quantity of coal ashes; the sump is also filled, and the space between it and the breast; the dotted part (*g g g*) fig. 3, represents the coal ashes: the fire is next lighted, and when the interior of the hearth has acquired a good red heat, the smelter throws on a few shovels full of gray slags (which have been previously broken to the size of an egg), and as the hearth settles, occasionally adds fuel or more slags as may be required: in a few minutes after charging with the slags a small perforation is made in the breast by passing a pointed iron rod through the ashes close to the bearer; the liquid scoria and lead flow through this opening down the inclined plane formed by the ashes; as they become hot the lead filters through them, and finds its way into the sump; the scoria from its viscidity remaining on the surface, from whence it is removed occasionally as it cools and becomes hard.

The slag hearth is continued working for 12 or 14 hours, the smelter adding materials occasionally as required, and judging of the proportion of fuel by the heat and appearance of the fluid scoria.

At the conclusion of the day's work the hearth is suffered to burn down as low as possible; and when the scoria ceases to flow the bellows are stopped, the scoria on the bed of ashes removed, and the lead which has collected in the sump is cast into pigs. Cool ashes are next spread over the hot bed, and the hearth drawn and cleared from what remains in it, and when moderately cooled, prepared with a bottom of ashes for a succeeding day's work.

The principal art in working a slag hearth is to keep a proper *noze*, and to have the hearth light and open in front, otherwise the blast does not work well and diffuse itself equally through all parts, but forces itself up behind and very soon destroys the pipe-stone.

The noze is a protuberance which surrounds the orifice, through which the blast passes; it is formed by the vitrified slags trickling down the pipe-stone, and cooled by the blast as it enters the hearth. With very fusible slags it is sometimes difficult to get a proper noze to form, and with refractory slags to keep it of a moderate size. With too large a protuberance, the hearth works most at front; with too small, chiefly at the back.

In general a noze may be prevented growing too large, by laying the fuel principally near the pipe-stone, and occasionally forcing in a pricker through the tuyre.

A noze may be enlarged by a contrary situation of the fuel, and throwing in close to the pipe-stone a few shovels of dust and ashes from the top of the hearth.

The fuel used at the slag hearth is coke.

The scoria, the refuse of the operation in the slag hearth, is called black slag; it contains a portion of metallic lead which is separated by stamping and washing.

The lead obtained by the slag hearth is hard and sonorous; it is of an inferior quality, and unfit for many of the purposes to which common lead is applied.

Cupola Smelting.

The method of smelting lead by the cupola furnace as principally practised in Derbyshire, I have taken from Watson's Chemical Essays; a more correct or interesting account cannot be given.

"The furnace called a cupol, or cupola, in which ores are smelted by the flame of pit coal, is said to have been invented about the year 1698, by a physician named Wright; though Beecher may, perhaps, be thought to have a prior claim to its invention, or introduction into Germany. But whoever was the inventor of the cupola, it is now in general use, not only in Derbyshire and other countries, for the smelting of the ores of lead, but both at home and abroad, where it is called the English furnace for smelting copper ores. This furnace is so contrived, that the ore is melted not by coming into immediate contact with the fuel, but by the reverberation of the flame upon it. The bottom of the furnace upon which the lead ore is placed is somewhat concave, shelving from the sides towards the middle; its roof is low and arched, resembling the roof of a baker's oven; the fire is placed at one end of the furnace, upon an iron grate, to the bottom of which the air has free access; at the other end, opposite to the fire-place, is a high perpendicular chimney; the direction of the flame, when all the apparatus in the sides of the furnace are closed up, is necessarily determined by the stream of air, which enters at the grate, towards the chimney, and in tending thither it strikes upon the roof of the furnace, and being reverberated from thence upon the ore, it soon melts it.

"It

"It is not always an easy matter to meet with a current of water sufficient to move the bellows required in smelting on an hearth furnace; and to carry the ore from the mine where it is dug to a considerable distance to be smelted is attended with great expense: this expense is saved by smelting in the cupola furnace, which, not requiring the use of bellows, may be constructed any where; wood is very scarce in every mining country in England, and the pit coals cost ten or twelve shillings a ton in Derbyshire, yet they can smelt a definite quantity of ore in the cupola at a far less expense, by means of pit coal than of wood. The flame which plays upon the surface of the ore, and smelts it in a cupola furnace, is not driven against it with much violence; by this means small particles of ore called *belland* may be smelted in a cupola furnace with great convenience, which would be driven away if exposed to the fierce blast of a pair of bellows in a hearth furnace. These are some of the advantages attending the use of a cupola in preference to a hearth furnace, and to these may be added, one superior to all the rest, the preservation of the workmen's lives: the noxious particles of the lead are carried up the chimney in a cupola, whilst they are driven in the face of the hearth smelter at every blast of the bellows.

"They generally put into the cupola furnace a ton of ore, previously broken small, and properly dressed, at one time; this they call a *charge:* if the ore is very poor in lead, they put in somewhat more; and they work off three charges of ore in every twenty-four hours. In about six hours from the time of charging, the ore becomes as fluid as milk.

"Before the ore becomes fluid, and even whilst it continues in a state of fusion, a considerable portion of its weight is carried off through the chimney; what remains in the furnace consists of two different substances, of the lead, for the obtaining of which the process was commenced, and of the *slag* or *scoria.* The proportion between these parts is not always the same, even in the same kind of ore: it depending much upon the management of the fire. The lead, being heavier than the slag, sinks through it as it is formed, and settles into the concavity of the bottom of the furnace. The pure slag, according to the idea here given, is that part of the ore of lead which is neither driven off by the heat of the furnace, nor changed into lead. *In order to obtain the lead free from the slag which swims over it, the smelters usually throw in about a bushel of lime;* not, as is usually supposed, in order to contribute towards the more perfect fusion of the ore, but to dry up the slag which floats on the surface of the lead, and which, being as liquid as lead, might otherwise flow out along with it. The slag being thus thickened by an admixture of lime, is raked up towards the sides of the furnace, and the lead is left at the bottom. There is a hole in one of the sides of the furnace, which is properly stopped during the smelting of the ore; when the slag is raked off, this hole is opened, and being situated lower than the lead in the furnace, the lead gushes through it into an iron pot placed contiguous to the side of the furnace; from this pot it is laded into moulds, each containing what they call a pig of lead: the pigs, when cold, being ordinarily stamped with the maker's name, are sold under the name of *ore* lead. After the lead has all flowed out of the furnace, they stop up the tap-hole, and drawing down the slag and lime into the middle of the furnace, they raise the fire till the mixture of slag and lime, which they term simple slag, is rendered very liquid; upon this liquid mass they throw another quantity of lime, to dry it up, as in the former part of the process. This second mixture of slag and lime is then raked out of the furnace, and the small portion of lead separated from the fusion of the first generally to the amount of twenty or thirty pounds, being let out of the furnace, a new charge of ore is put in, and the operation recommenced. In order to spare the lime and the expense of fuel attending the fluxing of the mixture of lime and slag, they have in some furnaces lately contrived a hole, through which they suffer the main part of the liquid slag to flow out, before they tap the furnace for the lead; upon the little remaining slag they throw a small portion of lime, and draw the mixture out of the furnace without smelting it. This kind of furnace they have nick-named a *maccaroni*."

LXVIII. *On the Error discovered in the Nautical Almanac. By Mr.* FIRMINGER, *late Assistant at the Royal Observatory, Greenwich* *.

To Mr. Tilloch.

SIR, IN your journal for the last month, your readers will doubtless notice, with much surprise, an account of an error in the Nautical Almanac for the year 1812, which is therein said to have been recently discovered by Dr. Kelly. To remove, however, from the minds of such readers and others, whom this circumstance may happen to interest, the mis-

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take into which that statement may tend to lead them, as well as the illiberal insinuation which it tends to throw upon the French astronomers and mathematicians, by challenging them with an unjust adoption of the results derived from the Nautical Almanac, into their *Connoissance des Tems*, under the mask of pretended originality, has given occasion to the production of this article; a circumstance which, from a long residence at the Royal Observatory, and of course a more familiar acquaintance with the habits of the late Dr. Maskelyne, I may be conceived to be better able to answer than others less acquainted with that justly celebrated man.

Dr. Maskelyne says, in his preface to the Nautical Almanac of the year above alluded to, that he has taken the mean obliquity of the ecliptic for the beginning of the year at 23° 27′ 43″,3, which he reduces to the apparent obliquity by applying the equation arising from the precession of the equinoxes, combined with a diminution of half a second a year from a change of this quantity in the plane of the obliquity itself, and an equation depending on the place of the moon's node arising from her action on the spheroidal figure of the earth:—these two equations are, as Dr. Maskelyne states, contained in two tables which were published at the same time with the first volume of the Greenwich Observations. The first equation amounts to —0″,5, and the second to —9″,0; their sum is —9″,5 which applied to 23° 27′ 43″,3, the mean obliquity above mentioned, gives the apparent obliquity of the ecliptic 23° 27′ 33″,8 at the beginning of 1802.

By comparing this deduction with that given opposite the first page of the Nautical Almanac, it will be found exactly the same.

The obliquity of the ecliptic at the beginning of the preceding year, viz. 1811, Dr. Maskelyne states in the preface of the Nautical Almanac to be 23° 27′ 51″9, and at the beginning of the year 1813 he takes it to be 23° 27′ 51″,3; so that the mean obliquity of the ecliptic at the beginning of 1812, it should appear from these two statements, ought to be 23° 27′ 51″,6, instead of 23° 27′ 43″3 as mentioned in the Almanac for that year. This apparent discrepancy making a difference of 8″,3, may seem to give sanction to the account contained in the Philosophical Magazine of last month, that the difference in question might have probably originated in mistake. Dr. Maskelyne was however by far too careful a man to suffer such a circumstance to have escaped his notice; and the frequent use of the obliquity of the ecliptic in the calculations that were constantly carrying on on at the Royal Observatory, rendering the accuracy of that datum a circumstance of the greatest importance, was a means which constantly drew the doctor's attention to that subject. He had been accustomed to settle the mean obliquity, as well as its secular variation, from observations made on the sun's zenith distances taken for ten days before and after the summer solstice, and reduced to the time of that solstice, instead of taking a mean of the reductions derived from the two solstices. This he did in consequence of the discrepancy in the results derived from each solstice; and as he was unable to say what produced this difference, and knowing (at that time) of no cause but the uncertainty of refraction, he was induced to give a preference to the deductions derived from the sun's zenith distance at the summer solstice. Here, however, another difficulty presented itself to him:—By a comparison of the deductions thus obtained with the same derived from observations made during the life of Dr. Bradley, the secular variation of the obliquity of the ecliptic came out much less than the secular variation derived from the late observations, or those in the time of Dr. Bradley with more ancient ones, in which a much longer period had been embraced; so that the variation of the obliquity of the ecliptic must from such determination appear to be decreasing less than formerly, for which no cause could be assigned. This led Dr. Maskelyne to consider whether he was right in giving more weight to the deductions made from the observations at the summer solstice than those at the winter solstice; and finding the deductions from the latter agreeing better when compared with those made in the time of Dr. Bradley, in giving the secular variation nearer to what arises from either of those observations when compared with ancient observations, induced him at last to adopt them in preference to the deductions derived from observations about the summer solstice.

It will be recollected that I have already said that Dr. Maskelyne knew of no cause to give preference to either; and it was a circumstance of anxiety to him to see so great a difference between the results at the two solstices as the Greenwich quadrant gave, without being able to assign a reason. Knowing the care taken in making the Greenwich observations, and their superior accuracy over those made on the continent, prevented him from relinquishing his deductions for an adoption of others which he could not bring his mind to believe were so much to be depended on. The suspense which this state of things induced, was not however to be of long continuance. A series of observations made by Mr. Pond, our present astronomer royal, on on the declinations of the principal fixed stars, taken with an excellent meridian circular instrument made by Mr. Troughton, and his comparison of those observations with a like series of observations made with the mural quadrant at Greenwich, pointed out to Dr. Maskelyne a circumstance of which till then he had never even admitted a conjecture, viz. that in the course of the number of years in which the mural quadrant at Greenwich had been suspended, it must have changed its figure. Dr. Maskelyne viewed Mr. Pond's observations at first with a very cautious eye; but having satisfied himself that they were taken with the greatest care and ability, he availed himself of the deductions drawn from them, and occasionally made use of their results, as corrections applied to observations made with the Greenwich quadrant. He brought back the obliquity of the ecliptic to nearly the same quantity he had before adopted; but whether he ever intended to give a correction to the Nautical Almanac of 1812, or not, I am unable to say. It is however likely he did not think the difference of sufficient importance to require any thing more than a note, merely stating what the obliquity of the ecliptic should be: indeed it is uncertain even to the last, whether Dr. Maskelyne was altogether satisfied on this point; for Mr. Groombridge told me a long time ago, that he mentioned this circumstance to Dr. Maskelyne, who in reply said he should see by and by;—that he was not sure it was wrong, but if it turned out so, he should correct it. It is worthy here of remark also, that Dr. Maskelyne never readily received notice of any inaccuracies in his deductions. Inaccuracies had been often pointed out to him by various persons, which, upon examination, were generally found to originate in a want of knowledge on their part of the true principles upon which the calculations were founded. A seeming indifference therefore in the conduct of Dr. Maskelyne was generally remarked, and complained of by persons who gave him such information, in his attention to their statements.

Having already observed that the mean obliquity of the ecliptic at the beginning of the year 1812 was given by Dr. Maskelyne 23° 27′ 43″,3, and that the apparent obliquity was 23° 27′ 33″,8, differing by 9″,5 from the mean obliquity; it appears from the statement in the Philosophical Magazine, that this difference has led to a conclusion, that the mean obliquity had been twice equated; and it is curious enough that the quantity of mean obliquity as assigned by Dr. Maskelyne should have been nearly this quantity different

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from the mean obliquity given at the beginning of the preceding year: but that it could not arise from that conjecture might have been easily concluded, without other evidence, from knowing that the mean obliquity of the ecliptic requires a correction to reduce it to the apparent for every day in the year. Had this mistake, therefore, originated in one instance, it could not have taken place in all the calculations throughout that year, and by each computer, as each computer would have to make all the reductions for himself; but, perhaps, the writer of the article above alluded to may not be aware of the necessity of this reduction, and has adopted the conclusion, that the apparent obliquity of the ecliptic is the same during the whole year.

Dr. Maskelyne having altered his quantity of the mean obliquity of the ecliptic at the beginning of the year 1813, was obliged to return back to his deductions, derived from the observed zenith distances of the sun at the summer solstice, and again to his deduced secular variation: hence he was compelled to notice this variation, which he does in his preface to the Nautical Almanac for the year 1813, where he says, "By the summer solsticial zenith distances of the sun of late years (only adding one second to the zenith distances for the error of the mural quadrant, altering its figure according to Mr. Pond's comparison of the declinations of the stars observed with circular instruments, by himself and other astronomers, by those given by myself at the end of the Greenwich Observations of 1802, and published in the Philosophical Transactions of 1806), and taking the mean annual diminution of the obliquity of the ecliptic at present to be at the rate of 42″,6 in 100 years, I have assumed the mean obliquity at the beginning of this year to be 23° 27′ 51″,3: these numbers were used in the computation of this ephemeris."

Dr. Maskelyne here plainly tells us, that he takes the obliquity of the ecliptic from the Greenwich observations; and although he gives the secular variation only 42″,6, he assigns no reason for his having taken it so much less than generally stated,—the French making it in their new Solar Tables, 52″,1.

It is but right, however, that I give in this account such documents as shall confirm the accuracy of the statements above adduced; for which purpose I shall state at full length the deduction of the observations of the sun's zenith distances taken at the Royal Observatory, and the obliquity of the ecliptic thence arising for the year 1802, which observations

servations being published, any one may repeat or examine the calculations.

	☉ observed Z. D. corrected for refraction.	Reduction to Solstice.	Solsticial Z. D. affected with Parallax.
1802.			
June 11	28° 24′ 23″,7	23′ 44″,6	28° 0′ 38″,7
12	.. 20 12,0	19 32,3	 39,7
13	.. 16 25,1	15 43,9	 41,2
15	.. 9 59,4	9 21,5	 37,9
16	.. 7 29,3	6 47,2	 42,1
17	.. 5 17,6	4 37,7	 39,9
18	.. 3 30,9	2 52,9	 38,0
20	.. 1 15,1	0 37,3	 37,8
21	.. 0 43,4	0 6,7	 36,7
22	.. 0 40,0	0 0,9	 39,1
23	.. 1 1,8	0 19,9	 41,9
25	.. 2 50,8	2 12,4	 38,4
29	.. 11 33,0	10 54,3	 39,7
July 3	.. 26 47,1	26 6,2	 40,9
		Mean	28 0 39,5
		Parallax	−4,0
		Nutation	+9,24
		Cor. collimat.	0,85
		Mean solsticial Z. D.	28 0 45,59
		Latitude......	51 28 40
		Mean obl. ecliptic. .	23 27 54,41
Dec. 12	74° 33′ 9″,4	23′ 27″,8	74° 56′ 37″,2
15	.. 45 11,4	11 31,4	 42,8
16	.. 48 13,3	8 28,2	 41,5
18	.. 52 51,7	3 45,9	 37,6
20	.. 55 45,3	0 56,4	 41,7
21	.. 56 23,9	0 14,1	 38,0
24	.. 55 38,6	0 57,1	 35,7
28	.. 48 10,3	8 31,2	 41,5
		Mean	74 56 39,5
		Parallax....	.. −8,5
		Nutation . ..	.. −8,7
		Cor. collim.	.. +1,0
			74 56 23,3
			51 28 40
			23 27 43,3

By

By an attention to these deductions, it will be seen that the obliquity of the ecliptic at the summer solstice, for the year 1802, comes out 11″,1 more than at the winter solstice; and that the obliquity at the winter solstice differs but little from that given by Dr. Maskelyne in the preface of the Nautical Almanac for the year 1812, and used in the calculation of the Almanac for that year. In a former communication to this journal, I took notice of the uncertainty, not only in the mean obliquity of the ecliptic, but in the quantity of secular variation, with a hope that astronomers would shortly be gratified in a knowledge of its actual state, both as to decrease and quantity, from a series of observations to be made with an instrument invented and executed by Mr. Troughton, whose skill as an artist is too well known and appreciated to require any commendation. Astronomers are much indebted to Mr. Pond for his excellent observations, and his discovery of the existing error in the mural quadrant at Greenwich, which, at the same time that they added to the accuracy of astronomical deductions, gave birth to the necessity of calling forth the inventive genius and superior excellence in the executing hand of Troughton.

There can now be but little doubt that the mean obliquity of the ecliptic, as given by Dr. Maskelyne in the Nautical Almanac for 1812, and as deduced by him from the winter solstice, is considerably wide of the truth; and that it is probable the obliquity is not much different from what he has assumed in the subsequent Almanacs. It may, therefore, appear necessary in very nice observations to allow a correction for this difference, though this perhaps will seldom occur*: for the only part of the ephemeris affected by it of consequence, will be the sun's declination and right ascension: the latter, however, is too small to be of importance, even in the nicest observations, as in astronomical observations we only want the daily difference of the sun's right ascension, which will not be sensibly affected by so small a quantity as that under consideration. I shall, however, give the investigation of a formula for making this correction, or any correction of a similar nature.

For Declination.

Let L = the sun's longitude; O = the apparent obliquity

* If the whole quantity of 8′,3 be allowed for as error, it makes an uncertainty of about one seventh of a mile in the latitude deduced from the sun's declination, and this only at the time of the solstices, viz. June 21st and Dec. 21st, an uncertainty even in the extreme cases by far too small to be noticed in nautical practice, it being a doubt whether the best sextants are not liable to as great or greater uncertainty.

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of the ecliptic assumed by Dr. Maskelyne; $p =$ the quantity to be corrected for; $D =$ the declination of the sun, computed from O and longitude L; $d =$ the new declination arising from $\overline{O + p}$.

Then $R : s,L :: s,O : s,D$

$$R : s,L :: s,\overline{O+p} : s,d$$

$$s,d = s,D \times \frac{s,\overline{O+p}}{s,O} = s,D \times \frac{s,O \,.\, \cos.\, p + \cos.\, O \,.\, s,p}{rad \times s,O}$$

$= s,D \times (1 + s,p \,.\, \text{cot. } O)$; therefore, $s,D \times s,p$. cot. $O =$ the quantity to be applied to the declination contained in the Almanac, to correct it for the quantity p; but sine $p \times$ cot. O is constant. Hence we have only to find the s,D, and five places of decimals in the logarithms will be sufficiently correct, and the calculation made only for every fifth day, the rest may be filled up by proportion.

For Right Ascension.

Put $A =$ right ascension in the Almanac; $a =$ the right ascension to be found: the rest as in the last case.

Then, cot. L : R :: cos. O : tang. A

$$\text{cot. } L : R :: \cos.\, \overline{O+p} : \text{tang. } a.$$

$$t,a = \frac{c,\overline{O+p}}{c,O} \times t,A = \frac{c,O \,.\, cp - s,O \,.\, sp}{rad \times cs,O} \times t,A.$$

$$= t,A \times (1 + t,O.\ \sin.\ p.)$$

Hence the correction for the right ascension will be tang. A $\times$ tang. O + sine p, where the tangent O $\times$ s,p is constant, and as in the case of the declination four or five places of decimals in the logarithms will be sufficient. Here, however, it will be necessary to attend to the signs of the quantities, as the quantity found to be applied to the right ascension given in the Almanac, will sometimes require to be added to, and at others to be subtracted from, those right ascensions, either of which is immediately determined by the sign of the quantity, that is, according as the tangent of A is positive or negative.

If the mean obliquity of the ecliptic for the beginning of the year be assumed at 23° 27′ 51″,6, and the correction be taken = 8″3: then, log. sine 8″,3 = 5·6046527 log. cot. 23° 27′ 51″,6 = 10·3624378, and log. s,p $\times$ cot. O = 5·9670905: if to the constant log. 5·96709 we add the log. sine of the declination taken to the nearest minute, the sum will be the log. sine of correction in declination. And in the same manner may the correction in right ascension be found, taking the log. tangent of O instead of the log.

cotangent,

cotangent, and log. tangent of right ascension instead of log. sine of declination.

As the right ascensions and declinations of the sun are calculated from his true longitude, and apparent obliquity of the ecliptic, we may derive these from the two equations above, viz.

R : *s*,L : : *s*,O : *s*, declination,

Cot. L : R : : cos. O : tang. right ascension.

Where L = the sun's true longitude, and O the apparent obliquity of the ecliptic; but the method above shown is much shorter in practice.

With respect to that part of the statement contained in the Philosophical Magazine, which accuses the French of copying from the calculations of the Nautical Almanac in making up the ephemeris for the *Connoissance des Tems*,—it must be confessed that if they have declared that those parts were actually calculated by themselves, and not taken from the Nautical Almanac, they are deserving of censure; but I must own, I never saw or heard any one say that they ever did see such a declaration: on the contrary, it is well known, or at least was so to me and several other persons, many years ago, that the French had of late years adopted the calculations of the Nautical Almanac into their *Connoissance des Tems*, by only allowing for the difference of meridians. But no one would surely accuse them of impropriety for so doing: a mutual correspondence had always subsisted between Dr. Maskelyne and the French astronomers, and they may be said to have mutually contributed their aid to the improvement of this great national work; and for any one to convince themselves of this fact, they need only read the preface to the Nautical Almanacs, where Dr. Maskelyne says he has, at different times, received direct communications not only from the French astronomers, but from their Board of Longitude. In the preface to the Nautical Almanac of 1812, he expresses himself thus: "The French Board of Longitude having been pleased to send me a manuscript copy of new improved Lunar Tables by Mr. Burg, of Vienna," &c.: and again, "The French Board of Longitude having last year published M. De Lambre's new tables of the sun, and Mr. Burg's new tables of the moon, and favoured me with copies for the use of the computers of the Nautical Almanac, and myself, I have," &c.

From which it clearly appears, that the French Board of Longitude did not send these copies merely in friendship to Dr. Maskelyne, but for the use of the Nautical Almanac.

They

They are well acquainted with the excellency of the plan upon which it is conducted, and the almost impossibility of its being inaccurate; they therefore, very wisely, evade the useless trouble of a recalculation of that which there can be no necessity but once to calculate; and after the care and liberality they have shown for its success, can it be fair to accuse them of making use of a work to which they have afforded their best helps? Let me ask what the Nautical Almanac would have been, had we not availed ourselves of the deductions of the profound researches of the mathematicians of the continent? Had they withheld from us their discoveries,—discoveries which Dr. Maskelyne acknowledges in all his prefaces to the Nautical Almanacs, and had published their *Connoissance des Tems* from their own manuscript tables founded on the elaborate theories of Laplace, let me ask, In this state of things what would have been the comparison in point of excellence between the French *Connoissance des Tems* and the Nautical Almanac? It appears to me that the English astronomers in this case would have gladly availed themselves of their superior accuracy even in preference to the Nautical Almanac, and that the French may with propriety say, that their *Connoissance des Tems, as now copied,* is from *original matter.* It would not perhaps be unjust, even to say that we are as much indebted to the French mathematicians for their liberal communications to the improvement of the Nautical Almanac, as to Dr. Maskelyne for the excellence of the plan upon which it is conducted. The French have made no display of this communication to Dr. Maskelyne; it is to the doctor himself that we are indebted for the information, who was always ready to do justice to those persons from whom he had received such favours. The statement in the Philosophical Magazine of last month seems to consider Dr. Maskelyne as entitled to great credit for the accuracy of the calculations contained in the Nautical Almanac. But I apprehend Dr. Maskelyne's fame, as connected with this useful work, soars much higher,—it is the plan on which the Nautical Almanac is conducted that insures its accuracy,—and that Dr. Maskelyne's fame rests more on his having furnished the most correct data as elements, and given general directions in the employment of them, than on the subordinate examination of its contents after the calculations were completed. This examination is always conducted by a person called the comparer, and the correctness of the calculations of the Ephemeris depends in a great measure on the care with which he discharges his duty. The French *Connoissance des Tems*, whether modelled or not on the plan of the Nautical Almanac, will be allowed to contain a number of essays on scientific subjects, not only original, but of the highest importance to the mathematician and astronomer, who has hitherto always regarded that work as a most valuable acquisition to his library*.

LXIX. *On the Error discovered in the Nautical Almanac.* *By* S. Groombridge, *Esq.*

To Mr. Tilloch.

Sir, In your Magazine for the last month, you have noticed the error in the Nautical Almanac for the ensuing year, of the obliquity of the ecliptic, both mean and apparent; the quantity of which is $8\frac{1}{2}$ seconds. You further mention, that this error was first discovered by Dr. Kelly, of Finsbury Square. Now, I must beg leave to assure you, that is not the fact; having in the early part of the summer 1810 shown the same to Dr. Maskalyne; who replied, that he would examine it, and notice it accordingly. The error is too trivial, to render it necessary to alter the right ascension and declination of the sun, for each day; which are the only two parts affected; the whole of the columns which are used for nautical purposes being calculated independently of the obliquity of the ecliptic. When Mr. Pond, the present astronomer royal, came into office, I also pointed it out, on my first visit to him; considering that where the error had originated, was the proper place from whence to announce the correction.

The greatest equation that is required by the assumption of the obliquity of the ecliptic $8\frac{1}{2}$ seconds in error, is only $\frac{1}{10}$ of a second in time for the right ascension of the sun; and the whole quantity at the solstices for the declination: the former is not worthy of notice; and the latter may be corrected by a table of the sun's declination, for each degree of longitude, with the variation for the differene of 10

* Since the above was written, I have been informed that the French have stated, either in their *Connoissance des Tems* or in the New Solar and Lunar Tables lately published, that they have availed themselves of the calculations contained in the Nautical Almanac, having occasionally calculated two or three of the first months at the beginning of the year, to satisfy themselves of the degree of care with which the Nautical Almanac has been computed.—T. F.

seconds

seconds in the obliquity. Notwithstanding, should any gentleman require the above two parts of the Nautical Almanac, for the use of his own observatory; whether for the correction of the tables, or the convenience of setting his instruments; I should recommend him to avoid the labour of calculating the same from the solar tables; whereby he might fall into a greater error, unless he should employ several computers. In that case, he might safely depend on the sun's longitude given in the Nautical Almanac, and, allowing for his difference of meridian from Greenwich, with the apparent obliquity of the ecliptic so corrected, find the true right ascension and declination of the sun. Indeed, were he to profess to have made the original calculations, and these were to agree relatively with those of the Nautical Almanac, he would be suspected of having used the same plagiarism which is ascribed to the French and American astronomers in their publications.

With respect to the *Connoissance des Tems*, the compilers acknowledge that they have depended on the calculations published in the Nautical Almanac; having on former occasions found them correct. From the superior industry of the French mathematicians, the Solar and Lunar Tables have been constructed; although the theories thereof have been principally confirmed by the numerous and accurate observations of the sun and moon, which have been made at Greenwich, during a long series of years, with the greatest skill, and the best instruments that have been hitherto produced.

I am, sir, your obedient servant,

Blackheath, Nov. 15, 1811. S. GROOMBRIDGE.

LXX. *Some Account of the different Theories of Arches or Vaults, and of Domes, and of the Authors who have written on this most delicate and important Application of Mathematical Science.* By a CORRESPONDENT.

FROM the general history of the progress of the inquiries into the theories of the equilibration of vaults, it will be seen, how important the subject has been considered by those who have not been architects, but have attained the highest eminence in mathematical and mechanical science. In reading this history of contrarious results, and in surveying and turning in the mind the vaults now subsisting, the productions of architects unacquainted with the present theories, much caution is necessary, lest the conclusion

 be

be drawn, that the question is merely curious, and the investigation of it useless except as a mathematical exercise. The brief account which is here given of the different theories will be found sufficient, by extending the application, to exercise the mathematician in his school attainments, and to enable the architect to compare his knowledge, arising from practice, with that derived from theory, and eventually to obtain that which may be permanently useful in his art.

By way of preface, it will be proper to look at an arch in the state it appears in a wall or bridge, freed from many hypothetical properties which have been ascribed to it.

An arch is composed of stones called *vaussoirs*, in the shape of truncated wedges, which resist each other, through their inclined sides, by means of that weight whereby they would otherwise fall, and are suspended in the air without any support from below, where a concavity is formed. The vaussoirs are subject to forces which arise from their own weight, from external pressure, from friction, and the cohesion of matter. All these forces compose a system which ought to be in equilibration; and moreover, that state of equilibration ought to have a consistence firm and durable.

The respective actions of the vaussoirs must be very different, according to their position in the vault: the vaussoir of the middle of the arch, which is vertical, and is called the key-stone, is sustained on each side by two vaussoirs precisely as by two inclined planes, and consequently the effort which it makes to fall is not equal to its weight, being so much less as the planes are more inclined by which it is sustained: if the planes were perpendicular to the horizon, as well as the sides of the key-stone, it would fall by its whole weight. The second vaussoir, on the right and left of the key-stone, is sustained by a third, which by virtue of the figure of an arch is more inclined in respect to the second than the second is to the first. By a parity of reasoning, all the vaussoirs, reckoning from the key of the vault, exert a decreasing portion of their whole weight, until the la-t, which lying horizontally does not exert any, or, which is the same thing, does not make any effort to fall, being wholly sustained by the base on which it rests.

If it be desired that all the vaussoirs should be in equilibration, it is manifest, that as each vaussoir, in proceeding from the key-stone, exerts only a part of its weight, the first for example exercising a half, the second a third, and the third a fourth, &c. it follows, to equalize their different actions,

actions, that each must be augmented according to its position; the second must be made more heavy than the first, the third than the second, and so on until the last, which must be infinitely heavy, because it does not make any effort to fall.

To render this subject practically more intelligible, we have only to consider, that every vaussoir except the last, in letting another vaussoir fall, must itself rise, and that it resists this elevation to the extent of the weight which it itself exerts to fall; and that only the last vaussoir on each side can let another fall, without itself rising, as it has only to slide along its horizontal bed: as a finite weight has not any power of resistance to a horizontal motion, every thing being considered lubricous; we must conceive the last vaussoir to be infinitely heavy, to made any lateral resistance.

An arch * may stand immediately on the earth, which is its base, or be sustained by a wall or abutment pier: in both cases the joint effort of the parts is communicated to the base, which is its foundation, as if it were one and the same body continued.

Curves used in arches are of three characters: 1st, Arcs of circles, wherein the height must always be equal, or less than half the width; in the first case the tangent at the springing will be vertical. The curtate cycloid is applicable in all the conditions of the circle, and the tangent at the springing may be vertical. 2d, The arcs of ellipses, whose widths may bear any proportion to their heights, and the tangent at the springing may be vertical. 3d, The catenaria, the parabola, the hyperbola, &c. whose spans may bear any proportions to their heights, but whose tangent at the springing cannot be vertical.

The natural consequence from these data seems to be, that when the given height of an arch exceeds half the width,

* The arch has been considered as a curve, infinitely thin, uniform throughout, and composed of an infinite number of joints; and the inquiry has been to determine the weight which may be placed upon each joint, in a direction perpendicular to the horizon, so that it may retain its position. In this way of considering the subject, the pleasing analogy between the chain and the arch, as applied in a popular experiment, together with the happy adaptation of the modern analysis to determine formulæ for universal practice, have, in the elegant display of the means, blinded the inquirers as to the end, and as to the absolute properties of the arch itself. It even has appeared to men of other habits, that if such an hypothesis were applicable, it must be subsequent to the determination of the arch, or the curve of infinite joints, which must at all events be determined on the principle of the wedge, or a collection of bodies butting on each other; the lower being an inclined plane to the superior; and then the weight, *however supposed* to bear on the arch, must be determined in relation to this previous investigation.

 the

the circle is inapplicable; and that some other curve should be adopted; but the simplicity in the construction and ancient practice have decided otherwise, and the semicircular arch has been generally sustained on vertical piers, where an arch of greater altitude might have been adopted.

By the theory elicited from an examination of the arch, it would follow, that if the vaussoir at the springing, lying on a horizontal bed, must be infinite, in like manner the pier on which it is sustained must be infinite also; because it can be considered only as another vaussoir, or an additional succession of vaussoirs: we may conceive the second vaussoir of an arch to be an abutment to the first, as the third is to the second; the hance [query haunch] is as much an abutment to the crown as the pier is to it. The abutment or arch cannot be said to begin anywhere; it appears therefore necessary, that the same theory should have cognisance over the whole, and that whatever theory may be applicable to the one, must also apply to the other: this mode of considering the subject, again confirms the propriety of adopting practically, what has appeared a natural consequence from the consideration of the properties of curves used in arcuation. It is manifest, that an arch may assume externally any appearance, and exhibit on its face a semicircular arch and abutment piers, and yet have internally the properties and proportions of another curve, which may be enclosed in that form. Admitting this, the theory of abutment piers does not in any wise differ from that of arches; for if an arch of equilibration be enclosed in an arch of the same thickness at the vertex, and its abutment-pier, it necessarily follows that they must stand; if the additional filling up, to produce the desired effect, does not alter that equilibration: indeed, the angle at which friction retains stone on an inclined plane would determine that in all common cases; and though the direction of the joints of the vaussoirs must be assumed, in the investigation, at right angles to the intrados of the enclosed arch, yet they need not be so in practice, but have that relation to the visible arch.

This principle of interpreting an arch standing on a pier, into an arch of greater altitude, whatever theory of equilibration of arches may be adopted, seems to offer a mean of ascertaining the correctness of any different theory that may be adopted for the piers, when considered separate bodies, the enclosed arch being of the same curvature at the vertex, and nearest approaching to the given arch and pier. On the contrary, the theory of abutment piers has

has been considered by some in a point of view distinct from that of arches, and indicated by the suppositions, that the arch is most apt to split in the middle between the impost and the key-stone; that the snperior vaussoirs above this point of fracture are bound together, forming one vaussoir or one wedge; and that the lower vaussoirs are so attached to the abutment pier, that they also form one stone, liable to revolve on the external lower angle of the pier, by the action of the upper part of the arch, involving the joint action of a wedge or inclined plane, and lever. The same principles are applicable, wherever the joint of splitting may be considered to be.

[To be continued.]

LXXI. *Notices respecting New Books.*

Extract from a Paper entitled "The Result of Eleven Years Practice at the Original Vaccine Pock Institution, No. 44, Broad Street, Golden Square," &c. 8vo. pp. 46. Printed by Reynell, 1811. Sold at the Institution.

THE public must feel much interested by this publication, containing the results of the practice of vaccination for a longer period distinctly than any other institution of a similar kind. Besides, the medical establishment being perfectly independent of any pecuniary remuneration, but on the contrary supporting the Institution chiefly at their own expense; they are not likely to be warped in their judgement, nor to be tempted to give unwarrantable or exaggerated accounts to answer private views. It is well known too, that this Society has been the first to make the public acquainted with the defects of vaccine inoculation, as well as with its advantages.

After a brief but very condensed history of the new inoculation, and a more particular account of their Establishment, in which mention is made of their own attempts to investigate the laws of agency of vaccine matter, the authors relate the more remarkable results of their practice. They desire to be considered as merely giving evidence, and distinctly disclaim any interference with the opinions or facts asserted by others. For many of these particulars, we refer to the pamphlet itself; but certainly they feel responsible for the facts asserted, however great may be their discrepancy from those of other Reports. We shall notice a few of these facts.

Matter taken early, *i. e.* before the 9th day, is more efficacious

cacious than at a later period; but it is alleged that no other consequences arise; the presence or absence of the areola furnishes no rule independent of the period of the pock. The health does not alter the matter. The rule for taking matter should be not according to the day after inoculation, but according to the state of the pock,—but *usually* the most efficacious matter is before the 8th or 9th day:—no mischief has been observed from the matter of irregular or anomalous pocks; but it is liable to fail in producing the due effect:—no such matter has been seen as that called *spurious*, *i.e.* which produces successively a disease mistaken for the cow-pock, but which does not give security against the small-pox:—no constitutional affection has been observed on the 5th day, as asserted on high authority, as well as at the well known period the 9th after inoculation. Several insertions in the same arm do not excite more inflammation than a single puncture, but they have the advantage of affording greater security.—Constitutional affections from vaccine and variolous matter have not been observed to coexist, so that there is no risque from vaccine inoculation when a patient has been exposed to the contagion of the small-pox.—Not one has died at the Institution, but a few cases have been communicated of death, by inflammation of the arms, and one perhaps by convulsions. A Table is given of the proportional mortality by the small-pox during twelve years preceding vaccine inoculation in London; and another during the twelve years of the practice of vaccination, to judge of its influence in diminishing the mortality in society by the small-pox. It appears that about 420 persons per annum died fewer during the twelve years of the new inoculation, than during each of the twelve preceding years before its introduction; yet the conclusion that the diminished mortality has been occasioned by the cow-pock is liable to error, although the probability is in favour of the influence of the new practice. With regard to the grand question which agitates so much the public mind, and which alarms so much particular families, viz. the security against the small-pox, it appears that out of 5000 registered patients who had all gone through the cow-pock in the most distinct manner, *nine* have subsequently taken the small-pox; and 40 more failures have been substantiated elsewhere, exclusive of the hon. Mr. Grosvenor's case, and some others communicated since this paper was read.

The very curious new pathological fact is noticed of the failure in particular families, apparently from a congenital unsus-

unsusceptibility, so that very often two or more out of several children in such families took the small-pox after the cow-pock. According to the severest tests, a very large proportion were found unsusceptible of small-pox after vaccination. Those who took the small-pox naturally, in at least 19 cases out of 20, had this disorder as mild as in the mildest inoculated small-pox: hence another new pathological fact, and without analogy, that the human animal œconomy, although not rendered incapable of the small-pox, may be rendered incapable of this disease severely, by undergoing the vaccine.—Lastly; except perhaps some slight eruptive complaints, no new disease has been introduced by the cow-pock.

In this publication particular directions are given for the mode of conducting vaccination. The rules also are specified to be observed by those who claim the compensation of five guineas on account of having been affected with the small-pox after having been certified to be secure by means of the cow-pock, by the medical officers of this Institution.

Mr. Gillman, Surgeon, of Highgate, is about to present the Public with a Treatise on the Bite of a Rabid Animal, being the substance of an Essay which received a prize from the Royal College of Surgeons of London.

The Second Part of the Philosophical Transactions has made its appearance. The following are its contents:

11. On the Causes which influence the Direction of the Growth of Roots. By T. A Knight, Esq. F.R.S. In a Letter to the Right Hon. Sir Joseph Banks, Bart. K.B.P.R.S. —12. On the Solar Eclipse which is said to have been predicted by Thales. By Francis Baily, Esq. Communicated by H. Davy, Esq. Sec. R.S —13. An Account of the great Derbyshire Denudation. By Mr. J. Farey, Sen. In a Letter to the Right Hon. Sir Joseph Banks, Bart. K.B.P.R.S. —14. An Account of an Appendix to the small Intestines of Birds. By James Macartney, Esq. F.R.S.—15. An Account of a vegetable Wax from Brazil. By William Thomas Brande, Esq. F.R.S.—16. Astronomical Observations relating to the Construction of the Heavens, arranged for the Purpose of a critical Examination, the Result of which appears to throw some new Light upon the Organization of the celestial Bodies. By William Herschel, LL.D.F.R.S. —17. Experiments to ascertain the State in which Spirit exists in fermented Liquors: with a Table exhibiting the relative Proportion of pure Alcohol contained in several Kinds

Kinds of Wine and some other Liquors. By William Thomas Brande, Esq. F.R.S.—18. Account of a Lithological Survey of Schehallien, made in order to determine the specific Gravity of the Rocks which compose that Mountain. By John Playfair, Esq. F.R S.—19. Observations and Experiments on Vision. By William Charles Wells, M.D. F.R.S.

LXXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Thursday, Nov. 7. This evening the Royal Society met after the long vacation, the right hon. President in the chair. A mathematical paper by Mr. Ivory was laid before the Society, detecting some errors in Laplace's *Mécanique Céleste* on spheroids. The introduction only of this paper was of a nature to be read, in which the author acknowledged the talents and learning of the French mathematician, and extenuated the mistake he was about to demonstrate by considering the immense extent of the work which contained it.

Nov. 14. On this evening another paper on spheroids, by Mr. Ivory, was also laid before the Society, and a part of its contents read. In this the author proposed a new theory of spheroids, after having examined that of Newton and of Laplace; the latter he refuted in the preceding paper, and the former he showed was founded on the assumed position that the earth was once an entire fluid mass: but had that been the case, its present appearance must have indicated that the fluid matter followed the laws of specific gravity; which is not the fact. He then proceeded to show what must be the nature of a spheroid circumstanced as our globe is, and consisting of land and water.

Nov. 21. A part of a paper by Mr. Glenie, on the quadrature of the circle, was read, in which the author conceives that he has discovered a method of approximating to a solution of that long discussed problem with sufficient accuracy.

The introduction to an experimental inquiry into the nature, formation, and constituent parts of the blood, by Mr. Brande, was read. The author began with noticing the difficulties and inaccuracy of all our methods of analysing animal matter; examined the process adopted by Fourcroy and Vauquelin; and was hence led to detect many of their errors, and particularly their opinion, which has now become

come general, of the existence of iron in the blood; which he appears to think wholly unfounded. He took a review of the process by which the living animal converts its food into chyle, and thence into blood; investigated the nature of serum, and analysed this fluid with much greater minuteness and scientific accuracy than have hitherto been used.

LXXIII. *Intelligence and Miscellaneous Articles.*

To Mr. Tilloch.

SIR, BEING a student of astronomy, and possessing a very scanty astronomical apparatus, I feel much indebted to those gentlemen who have kindly communicated the various observations on the comet, that have appeared in your journal. The following elements of the orbit deduced from the observations of Sept. 5th, given in your work, combined with two of my own made with a sextant, Sept. 30th and Oct. 20th, will be found, I believe, to represent the whole series yet published with considerable accuracy. If I am not anticipated in the same results, you have my leave to publish them.

Perihelion distance 1,03557.

Passage of perihelion Sept. 12th at $7^h\ 31^m$ Greenwich mean time.

Long. of ascending node	140°	22′
Inclination of the orbit	73	3
Long. of perihelion	74	57
Retrograde.		

I am, sir,

Your most obedient servant,

St. Andrews,
21st Oct. 1811.

T. J.

P. S. I venture to suggest, what I have often wished, that the astronomer royal, or any other gentleman who makes observations under a well ascertained meridian, would sometimes favour us by communicating in the periodical journals, the observed times of occultations and solar eclipses. I am satisfied that it would lead to many observations of the same kind elsewhere; and thus both excite a spirit of astronomical observation, and improve the geography of the countries where our monthly publications circulate.

RUSSEL

RUSSEL INSTITUTION.

A Course of Lectures on Electricity, comprising the History and Practice of that Science, its Application to Meteorological Phænomena, and the Extension of Chemical Knowledge, will commence at this Establishment towards the close of December. They are to be delivered by Mr. George Singer.

LIST OF PATENTS FOR NEW INVENTIONS.

To Edward Silvester, of Rochester, in the county of Kent, millwright, for his new drag or skid, to be applied to the wheels of carriages of different descriptions.—Sept. 14, 1811.

To Johannes Ambrosius Maas, of Hammersmith, in the county of Middlesex, gent., for his improvement in the making of vinegar.—Sept. 23.

To James Needham, of Islington Green, in the county of Middlesex, brewer and corn-dealer, for his portable apparatus for brewing beer and ale from malt and hops.—Sept. 23.

To William Strahan, of Poole Cottage, in the county of Chester, chemist, for his new method of making salt.—Oct. 1.

To John Miers, of the Precinct of the Savoy, in the county of Middlesex, jeweller, for a new method of accelerating the evaporation of liquid or fluid bodies, destroying the noxious and offensive effluvia arising from spent soap lyes, or other liquid, fluid, or solid substances, and generating an increased degree of heat without additional fuel.—Oct. 30.

To Frederick Koenig, of Castle-street, near Finsbury-square, printer, for further improvements on his method of printing by means of machinery.—Oct. 30.

To Richard Witty, of the town of Kingston-upon-Hull, gent. for his further improvements in his invention for the construction of steam engines, secured by letters patent bearing date the 14th day of February 1810.—Oct. 30.

To Joseph C. Dyer, of Boston, State of Massachussets, one of the United States, now residing in Gray's Inn, merchant, who, in consequence of a communication made to him by a certain Foreigner residing abroad, is become possessed of certain machinery to be used and applied in manufacturing cards for carding wool, cotton, silk, flax, and tow, and other fibrous materials of the like description.—Oct. 30.

To

To Richard Lomax Martyn, of Tillington, in the county of Sussex, clerk, for an instrument for hoeing turnips, and for other farming purposes, which he denominated an "Agricultural Hoe."—Oct. 30.

To William Rudder, of Birmingham, in the county of Warwick, cock-founder and warming-pan maker, for certain improvements in the construction of cocks used for the purpose of drawing ale, porter, beer, cyder, wine, water, and other liquids and fluids.—Oct. 30.

To Thomas Davies, of Brewer-street, in the county of Middlesex, goldsmith, for certain improvements in the construction of buckles for various purposes.—Oct. 30.

To John Curr, of Bellevue House, in the parish of Sheffield, in the county of York, gent. for certain methods of laying or making ropes, whereby the strands after being hardened are kept in more regular distention, and the ropes are more uniformly twisted than when laid by the sledge. —Oct. 30.

To Thomas Pearsall, of Willsbridge, in the county of Gloucester, iron-master, for a new method of constructing iron work for certain parts of buildings.—Oct. 30.

To John Lowndes, of Hollen-street, Soho, in the county of Middlesex, modeller, for an improved method of warming or heating baths.—Oct. 30.

To William Close, of Dalton, in the county palatine of Lancaster, apothecary, for his improvements in trumpets of different denominations, namely, the treble or common trumpet, the French-horn or tenor trumpet, and the bugle-horn.—Nov. 2.

To Charles Broderip, of Great Portland Street, in the county of Middlesex, gent. for certain improvements in the mode of constructing steam engines.—Nov. 2.

To Charles Random De Berenger, formerly of Germany, but now residing in Pall Mall, in the county of Middlesex, gent. who, in consequence of information received from a certain learned Foreigner residing in parts abroad; and from various researches and experiments made by himself, hath found out certain means of producing a valuable oil, and also soap and barilla, and a black colour or pigment.—Nov. 21.

To Joseph Baker, of Butler's Green, near Cuckfield, in the county of Sussex, navy contractor, for an improved method, by means of machinery, of kneading dough without manipulation.—Nov. 23.

To James Adam, of Petkellony, in the county of Perth, esq. for a new method of drying malt, and all other kinds of grain and seed.—Nov. 26.

METEORO-

METEOROLOGICAL OBSERVATIONS.

To Mr. Tilloch.

SIR, AN account having appeared in several of the daily newspapers, of a large meteor seen in the neighbourhood of London in the evening of Thursday the 7th instant, I proceed to communicate to you such particulars relating to it, as I have been able to collect from several persons who saw it at Hackney. According to their account it appeared about five minutes before nine o'clock in the north, and moved in a direction to the west: its motion was not regular in a straight line, nor in a uniform curve; but it leaped forward by successive jerks, describing a sort of undulated track; and they represented it as being of considerable magnitude: after being visible for some seconds, it apparently entered a cloud and disappeared. The circumstance of its peculiar motion is, I think, worthy of record; and if any of your readers should have made observations on it, at any considerable distance from London, I should be obliged to them to communicate the same in the Philosophical Magazine.

In investigating the causes of these luminous accensions, I think we may perhaps be assisted by observing and noting down accurately the peculiarities remarkable in the different kinds of them, which from time to time appear. The very large sort, which occasionally are seen, such for example as that memorable meteor which happened on the 18th of August 1783, or the large one recently observed at Geneva, are not numerous enough to admit of being arranged under any general description; besides that there are peculiarities in all of them, whereby each differs from every other. But the smaller kind which appear in common seem to me to be of three distinct varieties, and appear to derive their particular character from the kind of weather in which they happen.

The most common sort are those very small meteors which are prevalent in clear frosty winter nights, and indeed in summer also when there are dry easterly winds and very clear skies. They have very much of the appearance of the real stars, and have probably from this circumstance derived their vulgar name: they leave little or no train behind them, and shoot along in straight lines generally obliquely downward, but sometimes horizontally.

The second kind are larger and more brilliant, and generally

nerally appear in warm summer evenings, particularly when *cirrocumulus* and thunder clouds abound: some of them are very beautiful, and give much light: they vary somewhat in colour and size.

The third sort are strikingly different from the two above mentioned: they are generally small, and of a beautiful blueish-white colour; but their peculiar characteristic is that of leaving long white trains behind them, which remain visible for some seconds in the tract in which the meteors have gone. These tails seem to be lost by dispersion; they appear to fly off from all points, increasing in breadth as they become fainter, till at last they cease to be distinguishable. They are generally seen in the intervals of showery weather, and are most prevalent before the occurrence of high wind; of which they have been considered by Virgil as a certain prognostic*. These luminous tails have so much the appearance of the burning of some gas, that I have been induced to propose it as a question, Whether hydrogen may not be so diffused in the atmosphere, that it may be lighted by the meteor as it passes through it†? From what I have been able to observe, these tails seem to result rather from some such diffused gas set on fire by the meteor in its passage, than from any of the luminous substance of the meteor left behind it. It may be also remarked, that if the larger kind of meteors happen at the same time that these caudate meteors are prevalent, they always leave the tail behind them‡.

I shall not, at present, speculate further on the causes of igneous meteors in general, nor on the varieties in the state of the atmosphere, by which their peculiarities may be effected, hoping that meteorologists will note them down accurately in their journals.

I remain, sir, your humble servant,

Clapton, Nov. 15, 1811. THOMAS FORSTER.

* *Georgic.* lib. 1.

† The separation of the gases of water has been noticed by M. B. P. Van Mons.

‡ The train of light which the common meteors or *falling stars* appear to leave behind, and which lasts scarcely a moment, is probably only a hallucination of vision like the Δολιχοσκιον εγχος sung by Homer, and quoted by Dr. Darwin, *Zoon.* sect. iii. 5. 3.—to which, as well as to his paper *De Oculorum Spectris*, I refer the reader.

METEOROLOGICAL TABLE,
By Mr. Cary, of the Strand,
For November 1811.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Oct. 27	47	55°	44°	28·86	15	Cloudy
28	44	55	47	·87	5	Stormy
29	45	55	46	·89	6	Stormy
30	46	56	45	·90	10	Cloudy
31	47	59	57	29·78	18	Fair
Nov. 1	52	62	61	·72	0	Rain
2	62	63	52	·65	6	Showery
3	50	57	58	·69	10	Fair
4	51	55	46	·84	0	Showery
5	52	57	47	·90	0	Rain
6	48	56	50	·57	0	Rain
7	49	52	51	·58	10	Fair
8	49	51	50	·52	0	Rain
9	50	53	50	·70	12	Fair
10	50	53	46	·35	0	Stormy
11	43	53	43	·32	9	Fair
12	39	49	46	·90	32	Fair
13	49	51	45	·70	0	Rain
14	47	52	44	·72	0	Showery
15	43	47	36	·52	26	Fair
16	42	48	38	·50	0	Rain
17	40	49	43	30·05	30	Fair
18	44	51	47	·25	0	Small rain
19	47	52	41	·36	10	Showery
20	36	45	34	·39	15	Fair
21	33	42	40	·28	10	Fair
22	40	48	32	·21	12	Fair
23	28	43	34	·19	0	Foggy
24	36	46	37	·22	10	Fair
25	43	51	42	·32	0	Small rain
26	42	47	43	·38	7	Cloudy

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

ERRATA.

Page 308, in the Observations of the Comet,
Oct. 2. for 6^s 5° 45′ 46″ *Long.* read 5^s 24° 14′ 37″
Oct. 12. for 229° 39′ 28″ *A. R.* read 229° 31′ 28″.

LXXIV. *Some Speculations on the Analogy between Man and the Brute Creation.* By ARTHUR MOWER, *Esq.*

[No. III.]

In endeavouring to prove that the lower animals possess an intellectual principle, whose powers and faculties answer to the definition of the human soul; I have, it is true, cited only one instance, and that of a domestic quadruped. I conceive, however, that if it is satisfactorily shown, that even one individual of the brute creation has displayed in its actions a power of perception, memory, intelligence, and volition; and if at the same time it is certain that these powers and faculties can be exerted only by some principle essentially the same, or analogous to the soul of man, then it must be a fact firmly established, that other animals besides our own species possess some such same or analogous principle.

It is not by simply attending to the more intricate actions of the lower animals, that we alone discover signs of an intellectual principle more independent in its operation than the law of instinct; we must ascend yet higher: let us reflect on the passions and feelings which supply motives to such actions, and we shall then be sensible of a closer analogy between men and the brutes, than the pride of many of the former, perhaps, will suffer them to acknowledge. Observe a Newfoundland dog plunge into the water, and, seizing the hair of his drowning master, draw him in safety to the shore. Look at a terrier guarding for three months the remains of his benefactor, his friend and companion, who perished on a mountain. Shall we say that these animals acted merely from instinct? from no other cause than a certain blind impulse, and without any view to consequences? Shall we assert that Providence had so organized their frames, had so subjected them to the mysterious uncontrollable influence of a certain law, that they could not have acted otherwise? Will the warmest advocate for the superiority of his species deny, that in the first instance a dog felt as deeply, reasoned as acutely, and acted as wisely, as he himself could have done? When the little terrier lost his master, is it fanciful to suppose that his mind dwelt with gratitude on the remembrance of a benefactor who had so often fed, so tenderly caressed him?—No:—he doubtless recalled to his recollection a thousand acts of kindness; at the expiration of three months he was still watching his remains:—and did not this creature display as much sensibility and real feeling as half our species would have done at the loss of such a friend? Was it chained to the spot by the mechanism of a clock-work principle? No, no: let us acknowledge that other animals are sometimes as intelligent as ourselves;—let us adore the Creator of the universe, who in giving to brutes an intellectual principle capable of memory, volition, and possessing in a limited degree the faculty of reasoning, has not only provided for their immediate wants, but graciously multiplied their means of happiness. It is natural to suppose that the lower animals enjoy from their cogitations a very high degree of pleasure. A beast, a bird of prey, reposing in the solitary gloom of a deep forest, when not actually sleeping, enjoy a positive happiness, from the recollection of their last meal; from the anticipation of another; or in forming vague plans for surprising their next prey, when hunger shall impel them to pursue it. A little bird confined in a cage, and supplied with a sufficiency of food, is happy either in motion, or when sitting on its perch; it observes the actions of others, or gives itself up to a succession of confused ideas, of which perhaps it is scarcely sensible, and which leave not a trace behind. If we will not allow that brutes ever think; if we suppose them incapable of a single idea, however simple; it must certainly follow, that they are mere automatons, moved by mechanism: but if, as appears evident, they are capable of thought, it is equally clear that they must have a soul; for thought is an act of the mind, and mind (though as a substantive it is a word which sometimes expresses a power or faculty of the soul) in its largest sense is synonymous with soul.

Let it not be thought that I wish to degrade men to the rank of yahoos, and insinuate that they are inferior beings to horses. Many people, I know, will think it almost impious to suppose a dog to have a soul, because, they will say, the human soul, though now degraded by sin, was originally as much an emanation from the Creator, as a ray of light is an emanation from the sun; and the word itself implies an immortal spirit. But these are objections which appear to be supported neither by reason nor common sense. Man in his most perfect state was "lower than the angels;" the highest of archangels we must suppose infinitely below the Deity, as the lowest of the angels is below that archangel. Rays of light, proceeding from the sun, may be more or less imperfect and obscure by passing through bodies of a different density; light, as well as air, is capable of deterioration: but the Almighty is as perfect

in

in his nature as in his attributes; whatever is an emanation from his essence must be as perfect as that essence,—else must that essence itself be capable of imperfection. If the ray of divine light is not as pure and perfect as the source from whence it emanates, it is plain that a capability of imperfection must previously have existed in the whole of which it is a part. But this capability is incompatible with perfection, and we suppose God to be a perfect being. The soul of man, therefore, as well as those beings whom we call angels, must as necessarily have been created as the dust we tread upon. A created soul is as dependent for its existence on the Creator, as are any of the material objects by which we are surrounded, and can only become immortal by his permission; as motion cannot continue but by a repetition of impulse, or the constant action of the law by which it first began. Set an ourang-outang, indeed, or the most sagacious brute, by the side of a D.D, an LL.D., or an F.R.S.; compare their respective powers, faculties, and qualities, and the difference will be certainly so great, that it will appear shocking to make any comparison between them, and not easy to imagine that the one can have an intellectual principle analogous to that of the other. They were brought into the world for very different purposes; the one to enlighten and improve mankind, and the other to fill up a gradation in the great scale of animated beings. To answer these different ends, their capacities were differently proportioned; and the man is destined for immortal glory, while the brute must perish when he ceases to breathe. But let us not look only at the top of the scale, but contemplate human nature in its most savage as well as in its most civilized state. The lowest of the human species are said to be the Andamans, a race inhabiting islands of that name on the eastern side of the bay of Bengal. Their mode of life is degrading to human nature; and, like brutes, their whole time is spent in search of food. In the morning they rub their skins with mud, and wallow in it like buffaloes, to prevent the annoyance of insects. Their dwellings are the most wretched hovels imaginable. An Andaman hut may be considered as the rudest and most imperfect attempt of the human race to procure shelter from the weather. Three or four sticks are planted in the ground, and fastened together at the top in the form of a cone, over which a kind of thatch is formed with the branches and leaves of trees. An opening is left on one side, just large enough to creep into, and the ground beneath is strewed with dried leaves, upon which they lie*. Consider also the many tribes of human beings on the coast of New Holland, whose lives, with little variation, are spent, like those of brutes, in the pursuit of prey and in gratifying their appetites: with these, and not with members of learned societies, let us compare an eagle, a wolf, an ourang-outang, or a dog;—observe their intellectual powers displayed in their daily actions, their pursuits, their habits, their passions, their feelings;—is it too degrading to our nature, does it savour of impiety, to believe that these beings are in many respects equally rational, equally free agents?

Between an Andaman and a Sir Isaac Newton there appears to me to exist as great a distance as between an Andaman and an ourang-outang: yet would it be too revolting to the pride of a Sir Isaac, to acknowledge, that though the savage was governed far more by instinct than by reason, yet it was possible that he might possess a soul as well as himself? If we reject with due abhorrence and contempt the revolting, silly idea, that God created this world and its inhabitants as a display of power, and merely for his own glory; and that he keeps the most numerous part of animated beings under the control of instinct, to make that power and glory more conspicuous:—if, instead of this, we believe that the Almighty undertook the work of creation for the diffusion of happiness,—the face of creation will wear a very different appearance. Instead of this fair earth being created for a display of omnipotence, and destined for the use of an animal, certainly, not always the most respectable of living creatures; instead of viewing a multitude of automatons moved by clock-work, and made solely for the use of lordly man; we shall behold myriads of beings called into existence for very different purposes indeed, but all to serve as means to one great end, the diffusion of happiness. We shall behold the lower animals possessing a soul, or intelligent principle, analogous to that which animates the human species; as far as it reaches, as rational, as free as independent; but more limited in its powers, and confined only to subjects of observation and experience. These animals think, reason, will, and act; but they are spoken of in Scripture, as of "beasts that perish." Annihilation, however, is to them no evil. The brutes in this world are in general much happier than man. Our superior misery is as strong a proof of our immortality as

* Asiatic Researches, iv. p. 390 and 391.

our

our superior faculties. The phænomena then which the brûtes exhibit by their actions, authorize us to conclude that they possess a soul, or intelligent principle, analogous in its essence to that of our own, but more limited in its powers and capacity*.

That natural difference of intellect which we observe between a dog and a dormouse, between a dunce and a man of genius, is undoubtedly owing to some difference in the sentient or cogitative substance; it cannot be ascribed to matter, for matter is continually changing; and though a man at thirty has not a particle of the same body which he had at ten years of age, yet it is very plain that he may be as great a blockhead. That such a disparity in mental capacity is owing to an entire difference in the essence of the soul, cannot be supposed; for although it is equally easy for Providence to endow the most opposite substances with similar intellectual powers, yet the simplest hypothesis is always to be preferred as the most probable; for nature always acts by the simplest plan, and seldom, if ever, deviates from general laws to accomplish particular phænomena. That gradation, then, of intellectual capacity, which distinguishes from each other individuals of the human species, as well as of the brute creation, is to be ascribed, not to an entire difference in that substance called the soul, but to a different modification of that substance. But it will be asked, What is that substance? and how is it modified?

The essence of the soul, which has been a subject for speculation to philosophers of every age, has certainly given rise to many extraordinary theories; and it would be thought folly to waste much time and paper on a topic which no human capacity can ever satisfactorily explain.

* In the article *Instinct*, in the new Cyclopædia, we read the following: "When we see brutes thus using means to obtain their ends, as well as ourselves, must we not conclude that they reason? When the cat watches for hours in silent expectation of her prey; when the hound traverses a wide extent of country in the chase, they show as much persevering voluntarity as man can boast." If I know any thing of the meaning of words, this is as much as to say that brutes do sometimes reason, and act from reason, and that they have a power of volition. But at the end of the same article we are thus given the sum of the argument: "Thus we see that animals perform certain operations which are neither *rational*, habitual, nor mechanical; and although it cannot be doubted but some of them reason in several instances, still, even from the short details now given, we cannot allow that their natural operations are performed with a view to consequences." But do brutes reason, or do they not? If they do, then some of their actions *must be rational;* if they do not, they act entirely from the law of instinct, a law which they can neither alter nor control; and their actions are then as purely mechanical as is the movement of the minute hand on the dial-plate of a timepiece.

Language being entirely relative to objects with which we are surrounded, and which we call material, it is impossible to build any hypothesis on the nature or essence of the soul, without conveying to the reader an idea of a material substance; and it appears to me of little consequence, whether we imagine the soul to be material or immaterial. What is meant by the word spirit? It will be answered, An immaterial something in which thinking is inherent. But what reason have we for supposing that this something, this soul, may not be a portion of subtile matter?—not a medullary substance like the brain, or a combination of particles similar to those which compose our bodies; such matter is divisible, undergoing changes every moment, and by its nature liable to corruption: but can we suppose no matter with extension, but indivisible and incorruptible? Is it impossible for such a substance to exist? It is indeed plain, that the idea of corporeal substance is as remote from our conception, as that of spiritual substance, or spirit. We conclude that the operations of the mind, thinking, reasoning, &c. cannot subsist of themselves, nor can we apprehend how they can belong to body, or be produced by it. We therefore think these the actions of some other substance, which we call spirit. Of matter we have no other idea or notion, but something wherein those many sensible qualities which affect our senses, to subsist. We have, therefore, as clear a notion of the substance of spirit, as we have of body; the one being supposed to be (without knowing what it is) the substratum to those simple ideas we have from without, and the other (with a like ignorance of what it is) to be the substratum to those operations which we experience in ourselves within*. Again, "The immediate objects of sense, philosophers have agreed to term qualities, which they conceive as inhering in something which is called their subject, or substratum. It is this substratum of sensible qualities, which in the language of philosophy is denominated matter: so that matter is not that which we immediately see and handle, but the concealed subject or support of visible tangible qualities†." Spirit and matter, therefore, are merely an arbitrary union of certain letters, to signify two substratums, or two somethings, we know not what, on which to rest certain qualities:—But is it impossible to suppose a third substratum to exist, supporting a union of qualities drawn from the two former? Let us imagine a something, to which belong

* Locke's Essay. † Ency. Brit. article *Matter*.

all

all the operations of the mind, with incorruptible parts and indivisibility; what shall we call this something, or this substratum which supports these qualities? It cannot strictly be called matter, for to matter belongs the quality of divisibility; neither is it spirit, for spirit we allow to have neither parts nor extension. Whatever we agree to call it, it is not an improbable conjecture, that such a something with such qualities may exist. That the soul of man is powerfully acted upon and influenced by matter; that it is often held in subjection by the body, and strongly oppressed by physical causes, cannot be denied: but pure spirit is independent of matter. Were our souls purely spiritual and immaterial, our mental constitution would be totally different; we should be divinities, instead of human beings. Englishmen would not then shoot themselves on a foggy day, nor lunatics grow worse when the moon changed. But at present surrounding objects act upon our material bodies, to which they have an affinity; this action operates on the soul, through the medium of our nerves and fluids; and as no matter, as far as we have reason to suppose, can act on other substances than those to which it has some affinity, how could our nerves operate on the soul, or sentient principle, if that soul was purely spiritual, and had no affinity to matter? I will imagine a very subtile, invisible, ethereal substance, whose parts adhering together, by a strong principle of attraction, are indivisible by less power than that of Omnipotence; I will suppose this substance to be placed in the brain. God has endowed it with an intelligent power; with thought, reason, and volition; and ideas, which exercise these faculties, are conveyed to it from external objects, through the medium of the senses;—how, or in what manner, I do not conjecture;—but it is as easy to suppose one something, we know not what, to think, as another something of which we are equally ignorant. This substance, which is the soul, is in different animals (both men and brutes) of a different degree of tenuity; and this difference of tenuity is the cause of that gradation of intellect which regulates the scale of animated beings. Ideas conveyed to souls differently tenuous, produce a stronger or a fainter impression in exact proportion to the degree of tenuity; as rays of light passing through bodies more or less dense, will differently illumine the object on which they fall. Is there any thing improbable or revolting in this theory? Certainly not. If I suppose the soul to be material, I do not assert that a certain organization of matter will produce thought; or that thought is produced by motion,

tion, and is the inevitable effect of a certain law. Mind can only be produced by the operation of a first cause, which cause is an all-powerful Deity: but it is surely as easy for him to impart certain powers and faculties to one substance as to another; and to make that substance indivisible, incorruptible, and immortal. From the universal use of 'blockhead,' 'thickhead,' and other expressions of a like meaning, it is evidently the implied opinion of mankind, that want of intellectual capacity is produced by a cause similar to that of a substance in a state of condensation. An exercise of the faculties has undoubtedly a tendency to render the sentient substance more tenuous, and more fitted for thought and observation; while illness, or any cause which tends to derange the nerves and fluids, may be supposed to condense it, and render it less sensible to the impression of ideas. In sleep, the bodily functions still continue; the nerves retain their irritability, the heart and arteries continue their pulsation. It is probable that sleep is produced more by a change in the sentient substance than in the body, though that change may be affected by physical causes of which we have no conception. If it will not appear too extravagant a conjecture, I would suppose that this insensibility to external objects is owing to the action of matter on the soul, which it renders too condensed to receive ideas: the sounder the sleep is, the more perfect the condensation: when sleep is imperfect, memory is at work, and presents to our minds a succession of broken and confused ideas, and these ideas will be clearer as sleep is more imperfect, for then the soul will be more tenuous.

When we speak of the Almighty as of a being purely spiritual, we mean that he is perfect in his essence; that is, incapable of imperfection of any kind. Pure, when applied to express an essence, means that such essence is perfect. A pure spirit, therefore, means a perfect spirit; and what is perfect must be incapable of imperfection; because, if any essence contains within itself a capability of imperfection, it is very clear that such essence is not perfect. It is then very evident that there can exist only one pure perfect spirit, or one Being purely spiritual, which is God; and that such terms are applicable only to the Deity; for no other being can be in his essence purely spiritual; for all other beings are imperfect; and an essence purely spiritual is necessarily perfect. Since, therefore, the attributes of mind can in no other being belong to a substance purely spiritual, it follows that our sentient principle (for it is to

earthly

earthly beings that I now confine myself) must have an affinity to a grosser and more imperfect substance. We have no idea of any other than what is called matter; but this is not to say that no other does exist. I have supposed the soul to be a subtile ethereal substance, because æther is the most subtile refined substance of which we have any idea:

> "Vast chain of being, which from God began;
> Natures ethereal, human;—angel,—Man."

"At the height of four thousand miles," says Bentley, "the æther is of that wonderful tenuity, that if a small sphere of common air of an inch diameter should be expanded to the thinness of that æther, it would more than take up the orb of Saturn, which is many million times bigger than the Earth." If this be true, it surely is not extravagant to suppose that God can endow matter still more subtile with the attributes of mind; and make such matter indivisible, incorruptible, and immortal. I can and do suppose, that what we call soul is formed of such matter; that all animals possess a soul differently tenuous; and that the superiority of man to the lower animals, and the disparity of intellect which exists between individuals of the human and brute species, are owing to this difference of tenuity, which may be increased or diminished by the neglect or exercise of the faculties, and the mysterious operation of physical causes.

LXXV. *Some Account of the different Theories of Arches or Vaults, and of Domes, and of the Authors who have written on this most delicate and important Application of Mathematical Science.* By a CORRESPONDENT.

[Concluded from p. 391.]

THE theory of domes and their tambour walls may be elicited in the same manner as that of arches and piers, with some exception. It is to be recollected that, in the arch, before any voussoir can fall, the lower voussoir must rise outwards or descend inward, so as to let the centre of pressure of the upper free, internally or externally: but in a dome, as not any voussoir can descend inward, by reason of its being one, in a system of voussoirs in equilibration, which is one ring of the dome; the dilapidation can only arise from the centre of pressure being freed externally: hence curves concave outwards are applicable to domes, and the dome does not require a crown, each successive ring forming a key-a key-stone: and if, as has been before considered in the case of the arch, that every dome and tambour wall may be determined by supposing some other dome whose curve is of greater altitude to be contained within its thickness; we may conclude the practical maxim, that the abutment of a dome should be half that of an arch (*cæteris paribus*) to be correct; because, in the work of dilapidation, only half the force is exerted.

It does not appear that the ancient architects were conducted by certain and geometrical principles in the construction of their edifices. Experience and imitation served them as guides. Vitruvius, who has collected in his treatise of architecture all the sciences necessary to those who then exercised the art, does not anywhere speak of the advantage to be derived from mechanics, to ascertain and decompose forces, and to collect their efforts in the point most able to resist them. Probably the ancient architects, exclusively employed in what regarded decoration, and the external and internal distribution in their edifices, abandoned to the workmen that part of the art which has for its object the means of construction; and their buildings in a great measure confirm this opinion.

We have not any written evidence, whereby we can determine whether the Gothic builders had studied the equilibration of arches; all their knowledge on this subject was kept a profound secret, and formed the principal mystery of the society of Free Masons: in the change of style in architecture which took place about the end of the 16th century, this knowledge gradually died away, with the members of the corporation, which then ceased to exist as a society of builders. Those who are acquainted with the science of equilibration of arches perceive, in the buildings of the middle ages, effects so conformable thereto, that it is difficult not to believe them possessed of this knowledge. It is manifest that they were acquainted with geometry in its most complicated department, and were familiar with the sections of the sphere, the cylinder and the cone, whether by plane or curved surfaces, and consequently the intricate art of cutting vault stones. Those who are acquainted with the necessary working drafts which must have been made for the construction of such buildings as they erected, will be ready to admit to them a knowledge far beyond what may be directly seen or recorded. From an investigation of their buildings, it has been supposed that they adopted, in the construction of their buildings, the use of the catenaria, practically derived from the chain, which has since become

become the source of the various theories which have been invented to determine the equilibration of the arch. While the corporation of the Free Masons as builders was expiring, Dr. Robert Hooke, under the mask of a cipher, gave what he calls "The true mathematical and mechanical form of all manner of arches for building, with the true butment necessary to each of them, a problem which no architectonic writer hath ever yet attempted, much less performed." The cipher deciphered is, "ut pendet continuum flexile sic stabit contiguum rigidum inversum." Dr. Hooke applies the principle of the catenaria not only to all manner of arches, but their abutments also. About this time the problem of the catenaria was investigated by James and John Bernoulli, Huyghens, Leibnitz, and Dr. David Gregory: the latter in his paper in the Philosophical Transactions deduces the relation of an arch and its abutment walls from the catenaria. He, like Hooke, claims the priority of the invention (see his answer to the animadversions on his paper on the catenaria). Dr. D. Gregory's deduction translated is: "None but the catenaria is the figure of a true and legitimate arch or fornix; and when an arch of another figure is supported, it is because in its thickness some catenaria is included; neither would it be sustained, if it were very thin and composed of lubricous parts." From a preceding corollary he says "it may be collected, by what force an arch or buttress presses a wall outwardly, to which it is applied. For this is the same with that part of the force sustaining the chain; which draws according to a horizontal direction. For the force which in a chain draws inwards, in an arch equal to the chain drives outwards."

James Bernoulli, after the manner of Gregory to whom he refers, gave two solutions of the problem "De curvatura fornicis, cujus partes se mutuo proprio pondere suffulciunt sine opere cæmenti." They were published in his posthumous works (see *Opera Jac. Ber. Gen.* 1744, page 1119). The application of the theory of the catenaria to the abutment, by these mathematicians, seems to favour the theory by which an arch, standing on a pier, should be considered an arch of greater altitude, or to contain in its thickness a catenaria.

M. De la Hire, in his *Traité de Mécanique*, 1695, published from the theory of the wedge, the proportion according to which the voussoir ought to be augmented, from the key-stone to the impost, in a semicircular vault. His theorem is this. Let ABC (fig. 1, Pl. XI.) be a semicircular arch composed of many equal voussoirs; and if from the

vertex

vertex B of the key-stone a line BO be drawn at right angles to BG, and the radii answering to the beds of the voussoirs PQR be prolonged until they intersect BO, all the voussoirs will be in equilibration, if their absolute weight be expressed by HK, KL, LM, &c. the distances between the points of intersection.

The historian of the Academy of Sciences at Paris reports, 1704, that M. Parent determined according to the same principles, by points, the form which the extrados of a semicircular vault and pier ought to assume. This solution has not been seen by the writer of this paper, nor does he know whether it has been printed.

In 1712, M. de la Hire inserted a paper in the *Mém. Acad. Par.* in which he considers the problem of the resistance of abutment piers upon the suppositions which have been before hinted at. He gives a mode of constructing an abutment pier, which is as follows. Let *pv* (fig. 2) be a quadrant of a circle, bisected in *l*, *fo* the extrados, and *fc* perpendicular to the horizon, *c* the centre of the circle, *sn* the base of the pier. Parallel to *fc* draw *lb*, cutting the base line in *b*. Through *l* draw *qd* perpendicular to *fc*, cutting *vc* in ε. Make *lx* and *lz* equal to the square root of the superficies of the part of the vault *lm fv* (which by prop. of M. Couplet $= \frac{fr + vt}{2} \times vf$; *cr* passing though *l*, and *fr* and *tv* being tangents to the intrados and extrados). Draw $z\varepsilon$ and its parallel *xa*, cutting *lb* in *a*. Draw $l\varepsilon$, and *cq* perpendicular to it, cutting *qd* in *q*. Make $ld = \frac{ly}{2} + \varepsilon q - sb$, and having made $lh = ly$, with the radius *hd* and centre *d*, describe the arc *hi*, cutting *lb* in *i*. Make $su = il - ly$, the width of the abutment pier required.

In the volumes 1726, 1727 and 1728, *Acad. Sci. Par.* M. Couplet published a paper on embankment walls, and was thence induced in 1729 and 1730 to publish two memoirs on the equilibration, principally of circular vaults, and the thickness of their abutment piers: in the first he considers the voussoirs, as in all former inquiries they had been considered, as perfectly lubricous: he investigates from the funicular polygon the vault of uniform thickness: he also considers the charge of a vault on a centre of wood, in reference to the paper of M. Pitot in vol. 1726. In the second memoir he takes into the consideration, cohesion and friction. M. Bouguer observes, that we may be sure, that if the voussoirs will maintain their places when perfectly slippery, they must when subject to cohesion and friction;

friction; and they are auxiliaries in any case not too great to be allowed. But Dr. Charles Hutton seems to controvert this opinion, or fact, of Bouguer; for in a letter (Monthly Magazine, August 1802, vol. xiv. p. 27) he says, "The old theory (viz. that of De la Hire, &c.) so violently contended for, can have no place in the practice of arch building, because that here the arch stones cannot act as true mathematical wedges. For in these it is well known that they are retained in their places or have their weight and other forces acting on their back balanced by two forces acting perpendicularly against their sides, which are conceived to be perfectly smooth or polished. But will it be said that this is the case with the voussoirs or wedges of a stone arch? Are their sides polished, or quite void of friction? &c."

In his first memoir M. Couplet gives a geometrical mode of determining the extrados of a circular vault, which may be inserted to show the identity of the theory with that of De la Hire and M. Parent.

Upon *vc* (fig. 3) as a diameter describe the arc *vd*, cutting the intradòs at *d*, and draw $d\varepsilon$ perpendicular to and cutting *vc* in ε. Draw *fk*, so that $hk = v\varepsilon$, which expresses the weight of the voussoir A. To find *lm* of the voussoir B, make $\varepsilon z = gf$, which expresses the weight of the vaussoir B. Upon *cz* as a diameter describe an arc, cutting *vy* a tangent to the vertex in *y*. Make $lc = yc$; hence *lm* required. If the back of the voussoir B be bisected, the extrados of an arch of voussoirs infinitely small, will pass through the point of bisection.

M. Couplet's theory of abutment piers is formed, like that of De la Hire, from the results of the wedge and the lever.

M. Belidor in his "*Science des Ingénieurs*," 1729, investigated the theory of abutment piers more fully than had been before done, also upon the principles of De la Hire. In the volume of the *Acad. Scien. Par.* 1734, M. Bouguer published a memoir upon the curved lines proper to form a dome: he shows that an infinity of curved lines are proper to form domes, and indicates at the same time the manner of choosing them; and lastly determines the form, which is a mechanical curve, of the last of all the curved lines which is proper for a dome: he has given a table, whereby this curve may be constructed. His mode of investigation is analogical to the mode of determining the equilibration of the arch of uniform thickness.

Mr.

Mr. Emerson, in his Fluxions, his Miscellanies, and in his Mechanics, published a theory of equilibration entirely different from all that had preceded it; he also published a paper on the subject in the Gentleman's Magazine, 28th vol. 1758, in which he recommends a new curve, of his invention, being the only curve which, he says, under the circumstances, can have a horizontal extrados, and be in equilibration, for the new bridge then about to be built at Blackfriars.

This theory is thus defined in his Mechanics: DB, &c. (fig. 4) is a semicircle, whose centre is R and vertex B; and the wall ATSB &c. must be so built, that the height AT in any place A must be as the cube of the secant of the arch BA. Hence DC will be an asymptote to STV.

In his Fluxions, he investigates the equilibration of the circle, parabola, ellipsis, hyperbola, catenary, cycloid logistic and cissoid curves, and afterwards, upon similar principles, investigates the equilibration of the circle and cubic and biquadratic parabolas in relation to domes. In the extrados of the hemisphere of equilibration, a vertical line passing through the vertex, as well as a tambour wall vertical from the springing, are asymptotical to the extrados.

If this theory be true, it appears wholly inapplicable to any useful purpose; for to obtain a form of extrados, to suit the occurrences of life, as he himself observes, and maintain the equilibration, the density of the materials must infinitely increase, to obtain the proportion of the cube of the secant of the arch BA.

About this time there were many papers published in the periodical works upon this curious subject; among others may be distinguished, A Letter from Mr. Thomas Simpson: from what may be gathered from his letter, he seems to have followed a theory something like that of De la Hire; for he says, the key-stone being five feet in depth, the voussoir at the haunch may be seven feet; but the De la Hire theory renders the haunch considerably thicker than this proportion: for instance, from Mr. Atwood's tables, the thickness of the key-stone being 1, that at the haunch will be 1,865, which would make it 9,325. It is to be lamented, that the calculations and demonstrations, which he promised to publish in the Philosophical Transactions, are not to be found in those volumes.

On the occasion of the erection of the dome of the church of St. Genevieve, M. Bossut, in 1770, investigated the question of the equilibration of arches and domes, and of the

the thrust of all kinds of domes: they were published in the volumes 1774 and 1776, *Acad. Scien. Par.* His theory is similar to that of De la Hire, though more extended.

M. Coulomb in 1773 published a memoir on some problems in architecture, in which he treats of the equilibration of arches, with a view to practice.

Dr. Charles Hutton, in a work entitled The Principles of Bridges, published, in the year 1772, a tract on the equilibration of arches and on their thrust on their abutment piers, in which he has followed the theory of Emerson. His theory of abutment piers has been taken up under the the same notions as that of De la Hire. The method and simplicity with which Dr. Hutton has treated these subjects cannot be too much praised, and forms a strong contrast with the writings of the French mathematician in this respect. His formulæ are short, and the calculations to be made from them by no means laborious: but as the theory seems rather to apply to the weight to bear upon an arch, than to the arch itself; and as it is admitted that an arc of a circle, ellipse or cycloid of equilibration, containing more than 120 degrees, cannot be constructed, much requires to be done before this theory can be applied to practice. Dr. Hutton has since promised a full elucidation of the subject. Professor Robison in the article *Arch*, in the Ency. Brit. Sup. gives the theory of Emerson, but, before he quits the subject, leaves a strong conviction on the mind of his reader, of its inutility in its present state. He gives a geometrical mode of determining the extrados of a circular arch according to this theory, which is as follows.

Draw the vertical CS (fig. 5) cutting the horizontal diameter in S, and ST cutting the radius OC perpendicularly in T. Draw the horizontal line Tz, cutting the vertical in z. Join zo. Make zu = Vv, and draw ux parallel to zo. Then Cc, the height above C required, will be equal to Cx.

Professor Robison also investigates the theory of the equilibration of domes, after the manner of Bouguer.

Professor Robison gives a popular mode of determining a curve to a given extrados, by suspending from the links of a chain, suspended, forming a curve of a given height and span, bits of chain, whose extremities should be adjusted to the given line: hence are to be deduced the arch itself, and the weight thereon. This method is more plausible than real: so much modification is necessary in the experiment, and the bits of chain are such inadequate representatives of the filling up of the vault, as well as the chain

chain is of the arch, that no correct conclusion can be obtained: besides, the data are the reverse of what are generally given.

In 1785, Mascheroni printed at Bergamo a work entitled "*Nuove Recherche sull' Equilibrio delle Volte,*" which relate principally to the equilibration of domes.

In 1802, in consequence of a project submitted to the House of Commons for building an iron arch over the Thames, of 600 feet span, many eminent mathematicians were consulted; and in their answers they were led to the investigation of the subject of arches. There were seventeen persons consulted; their answers are in the third Report of the House of Commons on the Improvement of the Port of London. Mr. Atwood was induced to consider the subject, and published two tracts on it. His results are the same as those of De la Hire, Parent, and Couplet. Among the papers in the Report alluded to there is one by Professor Robison, in which there is this element of equilibration, "The load on every part of a circular arch should be as the cube of a line drawn from the centre of the arch through that point, till it meet a horizontal line. Thus the weights on *acb* (fig. 6) should be as od^3, of^3, oe^3; *o* being the centre, and *de* horizontal.

In the Philosophical Magazine, vol. xi. Mr. Southern, of Birmingham, published a paper on the equilibration of arches, with a view to make Emerson's and Dr. Hutton's theory more easily understood. He was also one of the seventeen persons alluded to.

In 1802, Bossut, having reconsidered his papers published in 1774 and 1776, republished them with additions, incorporating them into one memoir, at the end of his *Traité de Mécanique.* He has treated separately of the equilibration of arches and domes.

Dr. Olinthus Gregory, in his Treatise of Mechanics, has extracted the most interesting propositions of the Emerson theory from the works of Emerson, Hutton, and the article *Arch*, in the Supplement to the Ency. Brit. by Professor Robison.

In Dr. Rees's New Cyclopædia the subject is investigated under the articles *Arch* and *Bridge*, in which the Emerson theory has been adopted, and the propositions &c. from Emerson, Hutton, and Robison have been inserted. There is also some account given of the De la Hire theory, in which, with deference to the writer, it appears that that theory has been wholly misunderstood, as well as the practice of arch building: for in the one case it is not necessary that

that the voussoirs "should have liberty to slide, to remain in equilibrio," but it is only necessary to obtain equilibration: nor is it necessary nor required that "the wedges must be cut to different oblique angles." In the other case, instead of architects contriving to have the butting sides of their wedges so rough, &c. on the contrary, the great desideratum is to have them as smooth as possible: and again, cement is never used in great arches, but as a substitute for bad workmanship in this respect. The Romans did not use cement in arcuation; they paid more attention to the polishing the joints of their stones than to the faces of them.

In 1809, Mr. Ware published a work on this subject, entitled A Treatise of the Properties of Arches and their Abutment Piers; in which he follows the theory of Emerson and Dr. Hutton, taking the load on every point of an arch, as the cube of the secant; but that load is considered (as in the inclined plane whence this theory is deduced) acting in the direction of the voussoir, or normally to the intrados, instead of to the horizon: hence, as in the De la Hire theory, the horizontal line at the springing is an asymptote to the extrados. The abutment piers are determined, upon the same principle, by the flowing of the infinitely thin voussoir which shall produce the least abutment, and is determined by the intersection of a catenaria (equicurved with the extrados) with the line of the base; the tangent to the catenaria at that point will cut, and be at right angles to, the infinitely thin voussoir required. This theory of the abutment is applicable to any other theory; for, if the flowing voussoir be in equilibration, by any theory, the abutment, it is manifest, must be in equilibration also. It is here to be observed, that this abutment sustains the whole thickness throughout of the arch: were it thought necessary to sustain merely the centre of gravities of each voussoir of the arch, the abutment would be half what this theory produces, but there would be produced what has been called a tottering equilibration. The same reasoning which would reduce the abutment, would with equal propriety advise an equal reduction at the vertex; in which proceeding there would be no end, until we come to the inverted catenaria itself, the arch of infinite thinness.

The geometrical construction is as follows:

Let AB, &c. (fig. 7) be a semicircular arch; y the height of the abutment; VB the thickness at the vertex, and o the centre. Make od equal VB, oV being vertical, and draw $d\varepsilon$ horizontal, and any radii oi, oi, cutting εd at ii, and the intrados at aa: then the weight on any point aa, will be as as oi^3, oi^3; the force which it exerts to fall being always constant. Prolong the radii, and make *ab*, *ab* equal *oi*, *oi*; through *bb* draw the extrados: (or, from any point *a* draw *ak* vertical and equal BV, and draw *kb* horizontal, cutting the radius through *a*, in *b*, which is the point required) *ba*, *ba*, are infinitely thin voussoirs. On any voussoir *ba* construct the parallelogram, cutting the base-line *mt* of the abutment in *m*; *mbbv* is the extrados of the arch and abutment. To find the voussoir *ab* to produce the least abutment: Draw *bc* horizontal, equal *id* or *bk*, and *cm* vertical, cutting *bm*, drawn perpendicular to *bo*, in *m*; through *v* and *m* draw the curve *vm*, cutting the base line in *m*; join *mo*, and on it as a diameter describe the semicircle *mbo*, touching the extrados in *b*, the extremity of the voussoir required: the curve *vm* is a catenaria. It is remarkable, that upon reference to Table I. in this work, the key-stone being 1, the voussoir at the haunch will be 1,414, which is a proportion conformable to what Mr. Simpson recommended for Blackfriars Bridge, and what was adopted in that structure. Mr. Gwilt has since published a work entitled, A Treatise on the Equilibrium of Arches, with a view to render the tracts on this subject by Emerson and Dr. Hutton useful and more easily understood. He considers them at present "of little if any use in practice:" and in the alteration of the dress, the subject is rendered more familiar; but nothing new has been exhibited; and indeed, what has been considered the great and important discovery in the Emerson theory, the curve for the horizontal extrados, has been omitted.

M. Berard, professor of mathematics at the college of Briançon, has lately published at Paris a work on the equilibration of arches, entitled *Statique des Voûtes*, consisting of five chapters, with an appendix on the *anse de panier*, or false ellipse, a curve much used by French architects. Mr. Mylne adopted it at Blackfriars Bridge, in its simplest construction. As this tract is of greater extent than any yet published, a summary of the contents may be acceptable. The first chapter is occupied in determining, from a given intrados, the extrados; and under certain circumstances to find the intrados. He deduces a simple mode of determining the extrados to a semicircular arch, which is as follows:

Let AB, &c. (fig. 8) be a semicircular arch; *c* the centre; V*c* vertical, and VB the thickness at the vertex: draw V*a* and B*b* horizontal. Through any point *d*, draw the radius C*a*, intersecting V*a* and B*b* in *a* and *b*. On *a*C as a diameter

meter describe the arc aV; and with the centre a and radius Bb, intersect it in h with the arc εf. With the centre C and radius Ch, intersect Ca in i with the arc ih, and i is a point in the extrados.

This gives precisely the same results as the methods of De la Hire, Parent, Couplet, Atwood, and Bossut.

A table is given in this chapter, to describe a catenaria.

The second chapter is occupied in an inquiry into the thrust of vaults, under the two hypotheses, where the piers are liable to be overturned, and when considered liable to slide upon their bases.

The third and fourth chapters are occupied in an investigation of the equilibration of domes, and their tambour walls: in the third is given a table for the construction of a dome, whose thickness is constant, analogical to the table of the catenaria, for arches of the same character in the first chapter.

The fifth chapter treats of irregular vaults, where the joints are not perpendicular to the intrados.

Mathematicians are always found more eager to continue and extend the theories of others, than to investigate the original primary proposition, and it is difficult to obtain any reasoning upon a theory, but what has been copied from the works of the original inventor: they are willing to admit a theory correct, because they find the mechanical detail so: to calculation they are accustomed; but the abstruse theoretical part requires an exertion of the mind, which being rarely necessary, so the mind is seldom fit for such exertion. Hence, when the theory becomes extended, the results are found contradictory, at variance with practice, and often with common sense. We find Bossut determining, with Soufflot, that the parabolic arch of equilibration should be thicker at the springing than at the vertex. But Berard finds that they were both wrong, and it ought to be thicker at the key than at the springing, and agrees with Emerson in this respect, though he differs widely in every other. Bossut and Berard again differ in determining the intrados, and in the case of the minimum of materials to an abutment; and they both differ from Epinus, who treated of this subject in the Berlin Memoirs, 1855. We find the asymptote of the Emerson theory vertical, when the tangent at the springing of the arch is so, and consequently the weight from the haunch to the springing, infinitely increasing, without any power in man to fulfil this supposed law of nature: but by the same theory, the hyperbolic and parabolic arches are found to increase from the

 springing

springing to the vertex; so that here the strength decreases as the weight increases.

When the theories are extended to domes, results of the most extraordinary character are produced: we find a vertical line passing through the vertex, an asymptote; the extrados being terminated at the vertex by a *flèche infinie;* but these phænomena were not discovered by Bouguer, Bossut, and Robison. Soufflot's knowledge of geometry is even questioned, because he did not terminate the dome of the Panthéon François *en flèche infinie.* It has been asserted, not very humbly, by the followers of the Emerson theory, that "the wedge theory of La Hire is now completely exploded," and that the theory which they follow is the "only true one." In the preceding pages it has been attempted to preserve a sceptic's indifference.

LXXVI. *On the Causes which influence the Direction of the Growth of Roots. By* T. A. Knight, *Esq. F.R.S. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.**

I have shown, in a former communication, the effects of centrifugal force upon germinating seeds; from which I have inferred that the radicles are made to descend towards the earth, and the germs, or elongated plumules, to take the opposite direction, by the influence of gravitation; and I believe the facts I have stated to be sufficient to support the inferences I have drawn †. But the fibrous roots of plants, being much less succulent, though not uninfluenced in the directions they take by gravitation, are, to a great extent, obedient to other laws, and are generally found to extend themselves most rapidly, and to the greatest length, in whatever direction the soil is most favourable: whence many naturalists have been disposed to believe that these are guided by some degrees of feeling and perception, analogous to those of animal life.

I shall proceed to state some of the facts upon which this hypothesis has been founded, and others which have occurred in the course of my own experience, and which are favourable to it; after which I shall endeavour to trace the effects observed to the operation of different causes.

When a tree which requires much moisture has sprung up, or been planted, in a dry soil, in the vicinity of water,

* From Philosophical Transactions for 1811, part ii.
† Phil. Trans. 1806, part i. page 5.

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it has been observed, that much the largest portion of its roots has been directed towards the water; and that when a tree of a different species, and which requires a dry soil, has been placed in a similar situation, it has appeared, in the direction given to its roots, to have avoided the water and moist soil.

A tree growing upon a wall, at some distance from the ground, and consequently ill supplied with food and water, has also been observed to adapt its habits to its situation, and to make very singular and well directed efforts to reach the soil beneath, by means of its roots*. During the period in which it is making such efforts, little addition is made to its branches, and almost the whole powers of the plant appear to be directed to the growth of one or more of its principal roots. To these much is in consequence annually added, and they proceed perpendicularly towards the earth, unless made to deviate by some opposing body: and as soon as the roots have attached themselves to the soil, the branches grow with vigour and rapidity, and the plant assumes the ordinary habits of its species.

Du Hamel caused two trenches to be made so as to intersect each other at right angles, and a tree to be planted at the point of intersection; and taking up this tree some years afterwards, he found that the roots had almost wholly confined themselves to the trenches, in which the soil of the former surface must have been buried.

A trench which was twenty feet long, six wide, and about two deep, was prepared in my garden, in the bottom of which trench was placed a layer, about six inches deep, of very rich mould, incorporated with much fresh vegetable matter. This was covered, eighteen inches deep, with light and poor loam, and upon the bed thus formed, seeds of the common carrot (*Daucus carota*) and parsnip (*Pastinaca sativa*) were sowed. The plants grew feebly till near the end of the summer, when they assumed a very luxuriant growth, grew rapidly till late in the autumn, and till their leaves were injured by frost. The roots were then examined, and were found of an extraordinary length, and in form almost perfectly cylindrical, having scarcely emitted any lateral fibrous roots into the poor soil, whilst the rich mould beneath was filled with them.

In another experiment of the same season, the preceding process was reversed, the rich soil being placed upon the surface, and the poor beneath. The plants here grew very

* Smith's Introduction to Botany.

luxuriantly, and acquired a considerable size early in the summer; and when the roots were taken up in the autumn, they were found to have assumed very different forms. The greater part had divided into two or more unequal ramifications, very near the surface of the ground, and those which were not thus divided tapered rapidly to a point at the surface of the poor soil, into which few of their fibrous roots had entered.

In other experiments, seeds of almost all the common esculent plants of a garden were so placed that the young plants had an opportunity of selecting either rich or poor soil; which was disposed, in almost every possible way, within their reach; and I always found abundant fibrous roots in the rich soil, and comparatively few in the poor.

The following experiment afforded the most remarkable result, and one the least favourable to the hypothesis which I have advanced in a former paper*, and to the conclusion which I shall now endeavour to support; and therefore I think it necessary to describe it very minutely. Some seeds of the common bean (*Vicia faba*), the plant with which many former experiments were made, were placed upon the surface of the mould in garden pots, in rows which were about four inches distant from each other. A grate, formed of slender bars of wood, was then adapted to the surface of each pot, so as to prevent both the mould and the seeds falling out, in whatever position the pots might be placed; and the bars were so disposed, as not at all to interfere with the radicles of the seeds, when protruding. The pots were then directly inverted; and the seeds were consequently placed beneath the mould; but each seed was so far depressed into the mould, as to be about half covered: by which means each radicle, when first emitted, was in contact with the mould above, and the air below. Water was then introduced through the bottom of the inverted pot, in sufficient quantity to keep the mould moderately moist; and the pots being suspended from the roof of a forcing house, the seeds soon vegetated.

In former experiments†, wherever the seeds were placed to vegetate at rest, the radicles descended perpendicularly downwards, in whatever direction they were first protruded; but under the preceding circumstances they extended horizontally along the surface of the mould, and in contact with it; and in a few days emitted many fibrous roots upwards into it: just as they would have done, if guided by

* Phil. Trans. 1806, page 1. † Ibid.

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the instinctive faculties and passions of animal life; and as I concluded before I made the experiment that they would do, under the guidance of much more simple laws, whose mode of operating I shall endeavour to explain.

Whatever be the machinery by which the sap of trees is raised to the extremities of their branches, it is obvious that this machinery is first put into action by the stems and branches, and not by the roots: for the graft or bud, whenever it has become fully united to the stock, wholly regulates the season and temperature in which the sap is to be put in motion, in perfect independence of the habits of the stock; whether those be late or early. If all the branches of a tree, exclusive of one, be much shaded by contiguous trees*, or other objects, the branch which is exposed to the light attracts to itself a large portion of the ascending sap, which it employs in the formation of leaves and vigorous annual shoots, whilst the shaded branches become languid and unhealthy. The motion of the ascending current of sap appears therefore to be regulated by the ability to employ it in the trunk and branches of the tree; and this current passes up through the alburnum, from which substance the buds and leaves spring. But the sap which gives existence to, and feeds the root, descends through the bark †: and if the operation of light give ability to the exposed branch to attract and employ the ascending or alburnous current of sap, it appears not improbable that the operation of proper food and moisture in the soil, upon the bark of the root, may give ability to that organ to attract and employ the descending or cortical current of sap; and if this be the case, an easy explanation of all the preceding phænomena immediately presents itself.

A tree growing upon a wall, and unconnected with the earth, will almost of necessity grow slowly; and as it must be scantily supplied with moisture during the summer, it will rarely produce any other leaves than those which the buds contained which were formed in the preceding year. Some of the roots of a tree thus circumstanced will be less well supplied with moisture than others, and these will be first affected by drought: their points will in consequence become rigid and inexpansible, and they will thence generally cease to elongate at an early period of the summer. The descending current of sap will be then employed in promoting the growth and elongation of those roots only which are more favourably situated, and those, compara-

* Phil. Trans. 1805 and 1809, p. 8. † Phil. Trans. part i p. 1.

tively with other parts of the tree, will grow rapidly. Gravitation will direct these roots perpendicularly downwards, and the tree will appear to have adopted the wisest and best plan of connecting itself with the ground: and it will really have employed the readiest means of doing so, as effectively as it could have done if it had possessed all the feelings and instinctive passions and powers of animal life. The subsequent vigorous growth of such a tree is the natural consequence of an improved and more extensive pasture.

When the seeds of the carrot and parsnip, in the experiments I have stated, were placed in a poor superficial soil, but which permitted the roots of the plants to pass readily through it, these were conducted downwards by gravitation; whilst the plants grew feebly, because they received but little nutriment. The roots were in a situation analogous to that of the stems of tree in a crowded forest; and when the leading fibres of the roots came into contact with the rich mould, they acquired a situation correspondent to that of the leading branches of such trees which are alone exposed to the light. The form of the roots of the plants was consequently long, slender, and cylindrical, like the stems of such trees. The roots of the one required the actual contact of proper soil and nutriment; and the branches of the other required the actual contact of light, to promote their growth.

When on the contrary the seeds of the preceding species of plants were placed in a rich superficial soil, their situation was analogous to that of a tree fully exposed, on every side, to the light, whose branches would be extended, in every direction, immediately above the surface of the ground: and as the fibrous roots of the plants came into contact with the subsoil, which was not well calculated to promote their growth, their situation became analogous to that of shaded branches; and they consequently ceased to extend downwards. The fibrous roots of a tree, under similar circumstances, would have extended along the lower surface of the favourable soil; but after these roots had much increased in bulk, they would be found partly compressed into the subsoil, however poor and unfavourable, provided it contained no ingredients actually noxious. In obedience to similar laws, the roots of an aquatic tree will not extend freely in dry soil, nor those of a tree which requires but little moisture in a wet soil; and on this account the roots of the one will appear to have sought, and those of the other to have avoided, the contiguous water; though both,

both, in the first period of their growth, pointed their roots alike in every direction.

When the seeds of the bean, in the experiment I have described, were placed to vegetate beneath the mould of an inverted pot, a sufficient quantity of moisture was afforded by the mould to occasion the protrusion of the radicles: but as soon as the under points of these had penetrated through the seed-coats, their surfaces were necessarily exposed to dry air, and were consequently rendered rigid and inexpansible; whilst their upper surfaces, being in contact with the moist mould, remained soft and expansible. If both the upper and lower surfaces of the radicles, at their points, had been equally well supplied with moisture, gravitation would have attracted the sap to the lower sides, where new matter would have been added; and the radicles would have extended perpendicularly downwards, as in former experiments: but the influence of gravitation was, to a great extent, counteracted by the effects of drought upon the lower sides of the radicles, nearly as it was counteracted by centrifugal force, when made to act horizontally*.

As soon as the radicles had acquired sufficient age and maturity, efforts were made by them to emit fibrous roots; when want of proper moisture on the lower sides prevented their being protruded in any other direction, except upwards. In that direction therefore they were alone emitted (as I was confident that they would before I began the experiment); and having found proper food and moisture in the pots, they extended themselves upwards through more than half the mould which these contained.

This experiment was repeated, and water was so constantly and abundantly given, that every part of the radicles was kept equally wet; and they then became perfectly obedient to gravitation, without being at all influenced by the mould above them.

In other experiments, pieces of alum and of the sulphates of iron and copper were placed at small distances perpendicularly beneath the radicles of germinating seeds, of different species, to afford an opportunity of observing whether any efforts would be made by them to avoid poisons; but they did not appear to be at all influenced, except by actual contact of the injurious substances. The growth of their fibrous lateral roots was, however, obviously accelerated, when their points approached any considerable quantity of decomposing vegetable or animal matter: and when

* Phil. Trans. 1806, p. 6.

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the growth of the roots was retarded by want of moisture, the contiguity of water, in the adjoining mould, though not apparently in actual contact with them, operated beneficially: but I had reason to suspect that the growth of roots was, under these circumstances, promoted by actual contact with the detached and fugitive particles of the decomposing body, and of the evaporating water.

The growth and forms assumed by the roots of trees, of every species, are to a great extent dependent upon the quantity of motion which their stems and branches receive from winds; for the effects of motion upon the growth of the root, and of the trunk and branches, which I have described in a former memoir, are perfectly similar*. Whatever part of a root is moved and bent by winds, or other causes, an increased deposition of alburnous matter upon that part soon takes place, and consequently the roots which immediately adjoin the trunk of an insulated tree, in an exposed situation, become strong and rigid; whilst they diminish rapidly in bulk, as they recede from the trunk, and descend into the ground. By this sudden diminution of the bulk of the roots, the passage of the descending sap, through their bark, is obstructed; and it in consequence generates, and passes into many lateral roots; and these, if the tree be still much agitated by winds, assume a similar form, and consequently divide into many others. A kind of net-work composed of thick and strong roots is thus formed, and the tree is secured from the dangers to which its situation would otherwise expose it.

In a sheltered valley, on the contrary, where a tree is surrounded and protected by others, and is rarely agitated by winds, the roots grow long and slender, like the stem and branches, and comparatively much less of the circulating fluid is expended in the deposition of alburnum beneath the ground; and hence it not unfrequently happens that a tree, in the most sheltered part of a valley, is uprooted; whilst the exposed and insulated tree, upon the adjoining mountain, remains uninjured by the fury of the storm.

In all the preceding arrangement, the wisdom of nature, and the admirable simplicity of the means it employs, are conspicuously displayed; but I am wholly unable to trace the existence of any thing like sensation or intellect in the plants: and I therefore venture to conclude, that their roots are influenced by the immediate operation and contact of surrounding bodies, and not by any degrees of sensation

* Phil. Trans. 1803, p. 7.

and

and passion analogous to those of animal life; and I reject the latter hypothesis, not only because it is founded upon assumptions which cannot be granted, but because it is insufficient to explain the preceding phænomena, unless seedling plants be admitted to possess more extensive intellectual powers than are given to the offspring of the most acute animal. A young wild-duck or partridge, when it first sees the insect upon which nature intends it to feed, instinctively pursues and catches it; but nature has given to the young bird an appropriate organization. The plant, on the contrary, if it could feel and perceive the objects of its wants, and will the possession of them, has still to contrive and form the organ by which these are to be approached. The writers who have contended for the existence of sensation in plants, appear to have been sensible of the preceding and other obstacles, and have all betrayed the weakness of their hypothesis, in adducing a few facts only which are favourable to it, and waving wholly the investigation of all others.

In the description of the preceding experiments, I fear that I have been tediously minute; but, as I have selected a few facts only from a great number which I could have adduced, I was anxious to give as accurate and distinct a view of those I stated, as possible.

I am, dear sir,
with great respect, sincerely yours,

Downton, Jan. 15, 1811. THO. AND. KNIGHT.

LXXVII. *Specimen of an Indian Calendar, extracted from the* "Elements of Botany," *by Professor* BARTON, *of America.*

THE following specimen of the calendar of one of the more cultivated of the Indian tribes of the great tract of country now within the limits of the United States, will not be deemed incurious or unimportant.

The Onondagos, one of the Six-Nations, whose chief residence ever since the arrival of the Europeans in this country has been in the State of New-York, divide the year into twelve months, and begin *their* year with December*.

The

* The Onondaga year does, certainly, consist of twelve months; and these months, I am pretty certain, are *lunar*. In the language of this tribe of the confederacy, a month is called *aeighneeta*, which is the name the Oneidas, who are close neighbours of the Onondagos, and who speak a near dialect of the same language, give to the *moon*.—I do not mean, however, to assert that

The names of the months are as follow: viz.

CHE-TÓ-RE.	December: "the cool month."
CHE-TÓ-RE-KO-NAH*.	January: "the cold month."
TIS-SAH.	February: "the snow is *beginning* to pass away."
TIS-SAH-KO-NAH.	March: "snow is now gone in reality; or is fast going."
KONE-LA-TOCH-AH.	April: "the bud-month;" or the trees begin to leaf.
KONE-LA-TOCH-KO-NAH.	May: "the leaf-month;" the leaves being fully out.
EE-SET-AH.	June: "the corn-month;" the maize, or Indian corn, being come up; and fit for the *first* dressing.
EE-RA-KO-NAH.	July: "the corn is up in full," and fit to receive its *second* dressing.
HUS-HESS-KO-HAH.	August: "the corn is making its heads;" or ears.
KOS-HESS-KO-HAH.	September: the month when "the ear is full," or fit for boiling.
KAN-TAH-HAH.	October: the month when "vegetation begins to fall away;" being affected by the frost.—It may be called the month of DEFOLIATION.

that the Indian months, which I have mentioned, exactly correspond to the months of *our* calendar.—On the subject of the manner of dividing time among the Indians (as well the rude as more cultivated) of North America, I have collected some important information, which I shall communicate to the public at a future period. At present, I shall only observe, that from the neat and simple calendar of the Onondagos, it is easy to perceive that they were more of an *agricultural* people than many of the other American tribes; than those, perhaps, who had a "Herring-month," a "Sturgeon-month," and a "Beaver-month," in their enumeration of the year. And the Six-Nations, of whom the Onondagos formed a part, and a noble part, were, it is known, much more attached to the cultivation of the earth, than the Delawares, and many other tribes.—From this calendar it is also natural to infer, that the Onondagos have, for a very considerable length of time,—for several centuries at least,—been settled nearly in the same tract of country, or upon the same parallel of latitude, in which they now reside. If, as I suppose, they came from the south-west, they must have altered, and accommodated, their calendar to the more northern regions of which they took possession.—The Mexicans, we are informed, made a considerable change in their calendar, when, migrating from the northern Atzlan, they seated themselves in the milder and more southern and more happy clime of Anahuac.

* The meaning of *konah* seems to be "in reality," or "in great earnest."

KAN-

KAN-TAH-HAH-KO-NAH. November: the month when the "vegetation is chiefly fallen down," having been killed by the frost.—Evergreens, of which the number of species in the country of the Six-Nations is very considerable,—such as Pinus, Larix, Abies, Thuja, Cupressus, not to mention the smaller, but yet conspicuous, Taxus, Rhododendron, Kalmia, and many others,—are, doubtless, intended to be excepted from the observation, which is exclusively restricted to *deciduous* vegetables, and chiefly those of an *arborescent* stature.

LXXVIII. *An Account of a vegetable Wax from Brazil. By* WILLIAM THOMAS BRANDE, *Esq. F.R.S.**

§ I. THE vegetable wax described in this paper was given to the president by lord Grenville, with a wish on the part of his lordship that its properties should be investigated, in the hope that it might prove an useful substitute for bees wax, and constitute, in due time, a new article of commerce between the Brazils and this country.

It was transmitted to lord Grenville from Rio de Janeiro, by the comte de Galveas, as a new article lately brought to that city from the northernmost parts of the Brazilian dominions, the capiteneas of Rio Grande and Seara, between the latitudes of three and seven degrees north: it is said to be the production of a tree of slow growth, called by the natives *Carnauba*, which also produces a gum used as food for men, and another substance employed for fattening poultry.

When the comte wrote to lord Grenville in July last, orders had been sent to the governors of the districts where it grows, requiring them to report more particularly on the nature and qualities of this interesting tree: we may therefore hope that information will soon be obtained, whether the article can be procured in abundance, and at a reasonable price; in which case it will become a valuable addition

* From the Philosophical Transactions for 1811, part ii.

to

to the comforts of mankind, by reducing the price and improving the quality of candles, flambeaux, &c.

The article, in the state in which it was sent, resembles much that described by Humboldt as the produce of the Ceroxylon Andicola*; but it is not likely to be the same, as Humboldt's wax is collected from a stately palm-tree, which grows on the high mountains from 900 to 1450 toises above the level of the sea, and on the edge of the regions of perpetual snow. On the other hand, the Brazilian plant is described as a slow-growing tree, but not as a large one, and there are no high mountains delineated in the most accurate and recent maps of the capiteneas where it is found. But a more decisive argument against their identity is the analysis of Vauquelin, published by Humboldt, which shows that the produce of the Ceroxylon consists of two-thirds resin and only one-third wax; but the Brazilian article is entirely wax, and affords not the smallest trace of resin. The Brazilian plant, however, was not entirely unknown to Humboldt; for it appears from his book, that M. Correa had informed him that a palm, called *Carnauba* by the natives of Brazil, produced wax from its leaves.

§ II.—1. The wax in its rough state is in the form of a coarse pale-gray powder, soft to the touch, and mixed with various impurities, consisting chiefly of fibres of the bark of the tree, which, when separated by a sieve, amount to about 40 per cent.

It has an agreeable odour, somewhat resembling new hay, but scarcely any taste.

At 206° Fahrenheit, it enters into perfect fusion, and in this state it may be further purified by passing it through fine linen. By this process it acquires a dirty-green colour, and its peculiar smell becomes more evident. When cold, it is moderately hard and brittle. Its specific gravity is ·980.

2. Water exerts no action on the wax, unless boiled with it for some hours; it then acquires a slight brown tinge, and the peculiar odour of the wax.

3. Alcohol does not dissolve any portion of the wax, unless heat be applied.

Two fluid ounces of boiling alcohol, spec. grav. ·826, dissolve about ten grains of the wax, of which eight grains are deposited as the solution cools, and the remaining two grains may be afterwards precipitated by the addition of water, or may be obtained unaltered by evaporating the alcohol.

The solution of the wax in alcohol has a slightly green tinge.

* *Plantes Equinoctiales*, p. 3.

4. Sulphuric

4. Sulphuric ether, spec. grav. ·7563, dissolves a very minute portion of the wax, at the temperature of 60°.

Two fluid ounces of boiling sulphuric ether dissolve thirty grains of the wax, of which twenty-six grains are deposited by cooling the solution, and the remaining four grains may be obtained by allowing the ether to evaporate spontaneously.

5. The fixed oils very readily dissolve the wax at the temperature of boiling water, and form with it compounds of an intermediate consistence, very analogous to those which are obtained with common bees wax.

In examining some combinations which I had made of the vegetable wax with olive oil, I was surprised to find them perfectly soluble in ether, and sparingly soluble in boiling alcohol.

As it is commonly stated that the fixed oils are insoluble in ether and in alcohol, I was led to attribute the solution of the oil, in these instances, to its being combined with the wax; but subsequent experiments, of which I shall state the general results, have shown me that these opinions are erroneous.

Four fluid ounces of sulphuric ether, spec. grav. ·7563, dissolve a fluid ounce and a quarter of the expressed oil of almonds; of olive oil, the same quantity of the ether dissolves a fluid ounce and a half; of linseed oil, two fluid ounces and a half; and castor oil is soluble in any proportion in sulphuric ether of the above specific gravity.

The expressed oils of almonds and of olives are very sparingly soluble in alcohol, spec. grav. ·820.

Linseed oil is more soluble than the two former. Four fluid ounces of alcohol, spec. grav. ·820, dissolve nearly one fluid drachm.

Castor oil is perfectly soluble in every proportion in alcohol, spec. grav. ·820. In alcohol of a higher specific gravity, as ·840, it is very sparingly soluble *.

As some of the difficultly soluble resins are more easily dissolved in alcohol to which a small proportion of camphor has been added, I endeavoured to ascertain whether the fixed oils were rendered more soluble by the same means, but found that this was not the case, excepting with regard to castor oil, which, although very sparingly dissolved by alcohol of a spec. grav. above ·840°, becomes abundantly

* The solubility of castor oil in alcohol was mentioned to me some months ago by Dr Wollaston, who also informed me, that it had on that account been employed to adulterate certain essential oils of high value, especially the oil of cloves.

soluble

soluble by the addition of one part of camphor to eight parts of the alcohol.

Boiling alcohol, spec. grav. ·840, takes up a considerable portion of castor oil and of linseed oil; it also dissolves a small quantity of the oils of almonds and of olives; but they are copiously deposited during the cooling of the alcohol, and only a small portion retained in permanent solution.

When water is added to any of these solutions of the fixed oils in ether, and in alcohol, a milky mixture is formed, and the oil gradually separates upon the surface, without having undergone any apparent alteration.

6. One hundred grains of the wax were boiled for half an hour in a solution of caustic potash, spec. grav. 1090. The solution acquired a pale-rose colour, but appeared to exert no further action on the wax, which after having been washed with warm water retained its fusibility and other properties. No combination therefore, similar to a soap, was produced, nor was any precipitate occasioned by the addition of acids to the rose-coloured alkaline solution.

7. The effects produced by boiling the wax in solutions of pure soda, and of the subcarbonates of soda and of potash, were analogous to those of the caustic potash.

8. Solutions of pure and of carbonated ammonia exert scarcely any action on the wax.

9. When the wax is boiled in nitric acid, spec. grav. 1·45, there is some escape of nitrous gas, and the colour of the wax is gradually changed to a deep yellow.

When the wax is removed from the acid, and washed with hot water, it is found to have become more brittle and hard, but it still retains much of its peculiar odour.

In this state it remains insoluble in the alkalies; but they now change its colour to a very bright brown, which is destroyed by washing with dilute muriatic acid, and its original yellow colour restored.

Neither the fusibility nor the inflammability of the wax is impaired by this process.

Nitric acid, diluted with eight parts of water, produces the same change in the colour of the wax as the concentrated acid.

Having been unsuccessful in my attempts to bleach the wax in its original state, I made some experiments to ascertain whether its colour could be more easily destroyed, after it had been acted upon by nitric acid, and found that, by exposing it spread upon glass to the action of light, it became in the course of three weeks of a pale-straw colour, and

and on the surface nearly white. The same change was produced by steeping the wax in thin plates in an aqueous solution of oxymuriatic gas, but I have not hitherto succeeded in rendering it perfectly white.

10. Muriatic acid has little action on the wax: when boiled upon it for some hours, it destroys much of its colour.

11. Sulphuric acid changes the colour of the wax to a pale brown, and when water is added, it becomes of a deep rose colour; the inflammability and the fusibility of the wax are slightly impaired by this process.

When heat is applied, the wax is decomposed with the usual phænomena, sulphurous acid is developed, and charcoal deposited.

12. Acetic acid has very little action on the wax, when cold.

When the wax is boiled in this acid, a minute portion is dissolved, and again deposited as the solution cools. By long continued boiling in acetic acid, the wax is rendered nearly white; but when it is afterwards washed with water, and fused, it resumes its former colour.

13. When the wax is fused in oxymuriatic gas, it is rapidly decomposed, and parting with hydrogen and oxygen, muriatic acid and water are formed, and charcoal is deposited.

14. The results of the destructive distillation of the vegetable wax are very analogous to those of bees wax.

An acid liquor mixed with a volatile oil are the first products; these are succeeded by a large proportion of a butyraceous oil, and a very small quantity of charcoal affording traces of lime remains in the retort. During the process, a little carburetted hydrogen gas is given off.

I have not considered it necessary to dwell upon the relative proportions of these different products, as they will necessarily vary according to the rapidity with which the distillation is conducted.

§ III.—From the preceding detail of experiments it appears, that although the South American vegetable wax possesses the characteristic properties of bees wax, it differs from that substance in many of its chemical habitudes; it also differs from the other varieties of wax, namely, the wax of the Myrica cerifera*, of lac †, and of white lac ‡.

* Vide Dr. Bostock's Experiments on the Wax of the Myrica cerifera, in Nicholson's Journal for March 1803.

† Vide Analytical Experiments and Observations on Lac, by Charles Hatchett, Esq. F.R.S. in the Philosophical Transactions for 1804.

‡ Vide Observations and Experiments on a Wax-like Substance from Madras, by George Pearson, M.D. F.R.S in the Phil. Trans. for 1794.

The attempts which I have made to bleach the wax have been conducted on a small scale; but from the experiments related it appears that, after the colour has been changed by the action of very dilute nitric acid, it may be rendered nearly white by the usual means. I have not had sufficient time to ascertain whether the wax can be more effectually bleached by long continued exposure, nor have I had an opportunity of submitting it to the processes employed by the bleachers of bees wax.

Perhaps the most important part of the present inquiry is that which relates to the combustion of the vegetable wax in the form of candles.

The trials which have been made to ascertain its fitness for this purpose are extremely satisfactory; and when the wick is properly proportioned to the size of the candle, the combustion is as perfect and uniform as that of common bees wax.

The addition of from one-eighth to one-tenth part of tallow is sufficient to obviate the brittleness of the wax in its pure state, without giving it any unpleasant smell, or materially impairing the brilliancy of its flame. A mixture of three parts of the vegetable wax with one part of bees wax also makes very excellent candles.

LXXIX. *Theorems for calculating the Temperaments of such regular Douzeaves as are commensurable, or defined by a certain Number of equal Parts, into which the Octave is divided.* By JOHN FAREY, *Sen.*

To Mr. Tilloch.

SIR, HAVING been applied to by a musical friend, to point out the method of calculating the Temperament of the Fifths, in any given commensurable system of Musical Intervals, without reference to the Memoirs of the Academy of Sciences (a work which many have not the opportunity of consulting) as is done in the 1st, and several others of the Scholia to my *Six Musical Theorems* in your 36th volume, p.45; I beg the favour of you to give a place to the following Observations and Theorems on Commensurable Systems.

It is well known, that merely giving the number of *equal parts* into which the octave is to be divided, is not sufficient to define, in all cases, the regular douzeave that results, or system wherein all the Fifths to a given extent are alike tempered; the common property of which is, as Dr. Smith

Smith has shown, in his truly excellent "Harmonics," Prop. III. and XVIII, that the sum of 5 of the mean Tones (T) and two of the major Limmas (L) are equal to the Octave, in all such systems. Thus in the system of M. Henfling (Schol. X. p. 50) wherein the Octave is divided into 50 equal parts, six different sets of T and L may be found to answer the above condition, viz.

$$\begin{array}{cccccccc} & & T & & & L & & \\ 5 & \times & 10 & + & 2 \times & 0 & = & 50 \\ 5 & \times & 8 & + & 2 \times & 5 & = & 50 \\ 5 & \times & 6 & + & 2 \times & 10 & = & 50 \\ 5 & \times & 4 & + & 2 \times & 15 & = & 50 \\ 5 & \times & 2 & + & 2 \times & 20 & = & 50 \\ 5 & \times & 0 & + & 2 \times & 25 & = & 50 \end{array}$$

Which sets of answers may, in general, be obtained by this rule, viz.

From the given number of equal parts in the Octave (a), deduct successively the even numbers in the series 0, 2, 4, 6, 8, &c. until a remainder is found, divisible by 5, or which ends with 0 or 5, and let such *even* subtrahend be called b: then will $\frac{a-b}{5}$ be the greatest value of T, and $\frac{b}{2}$ the least or corresponding value of L; and all the other corresponding values of T will decrease from this by 2, and those of L increase by 5, in succession, as in the example above. It will however be unnecessary, to carry this process on any further, than till L becomes equal to T; since in all practical systems, the value of L cannot differ very greatly from the half of T, and thus the second line in the above example, is the only practical system that results from a division of the Octave into 50 equal parts; and so of any other value of a.

In practice therefore, the value of b (or 2L) will be restricted to some of those *even* subtrahends that produce practical systems, and which may be determined, in my new notation, by

Theorem 7. $-r = \frac{b}{a} \times 61{\cdot}421264\Sigma - 9{\cdot}23622212\Sigma$, the flat temperament of the Fifths, in the system having a equal parts in the Octave.

Or, in reciprocals of common logarithms, by

Theor. 8. $-r = \frac{b}{a} \times {\cdot}0301029{,}99566 - {\cdot}0045267{,}3834$.

For example, in Mercator's System, mentioned in Schol. 1, $a = 53$ and $b = 8$, and we have $8 \times 61{\cdot}421264 \div 53 - 9{\cdot}236222 = -{\cdot}03491121\Sigma$ the flat Temperament of the

 Fifth:

Fifth: and in the other Scholia, we have for the Commensurable Systems mentioned, as follows, viz.

Schol. 2	$a = 112$	$b = 8$	$-r = -2{\cdot}828669\Sigma$
3	19	4	$-3{\cdot}694570$
5	43	8	$-2{\cdot}190990$
6	12	2	$-1{\cdot}000655$
8	67	12	$-1{\cdot}764601$
9	74	14	$-2{\cdot}384017$
10	50	10	$-3{\cdot}048031$
12	31	6	$-2{\cdot}651765$
13	55	10	$-1{\cdot}931281$

Which results, it will be observed, are much more exact than those which I before gave, owing to too few places of decimals having been used, as my able friend, the Rev. C. J. Smyth, long ago pointed out to me, after recalculating and enlarging M. Sauveur's Table of Commensurable Systems, and which it were much to be wished that he would publish.

The Temperament of the Fifth or $-r$ being thus obtained, for any Commensurable System, we have only to consider s as $= 1$, $-t = 0$, and $u = 1$, and all the other Temperaments and the Wolves of any such system will readily be obtained from the Theorems at page 41 of the 36th volume, or from the Corollaries that follow at page 371.

If we select the 6th Scholium as an example of the use of Theorem 8, we have $\frac{b}{a} = \frac{2}{12} = \frac{1}{6}$, and ·0301029,9957 $\div 6 -$ ·0045267,3834 = ·0004904,2825 $= -r$, the reciprocal logarithm of the flat temperament of the *Isotonic* System: from which if we take ·0004901,0713 the reciprocal logarithm of Σ, we have 3·2112 (or $\frac{1}{12}$ of 38,5342 the reciprocal logarithm of m), or $\Sigma + \frac{1}{2}m$, as the proper flattening of the Equal Temperament.

Allow me here to correct two errata in the 5th Scholium, p. 46, of volume xxxvi. bottom line and line 6 from the bottom, for *flat* read *sharp*.

I am, sir,

Your obedient servant,

JOHN FAREY, Sen.

Upper Crown Street,
Dec. 4, 1811.

LXXX. *The Originality of Daniel's Life-Preserver disputed.*

To Mr. Tilloch.

SIR, THE advantages that have accrued to the public, from the institution of the Society for the Encouragement of Arts and

and Manufactures, are generally acknowledged: and that their exertions have been gratefully recognised and appreciated, is sufficiently obvious from the constant annual increase of subscribers, since its commencement in 1754. The standing rules and regulations of the Society have had two principal objects in view, equally necessary to the permanent reputation of every scientific institution. Their laws are calculated to exclude no class of the community from the stimulus they offer to talent; all are admitted to fair and equal competition; to the wealthy, honorary distinctions are offered as the reward of patriotism or ingenuity, while at the same time the skill of the indigent mechanic is excited to industrious activity, by the certainty of meeting with pecuniary recompense, proportioned to the utility of his invention. If, then, care has been taken to found the instutution on the broad basis of generally utility, they have endeavoured to draw the line between liberality becoming a public body, and the undue appropriation of the funds to objects unworthy their patronage. The peculiar advantages which a public society possesses over a private body of associated individuals is, that in conferring an honour, or in bestowing a pecuniary reward, all idea of personal obligation is done away. It admits of a more unconstrained examination of the relative merits of the candidates; all is submitted to general discussion; and while it precludes any undue preference to a favoured individual, the members are at liberty to reject or approve, without fear of violating personal feelings. From an earnest wish then for the reputation of a society at once liberal and politic, it was not without considerable pain that I saw recorded in their Transactions a circumstance, which, if frequently repeated, would not fail to deprive their premiums of that value which they ought to possess. The approbation of the society has hitherto been considered a very important acquisition, which cannot be better exemplified than by the anxiety shown by Mr. F. C. Daniel, of Wapping, to wrest from the society that approbation which to me appears unmerited. I presume that in voting to Mr. Daniel, in the year 1807, the gold medal of the society for his "apparatus to secure persons from sinking in water, or, to act as a life-preserver when shipwrecked;" it was under the impression that it was as new as important, as original as ingenious. It is unnecessary to refer the reader to Vegetius, or to Folard, to prove that the idea had neither originality nor novelty to recommend it, while there is a book extant in our own language published near two centuries and a half ago, containing,

ing, with the exception of the straps, the identical Daniel's life-preserver now under consideration. In referring the reader to the plate*, we shall merely add the following extract from the book in question, entitled 'Certaine waies for the orderyng of Souldiers in Battel-ray, &c. &c. gather'd and set foorthe by Peter Whitehorne, Student of Graies Inne, London, 1562.

'How to make a Girdell for Souldiers or Fishers, whereby they may goe in the water and passe over a river withoute eyther Bridge or Bote.

'This girdel ought to be made accordinge to the fation of the figure nexte following, and of such lether that must be dressed in lyke sorte as the same is wherwith foote-bals are made, wherunto a pype must be fasten'd lyke unto a beggepype, so that the girdell when it is girte about a souldier upon his armur may be blowen full of wynde, by helpe whereof he may then safely passe over a river goinge through the same how depe soever it be, wher he shall not sinke in the water forther than from the girdell stede dounewards, which for men of warre is very commodious, and a moste necessary thing.'

After perusing the above passage, copied verbatim from the original black letter, I deem it unnecessary to offer any apology to the readers of your very interesting Magazine, for questioning the justice of this gentleman's claims on the society as the inventor of the life-preserver. In admitting, then, that Mr. Daniel has conferred an important obligation on mankind, in having rescued from unmerited obscurity a most valuable machine for the preservation of his fellow-creatures, I think he is placed precisely at that point in the scale of merit to which his services entitle him.

A Member of the Society for the Encouragement of Arts, Manufactures, &c.

London, Feb. 18, 1811.

LXXXI. *Remarkable Disease of the Lungs and Kidneys. By* John Taunton, *Esq. Surgeon to the City and Finsbury Dispensaries, and to the City Truss Society, Lecturer on Anatomy, Surgery, Physiology, &c.*

Daniel Lewis, aged 49, was taken ill in June last with great pain in the right lumbar region, which was succeeded by a discharge of blood and pus with the urine. These sym-

* Fig. 4. Plate X.

ptoms

ptoms continued with little variation till about the middle of September. A strengthening plaster was then applied to the part, and opium given by the direction of Dr. Hancock (the man having obtained a letter for the Finsbury Dispensary). These succeeded in removing the pain; but in about a week after the plaster was applied, a small swelling appeared in the loins over the right kidney. The urine was now more healthy. It was concluded that an abscess was forming in the kidney.

The swelling continued to enlarge and become more painful. His appetite and strength being impaired, cordial medicines were given; but the only relief which he experienced was by increasing the dose of opium.

At the beginning of November his debility had increased: the pulse was weak, the tongue was foul, the respiration was difficult, with a bad cough; he did not pass the fæces without an aperient medicine, or a glyster. The tumour had now become nearly as large as a cocoa nut, but the integuments were not inflamed. Cordial medicines combined with opium were given.

Nov. 2d. The tumour burst by a small opening, from which about a pint of rather thin matter, of a whitish colour, with a sky-blue tint, was discharged; he made water freely, and felt much easier: the skin was hot and dry: the stools were not passed without glysters.

Nov. 9th. The discharge has been considerable, his health is worse, the pulse weaker, and cough has been very troublesome: fomentations and poultices have been applied to the part: the anodyne medicines have been continued, but the only temporary relief which has been obtained has been by increasing the dose of opium.

Nov. 14th. Much worse: the bad symptoms have all increased; the respiration is hurried and laborious; the pulse small, thready, but not quick; the countenance appears dejected; the voice is indistinct; the abscess remains much the same.

21st. The pulse intermits; the stools are frequent, thin and watery: he rests more easily. Give the misturæ cretæ cum tinctura kino.

Dec. 6th. He is weaker: the only rest obtained is from opium. He died on the evening of the 11th, without any material change in the symptoms.

13th. The body was examined in the presence of Dr. Hancock, Mr. Priest, and Mr. Burn. The *left lung was entirely destroyed* by abscesses: the right lung adhered to the

 pleura

pleura costalis in many parts, and contained a great number of tubercles: the pericardium and heart were natural.

The liver was rather pale and mottled on its surface, but its internal structure was natural. The gall-bladder was partially filled with bile: the bile-ducts and blood-vessels of the liver were healthy: the spleen, pancreas, omentum, stomach, small and large intestines, were in a healthy state. The descending colon was contracted, and the rectum much dilated, so as to occupy almost the whole cavity of the pelvis; this dilated intestine was filled with fæces of a soft adhesive nature, almost of the consistence of bird-lime. The internal coat of the bladder was thickened, irregular, ulcerated, and much inflamed, with two calculi lodged in its coats; yet there had not been any symptoms of disease in this organ for several months past. The canal in each vas deferens was *completely obliterated*, that on the right side by a calculus; and the *vesiculæ seminales were unusually small;* the prostate gland was also *very small.* The left kidney was perfectly healthy; the right kidney was rather large, having a whitish surface: the emulgent vessels were obliterated at their entrance into the kidney in an inflamed mass: the coats of the ureter near to the kidney were thickened, and its canal filled with pus, but nearer to the bladder it was obstructed: the posterior surface of the kidney formed the boundary on that side to the sac of the abscess, which had burst in the loins: this communicated with a small abscess in the pelves of the kidney, in which some calcareous substance was formed. The whole cortical part of the gland was formed into separate abscesses, which had not burst externally, nor did they appear to communicate with each other.

If the adhesive inflammation had not taken place in the emulgent vessels, the pus must have been conveyed by them into the circulation, and have destroyed life much sooner.

In this patient, respiration was carried on by the right lung only, which was in a diseased state, and the urine was secreted entirely by the left kidney. The preparations are preserved in my collection, and may be seen by any person who is desirous of making further inquiries into this interesting case.

On making application to my friend and colleague Dr. Hancock, under whose care this man had been placed before I saw him, I was favoured with the following particulars.

Daniel Lewis had been free from any remarkable disease for

for the last two years, but early in life he had debilitated his constitution by excess in venery, so that, from the age of thirty to the time of his decease, *nunquam erat emissio seminis in coitu*, though his animal propensities were strong. He was employed in a brewery, and had frequent access to malt liquor. About two years ago he was confined some weeks with pain in the region of the right kidney, and had a discharge of a whitish, thick, and sometimes bloody urine, which indicated that suppuration had taken place either in the kidney or coats of the bladder. At intervals from that period the urine assumed this morbid appearance. Is it not highly probable, that when the communication between the kidney and the bladder was cut off by the inflammation and obliteration of the canal of the ureter, the formation of the tumour commenced in the loins to form an outlet for the matter from the kidney? Is it not also probable that the ulceration in the bladder was subsequent to the suppuration in the kidney? and that the ureter, which was diseased in its whole course, was the medium of morbid communication?

I was not aware of any impediment having existed in the discharge of the seminal fluid, till some days after the examination, or I should have paid particular attention to the state of the testes and vasa deferentia through their whole course; the latter tubes were completely obstructed for two inches before their termination. The vesiculæ seminales were *very small*, hardly so large as the termination of the vasa deferentia. There was not any fluid contained: indeed the cells of the vesiculæ appeared to be nearly obliterated.

JOHN TAUNTON.

21, Greville-street, Hatton-garden, Dec. 17, 1811.

LXXXII. *Experiments to ascertain the State in which Spirit exists in fermented Liquors: with a Table exhibiting the relative Proportion of pure Alcohol contained in several Kinds of Wine and some other Liquors.* By WILLIAM THOMAS BRANDE, *Esq. F.R.S.**

Section I.

It has been a commonly received opinion, that the alcohol obtained by the distillation of wine does not exist ready formed in the liquor, but that it is principally a product of the operation arising out of a new arrangement of its ultimate elements.

* From the Philosophical Transactions for 1811, part ii.

The

The proofs which have been brought forward in support of this theory, are chiefly founded on the researches of Fabroni*, who attempted to separate alcohol by saturating the wine with dry subcarbonate of potash, but did not succeed, although by the same means he could detect very minute portions of alcohol which had been purposely added.

To obtain satisfactory results from many of the following experiments, it became necessary to employ wines to which little or no spirit had been added; for a very considerable addition of brandy is made to most of the common wines, even before they are imported into this country. I therefore occasionally used Burgundy, Hermitage, Cote Roti, Champagne, Frontignac, and some other French wines, to which, when of the best quality, no spirit can be added, as even the smallest proportion impairs the delicacy of their flavour, and is consequently readily detected by those who are accustomed to taste them. For these, and for the opportunity of examining many of the scarce wines enumerated in the table annexed to this paper, I am indebted to the liberality of the Right Hon. Sir Joseph Banks.

Dr. Baillie, who took considerable interest in this investigation, was also kind enough to procure for me some Port wine, sent from Portugal for the express purpose of ascertaining how long it would remain sound, without any addition whatever of spirit having been made to it.

Lastly, I employed raisin wine which had been fermented without the addition of spirit.

At a very early period of the present inquiry, I ascertained by the following experiments, that the separation of the alcohol by means of subcarbonate of potash was interfered with, and often wholly prevented, by some of the other ingredients of the wine.

A pint of Port wine was put into a retort placed in a sand heat, and eight fluid ounces were distilled over, which by saturation with dry subcarbonate of potash afforded about three fluid ounces of tolerably pure spirit floating on the surface.

I repeated this distillation precisely under the same circumstances, and mixed the distilled liquor with the residuum in the retort, conceiving that, if the spirit were a product, I now should have no difficulty in separating it from the wine by the addition of subcarbonate of potash: but although every precaution was taken, no spirit separated: a portion of the subcarbonate, in combination with some

* *Annales de Chimie*, tome xxxi. p. 303.

of

of the ingredients of the wine, formed a gelatinous compound, and thus prevented the appearance of the alcohol.

It has been remarked by Fabroni, in the memoir above quoted, that one hundredth part of alcohol purposely added to wine may be separated by subcarbonate of potash, but several repetitions of the experiment have not enabled me to verify this result: when however a considerable addition of alcohol has been made to the wine, a part of it may be again obtained by saturation with the subcarbonate. The necessary addition of spirit to Port wine, for this purpose, will be seen by the following experiments.

Four ounces of dry and warm subcarbonate of potash were added to eight fluid ounces of Port wine, which was previously ascertained to afford by distillization 20 per cent. of alcohol (by measure), of the specific gravity of 0·825 at 60°.

In twenty-four hours the mixture had separated into two distinct portions; at the bottom of the vessel was a strong solution of the subcarbonate, upon which floated a gelatinous substance, of such consistency as to prevent the escape of the liquor beneath when the vessel was inverted, and which appeared to contain the alcohol of the wine, with the principal part of the extract, tan, and colouring matter, some of the subcarbonate, and a portion of water: but as these experiments relate chiefly to the spirit contained in wine, the other ingredients were not minutely examined.

To seven fluid ounces of the same wine I added one fluid ounce of alcohol (specific gravity 0·825), and the same quantity of the subcarbonate of potash as in the last experiment: but after twenty-four hours had elapsed, no distinct separation of alcohol had taken place.

When two fluid ounces of alcohol were added to six fluid ounces of the wine, and the mixture allowed to remain undisturbed for the same length of time as in the former experiments, a stratum of impure alcohol, of about a quarter of an inch in thickness, separated on the surface.

The addition of three fluid ounces of the alcohol to five fluid ounces of the wine, formed a mixture from which a quantity of spirit readily separated on the surface, when the subcarbonate was added, and the gelatinous compound sunk nearly to the bottom of the vessel, there being below it a strong solution of the subcarbonate.

When in these experiments Madeira and Sherry were employed instead of Port wine, the results were nearly similar.

It was suggested to me by Dr. Wollaston, that if the

wine

wine were previously deprived of its acid, the subsequent separation of the alcohol, by means of potash, might be less interfered with. I therefore added to eight fluid ounces of Port wine a sufficient quantity of carbonate of lime to saturate the acid, and separated the insoluble compounds produced, by means of a filter. The addition of potash rendered the filtered liquor turbid, some soluble salt of lime, probably the malate, having passed through the paper; but the separation of alcohol was as indistinct as in the experiments just related.

It is commonly stated, that the addition of lime water to wine, not only forms insoluble compounds with the acids, but also with the colouring matter, and that these ingredients may be thus separated without heat; but on repeating these experiments, they did not succeed, nor could I devise any mode of perfectly separating the acids, and the extractive and colouring matter (excepting by distillation), which did not interfere with the alcohol.

If the spirit afforded by the distillation of wine were a *product* and not an *educt*, I conceived that by performing the distillation at different temperatures, different proportions of spirit should be obtained.

The following are the experiments made to ascertain this point.

Four ounces of dried muriate of lime were dissolved in eight fluid ounces of the Port wine employed in the former experiments: by this addition, the boiling point of the wine, which was 190° Fahrenheit, was raised to 200°. The solution was put into a retort placed in a sand heat, and was kept boiling until four fluid ounces had passed over into the receiver, the specific gravity of which was 0·96316 at 60° Fahrenheit *.

The experiment was repeated with eight fluid ounces of the wine without any addition, and the same quantity was distilled over, as in the last experiment: its specific gravity at 60° Fahrenheit was 0·96311.

Eight fluid ounces of the wine were distilled in a water bath; when four fluid ounces had passed over, the heat was withdrawn. The specific gravity of the liquor in the receiver was 0·96320 at 60° Fahrenheit.

The same quantity of the wine as in the last experiment was distilled at a temperature not exceeding 180° Fahrenheit.

* It was supposed that in this experiment a small portion of muriate of lime might have passed over into the receiver; but the distilled liquor did not afford the slightest traces of it, to the tests of oxalate of ammonia and nitrate of silver.

This

This temperature was kept up from four to five hours, for five successive days, at the end of which period, four ounces having passed into the receiver, its specific gravity at 60° was ascertained to be 0·96314.

It may be concluded, from these results, that the proportion of alcohol is not influenced by the temperature at which wine is distilled, the variation of the specific gravities in the above experiments being even less than might have been expected, when the delicacy of the operation by which they are ascertained is considered.

I have repeatedly endeavoured to separate the spirit from wine, by subjecting it to low temperatures, with a view to freeze the aqueous part; but when the temperature is sufficiently reduced, the whole of the wine forms a spongy cake of ice.

In a mixture of one fluid ounce of alcohol with three of water, I dissolved the residuary matter afforded by evaporating four fluid ounces of Port wine, and attempted to separate the alcohol from this artificial mixture by freezing; but a spongy cake of ice was produced as in the last experiment.

When the temperature is more gradually reduced, and when large quantities of wine are operated upon, the separation of alcohol succeeds to a certain extent, and the portion which first freezes is principally if not entirely water: hence in some countries this method is employed to render wine strong.

Section II.

Having ascertained that alcohol exists in wine ready formed, and that it is not produced during distillation, I employed that process to discover the relative proportion of alcohol contained in different wines.

In the following experiments, the wine was distilled in glass retorts, and the escape of any uncondensed vapour was prevented by employing sufficiently capacious receivers, well luted, and kept cold during the experiment.

By a proper management of the heat towards the end of the process, I could distil over nearly the whole of the wine without burning the residuary matter: thus, from a pint of Port wine, of Madeira, of Sherry, &c. I distilled off from fifteen fluid ounces to fifteen fluid ounces and a half; and from the same quantity of Malaga and other wines containing much saccharine matter, I could readily distil from fourteen to fifteen fluid ounces.

In

In order to ascertain the proportion of alcohol with precision, pure water was added to the distilled wine, so as nearly to make up the original measure of the wine, a very small allowance being made for the space occupied by the solid ingredients of the wine, and for the inevitable loss during the experiments: thus, five fluid drachms and a half of distilled water were added to fifteen fluid ounces and a quarter of the liquor procured by the distillation of a pint of Port wine, and in other cases nearly the same proportions were observed. This mixture of the distilled wine and water was immediately transferred into a well stopped phial, and having been thoroughly agitated, was allowed to remain at rest for some hours; its specific gravity (at the temperature of 60° Fahrenheit) was then very carefully ascertained, by weighing it in a bottle holding exactly one thousand grains of distilled water at the above temperature, and the proportion of alcohol per cent. *by measure*, was estimated by a reference to Mr. Gilpin's tables *, the specific gravity of the standard alcohol being 0·82500 at 60°.

As the most convenient mode of exhibiting the results of these numerous experiments, I have thrown them into the form of a table: in the first column the wine is specified; the second contains its specific gravity after distillation, as above described; and the third exhibits the proportion of the pure spirit, which every hundred parts of the wine contain. I have also inserted porter, ale, cyder †, brandy, and some other spirituous liquours, for the convenience of comparing their strength with that of the wines.

* Phil. Trans. 1794.

† The proportion of spirit, which may be obtained from these three liquors, is subject to considerable variation in different samples: the number given for each, in the table, is therefore the mean of several experiments, as it did not seem necessary to specify them separately.

Wine.

Wine.	Specific Gravity after Distillation.	Proportion of Alcohol, per Cent. by Measure.	Wine.	Specific Gravity after Distillation.	Proportion of Alcohol, per Cent. by Measure.
Port - -	0·97616	21·40	White Hermitage	0·97990	17·43
Ditto - -	0·97532	22·30	Red Hermitage	0·98495	12·32
Ditto - -	0·97430	23·39	Hock - -	0·98290	14·37
Ditto - -	0·97400	23·71	Ditto - -	0·98873	8·88
Ditto - -	0·97346	24·29	Vin de Grave -	0·98450	12·80
Ditto - -	0·97200	25·83	Frontignac -	0·98452	12·79
Madeira - -	0.97810	19·34	Cote Roti - -	0·98495	12·32
Ditto - -	0·97616	21·40	Rousillon - -	0·98005	17·26
Ditto - -	0·97380	23·93	Cape Madeira -	0·97924	18·11
Ditto - -	0·97333	24·42	Cape Muschat -	0·97913	18·25
Sherry - -	0·97913	18·25	Constantia -	0·97770	19·75
Ditto - -	0·97862	18·79	Tent - -	0·98399	13·30
Ditto - -	0·97765	19·81	Sheraaz - -	0·98176	15·52
Ditto - -	0·97700	19·83	Syracuse - -	0 98200	15·28
Claret - -	0·98440	12·91	Nice - -	0·98263	14·63
Ditto - -	0·98320	14·08	Tokay - -	0·98760	9·88
Ditto - -	0·98092	16·32	Raisin Wine -	0·97205	25·77
Calcavella -	0·97920	18·10	Grape Wine -	0·97925	18·11
Lisbon - -	0,97346	18·94	Currant Wine -	0 97696	20·55
Malaga - -	0·98000	17·26	Gooseberry Wine	0·98550	11·84
Bucellas - -	0·97890	18·49	Elder Wine -	0·98760	9·87
Red Madeira -	0·97899	18·40	Cyder - -	0·98760	9·87
Malmsey Madeira	0·98090	16·40	Perry - -	0·98760	9·87
Marsala - -	0·97196	25·87	Brown Stout -	0·99116	6·80
Ditto - -	0·98000	17·26	Ale - -	0·98873	8·88
Red Champagne	0·98608	11·30	Brandy - -	0·93544	53·39
White Champagne	0·98450	12·80	Rum - -	0·93494	53·68
Burgundy -	0·98300	14·53	Hollands - -	0·93855	51·60
Ditto - -	0·98540	11·95			

LXXXIII. *Chemical Analysis of the Green Shell of the Walnut. By M.* HENRI BRACONNOT, *Professor of Natural History, Director of the Garden of Plants, and Member of the Academic Society of Nancy**.

THE shell of the walnut is, as every person knows, white in its interior when recent, but very speedily becomes coloured, and is at length of a very deep brown, which must be entirely owing to the contact of air; for, when plunged in boiling water it may be preserved some time without undergoing the least alteration. If it be exposed to the atmospheric air under a bell-glass, the oxygen is speedily converted, almost entirely, into carbonic acid; the shell becomes of a

* *Annales de Chimie*, 1811.

black

black colour; there is also without doubt water produced; so that altogether the phænomena of a real slow combustion take place. The oxygenated muriatic acid appears to have a different kind of action upon it, for instead of blackening, it causes it to take a yellow colour; the same effect is produced by the nitric acid.

In order to proceed to the analysis of the matters contained in the shell, I bruised a certain quantity of it in a marble mortar; the expressed juice was passed through a linen cloth and filtered; there remained upon the filter a green fecula, which soon passed into a deep brown by the contact of air; this matter, washed and dried, was put into some alcohol, which extracted from it the green resin common to most vegetables: the residue, insoluble in alcohol, was still coloured, and soft to the touch; a portion of it was mixed with weak nitric acid, which converted it into a thick viscid substance, soluble in water; alcohol produced from this solution a white flocculent precipitate. The same coloured residue, mixed with water, containing a small quantity of potass, produced an abundant quantity of a very light substance, of a deep red colour, similar to the crassamentum of the blood: lastly, another portion of the same residue was treated by boiling water, and formed starch jelly; whence it results, that the substance contained in such large quantity in the walnut shell is starch, contaminated by the colouring matter.

The juice of the walnut rind recently filtered is of an amber colour, of an acrid and acid taste, mixed with some bitterness; the acrid principle appears to be readily destructible, for the recent juice when left to stand some days, in changing its yellow colour for a blackish brown, where it has been in contact with the air, loses also its acrid taste, and becomes decidedly acid; at the same time there are formed on its surface black pellicles, which are soon renewed after they have been removed: these pellicles, carefully collected and well washed, afford by drying, a black brittle substance, of a shining vitreous fracture, and very like asphaltum or Jews pitch, but burning without flame, and therefore resembling charcoal. This coaly matter is soluble in potass, and is precipitated in flakes by an acid. It may be obtained more easily by evaporating, at a gentle heat, the recent juice, and diluting the residue with water: the supernatant liquor is of an agreeable acid taste; whence it results, that the acrid and bitter principle is entirely destroyed, and appears to be converted into the black matter nearly in the state of coal. No acetic vapours are disengaged

gaged from this same extract by the addition of sulphuric acid, even when assisted by heat; it does not therefore contain any acetic acid.

From what has been said, it appears, that in the rind of the walnut, as well as in many other herbaceous plants, there exists a matter held in solution in their fluids, the radical of which is hydro-carbon, more or less easily decomposable by the simple contact of air, which thus favours the production of water in rendering the carbon predominant. It will readily be conceived, that it is impossible to have a very exact notion of a matter so little permanent. Nevertheless, we perceive that it is scarcely coloured in the vessels of the plant, and that the action of air or caloric changes it greatly, causing it to pass by degrees into the state of extractive; another principle but badly defined, unstable in its elements, appearing rather to be the result of a decomposition, than a true product of living nature*.

The juice of the walnut shell, analysed by different reagents, afforded the following results.

It strongly reddened the infusion of turnsole. A solution of gelatin produced from it a slight precipitate, which must be owing to the tannin it contains. The sulphate of iron strikes so deep a green colour with this juice, that the liquor appears black; it afforded no precipitate, even by time, on account of the uncombined acid in the mixture, which is capable of giving a fine gray colour to wool and silk. Oxalate of ammonia indicated the presence of lime in this juice. Nitrate of barytes did not produce any sulphate. The nitrate of silver acts upon the juice, so as to detect the presence of the hydro-carbon radical, for it produced from it an abundant precipitate which speedily became coloured, and put on a metallic brilliant appearance by the reaction of the vegetable matter upon the oxygen of the oxide of silver: this precipitate was only partly soluble in the nitric acid, and left a coaly residue. Alkalies colour the walnut

* Having occasion to examine some extract of the *Rhus toxicodendron* prepared a few years ago, I made the following experiment. I applied some of it upon the skin of an animal, and caused him to swallow some large doses of it, without his experiencing any untoward effects; while a drop of juice from the stalk of the plant occasioned a violent inflammation upon the skin, terminating in an ulcer. The principles of plants therefore approaching to the state of extract, undergo an alteration which continually increases with time, and which must considerably vary their action upon the animal œconomy. Apothecaries might to a certain degree prevent this alteration, by inclosing the extracts in a state of perfect dryness in well-stopped vessels; for the humidity which they contain, or tend to absorb, contributes no less than the contact of air to destroy the weak equilibrium of some of their elements.

 juice

juice of a deep red, and produce precipitates which contain lime. If at the end of a certain time an acid is poured upon the liquor, another flocculent precipitate takes place, which when dried becomes black, vitreous in its fracture, and appears of a similar nature with the pellicles which are formed on the surface of the juice exposed to the air. Acetate of lead produced from the juice a flocculent precipitate of a whitish colour, and very abundant, entirely soluble in distilled vinegar. This precipitate, decomposed by sulphuretted hydrogen, afforded a coloured liquor of a very strong acid taste, combined with austereness; a sediment was produced in it by gelatin, and with acetate of lead it gave a precipitate soluble in vinegar. This acid of walnut, submitted to a gentle heat, afforded some small imperfect crystals swimming in an uncrystallizable liquor: the whole was mixed with carbonate of lime, and after having heated the mixture, which contained an excess of acid, it was filtered. I obtained by evaporation a thick coloured mass formed by the union of a number of small acicular crystals: this salt, treated with cold water, was partly dissolved by it; the solution, evaporated to dryness, left a brown residue like varnish, with the same habitudes as malate of lime, holding some tannin, which precipitated iron of a blackish-blue colour. That portion of the calcareous salt which was not dissolved in the cold water, was treated by weak sulphuric acid, which separated from it some citric acid still contaminated by malic acid.

The juice of the walnut shell thus freed from a part of the substances it held in solution was still coloured; the superacetate of lead produced another precipitate from it, and rendered the supernatant liquor nearly colourless: this precipitate furnished by analysis the same products obtained above, viz. malic acid, colouring matter and tannin, which had escaped the first precipitation on account of the presence of the acetic acid which was predominant in the liquor.

The fæces remaining after the expression of the juice were infused in alcohol, which extracted from them a green resinous matter; they were then boiled in water to free them from the starch and colouring matter which were contained in them: thus prepared, they were digested in weak nitric acid, which separated phosphate and oxalate of lime: these were precipitated from the acid liquor by ammonia. The method I employ to obtain separately these two earthy salts, so often combined in vegetables, is founded upon the property

perty possessed by distilled vinegar, when diluted with water, of dissolving the phosphate of lime without sensibly attacking the oxalate with the same base.

Although the walnut shell has a particular odour, it offers nothing remarkable by distillation in a water bath; there is only obtained an insipid liquor, which is of a brownish colour: on the surface of this liquor may be perceived small variegated crystals, which are at length deposited in the form of a sediment at the bottom of the fluid. The incineration of the shell gave for its product potass, carbonate of lime, phosphate of lime, and oxide of iron.

From this analysis it appears that the green fleshy shell of the walnut contains,

1. Starch. 2. An acrid and bitter substance extremely destructible, and which is converted into a carbonaceous state by the contact of air. 3. Malic acid. 4. Tannin. 5. Citric acid. 6. Phosphate of lime. 7. Oxalate of lime. 8. Potass.

LXXXIV. *Additional Facts relating to the Error discovered by Dr.* Kelly *in the Nautical Almanac.*

To Mr. Tilloch.

Sir, The statement of facts respecting the Nautical Almanac and *Connoissance des Tems,* which I sent you, and which was honoured with insertion in your Journal of October last, has been, I understand, read with very general interest; and considered as a plain, honest, and unassuming statement that required neither voucher nor signature. It was not inserted through any motive of vanity, but through the necessity of a very disagreeable and very singular case, which a further necessity now compels me to explain.— Neither was the statement dictated in terms likely to give offence, and yet it has called forth two letters of animadversion in your last Number, which are not of the most gentle tone or texture, and which I must therefore consider a kind of partnership production. But notwithstanding their manifest disposition, such is the force of truth, that all the leading facts in the statement remain uncontroverted, and indeed wholly untouched. They allow that an error has been committed in the Nautical Almanac, that the French and American astronomers have copied it; and that I have discovered it: but these writers must contradict

 something,

something, or what is the use or even the pretence of their letters? They therefore volunteer the French cause, and deny at considerable length that their astronomers pretend to original computation in the *Connoissance des Tems*, although the contrary is thus stated in the preface to that work, page 3: "*Les calculs ont été faits comme à l'ordinaire sous l'inspection du Bureau des Longitudes, par MM. Marion, Lalande,*" &c.

Here, sir, even the names of the computers are specified: but so hard pressed are these writers for subjects of controversy, that the second letter is, indeed, a complete refutation of an assertion that never was made. The quotation is as follows:

"You [Mr. Editor] further mention, that this error was *first* discovered by Dr. Kelly of Finsbury-square. Now, I must beg leave to assure you that is not the fact, having in the early part of the summer of 1810 shown the same to Dr. Maskelyne." Here the writer does not deny my having discovered the error; neither does he pretend to it himself; he only disputes my priority, though the word *first* is not to be found in the whole statement. No prudent man can positively say he is the first in any discovery; but he who detects an error and first announces it, and thus causes it to be corrected, will be considered as the legitimate claimant, and will therefore receive from the public whatever credit may be due to such a discovery. But perhaps I may have dwelt too long on this interpolation, which I am willing to believe was accidental; nor should I have noticed it, nor indeed any part of their letters, but for an assertion which follows in the same page, where the writer states that "*he pointed out the error in question to Mr. Pond, when he first came into office as Astronomer Royal;*" which was in the beginning of the present year. Now, sir, I can affirm with perfect truth and confidence, that in the month of September last Mr. Pond professed himself to me wholly unacquainted with any such error. It remains therefore between these two gentlemen to settle this mysterious and delicate question.

On my part, it may be proper to be more minute and circumstantial; for, if one assertion be ever set up against another, the truth can be distinguished only by the evidence of circumstances. When I first observed this error, I made numerous calculations to ascertain its extent; and here I may be permitted to say, that from my professional avocations, I was perhaps more likely to discover such

such a mistake than astronomers of higher pretensions. I have been for many years in the constant practice of teaching mathematical students to compute the columns of the Nautical Almanac; and I believe that my work on Spherics and Nautical Astronomy is the only publication where such calculations are particularly exemplified. It was not therefore surprising, that in the course of such practice an inaccuracy should be discovered which had escaped the notice of the principal astronomers of Europe. As soon as I had ascertained the extent of the error, I showed it to a few astronomical friends, who considered it till then unobserved, and advised that it should be immediately submitted to the Board of Longitude; which I resolved to do. But I first took care to consult the Astronomer Royal, lest the mistake might have been already known, and thus I should make myself ridiculous in announcing that as new, which was not so, and which I might therefore have had from hearsay. I accordingly waited on Mr. Pond on the 10th of September last; and, as I before stated, he professed himself wholly unacquainted with any such mistake. He indeed said that he thought he had heard something about an error in the Almanac of 1815 or 1816, (he believed,) but that he never looked for it; "*for you know,*" said he pleasantly, "*that to search for an error in the Nautical Almanac, would be like looking for a needle in a bundle of hay.*"

After I had satisfied him of the mistake by comparing different almanacs, I informed him that I should write to Sir Joseph Banks on the subject; which he approved of, and added—"*The Almanac must be corrected, and I shall state in the preface who it was that discovered the error.*"

It may not be improper to notice, that on my way to the Observatory I called on the Rev. Dr. Burney, and informed him of the purpose of my visit; and immediately on my return, I stated to him the substance of the above conversation.

Being now convinced that the error had not been previously known at the Royal Observatory, where it was chiefly interesting, I concluded that I might safely communicate it to the Commissioners of Longitude. I accordingly wrote to Sir Joseph Banks and to Professor Vince; and the latter gentleman in his answer observes, "The Board of Longitude must consider themselves much obliged to you for the discovery of the error in the Nautical Almanac;" by which it appears that even this great Astronomer, who was

Dr. Maskelyne's confidential friend, had not been aware of any such mistake.

Among the other scientific persons to whom the communication was made was the Earl of Rosse; which led to an official correspondence on the subject, between the Board of Admiralty and the Astronomer Royal.

I now, sir, considered the question as settled, so far as it regarded me, and therefore I dismissed it from my mind; but you may judge of my surprise soon after, on being unexpectedly shown an official copy of Mr. Pond's letter to the Admiralty, dated the 25th of September, and beginning thus: "*The mistake in the Nautical Almanac, 1812, alluded to by Dr. Kelly, I have been acquainted with ever since my appointment to my present situation**."

From this extraordinary preamble it might or might not be inferred, that the writer himself was the detector of the mistake; but the direct inference was, that my pretensions were wholly unfounded, having only *alluded* to a well-known subject; and to aggravate the insinuation, that I had made a Nobleman of the first consequence the medium of such delusive and frivolous communication.

In order therefore to undeceive such persons as might have seen this correspondence, the statement of facts was inserted in your journal; and it was drawn up with all possible delicacy, so as not at all to allude to Mr. Pond's letter: it only noticed Mr. Pond's having compared the almanacs of different years, by which it might be supposed that he was not acquainted with the error. And thus, sir, the question might have remained at rest, had not his mistaken friends (whether with or without his concurrence it is hard to say) thus imprudently interfered, and forced me to a further explanation, which I make with extreme reluctance, but which I am compelled to do in justice to myself. The question has indeed taken a most unfortunate turn; it is no longer a dispute about the credit of discovery, but the discredit of misstatement.

I could, sir, in corroboration of the foregoing circumstances, have accompanied this article with letters from some

* The remainder of this official letter was a recommendation to the Board of Admiralty, to postpone the correction of the error until the whole edition should be sold off;—a very uncertain period, and, if late in the year, a very ridiculous one. It is, however, important to observe, that as soon as the statement of facts appeared in the Philosophical Magazine, an order was given for computing and printing a new and accurate impression of the Nautical Almanac.

of

of the first characters; but it seems quite unnecessary. The facts speak for themselves. I want neither auxiliaries nor substitutes; nor shall I hereafter reply to any.

I am, sir,
your obliged and faithful servant,
P. Kelly.

Finsbury Square,
Dec. 28, 1811.

LXXXV. *Notices respecting New Books.*

Hortus Elginensis; *or A Catalogue of Plants, indigenous and exotic, cultivated in the Elgin Botanic Garden, in the vicinity of the City of New York. Established in* 1801 *by* David Hossack, *M.D. F.L.S. Professor of Botany and Materia Medica in Columbia College, Member of the American Philosophical Society, &c.* 2d Edition enlarged, pp. 76, 8vo. New York, 1811, with a well executed View of the Garden.

A Statement of Facts relative to the Establishment and Progress of the Elgin Botanic Garden, and the subsequent Disposal of the same to the State of New York. By D. Hossack, *M.D. &c.* pp. 58, 8vo. New York, 1811.

In the "Statement of Facts," Dr. Hossack details the difficulties, illiberal reflections, legal caution of the commissioners, and all the various obstacles which he had to encounter in selling his Botanic Garden to the State of New York. These are so numerous and so vexatious, that many years must elapse before any other person will venture to dispose of his property to the State. Commissioners were appointed by an act of the New York Legislature. These men nominated appraisers, who estimated the ground, consisting of twenty acres, and plants in the author's garden at 103,137 dollars: but the commissioners, after much delay and hesitation, thought proper to pay Dr. H. with only 74,288¾ dollars: this sum, independent of all his personal labour and attention during ten years, is more than 28,000 dollars less than he would have received for the money he disbursed, at simple interest. To the legislators he respectfully appeals for indemnity; and if his very temperate and just remonstrance does not meet due attention from the legislature of New York, the consequences will be more injurious to the progress of science in America, and more disgraceful to the State, than the ignorant and sordid citizens at present expect. The commissioners appear to have estimated their own merit by the extent of the sum they could withhold

withhold from this ingenious botanist and zealous lover of science. All the botanists, philosophers, and enlightened men in the State bear testimony to the merits of Dr. H. and his botanic garden.

The Catalogue of Plants, although in alphabetic order, will be found very convenient to students in botany who are but imperfectly acquainted with the English synonyms of vegetables, their habitats, and whether they are annual, biennial, perennial, shrub, tree, hardy, or belong to the green- or hot-house. Since Dr. H. was appointed Professor of Botany and Materia Medica in Columbia College, he wished to extend his botanical researches, particularly in collecting domestic plants. In his garden he has brought a conservatory and two spacious hot-houses, exhibiting a front of 180 feet, to great perfection. These he has enriched by presents from all the principal botanists in Europe, with whom he keeps up an extensive correspondence, and to whom he thus publicly expresses his obligations. Dr. H. also declares his "intention immediately to commence the publication of 'American Botany, or A Flora of the United States.' In this work it is his design to give a description of the plant, noticing its essential characters, synonyms, and place of growth; with observations on the uses to which it is applied in medicine, agriculture, or the arts;" illustrated with coloured engravings like Dr. Smith's English Botany. From the well known talents and industry of the author, European botanists will naturally await the appearance of such a work with much anxiety.

An Experimental Examination of the last Edition of the Pharmacopœia Londinensis; *with Remarks on Dr. Powell's Translation and Annotations. By* Richard Phillips. pp. 158, 8vo. W. Philllips and T. Underwood, 1811.

In our 26th vol. Mr. Phillips published an account of his analyses of Bath waters, and the ingenuity and mathematical accuracy of those experiments have ranked him among the most correct analysts of the day. Had the officiating members of the College of Physicians possessed any knowledge of chemistry, or even much chemical reading, such a chemist, as well from his practical as his theoretical knowledge, must have been one of the first persons whom they would have addressed on the subject of a new Pharmacopœia. This, however, it must be confessed, would have been more just than politic, as disease not health is the interest of physicians, and genuine science is by no means

means very propitious to the right worshipful art of man-slaying. The worthy collegians are too much men of business to overlook or neglect this obvious truth; and their *improved* Pharmacopœia will be a memorable example of their zeal for extensive trade in their own way. Formerly those intrusted with licenses "to kill," were obliged to wear either red coats, or a three-tailed wig with a black gown. Now, perhaps, less exterior ceremony may be necessary, and those authorized by law to "emancipate souls" may wear "black, brown, blue, or white," provided they operate *secundum artem.* This plan has at least œconomy to recommend it, and it may at the same time contribute to allay the gloomy apprehensions of Mr. Malthus and his disciples. Had Mr. Phillips considered the subject in this enlightened philosophical view, however he might have indulged himself occasionally in twitching their worships anonymously, he would most assuredly never have thus openly and decidedly endeavoured to obstruct such a glorious and patriotic purpose by an exposure of the ignorance and incapacity of the Royal College of Physicians. Poor man! his mind appears so wholly engrossed with chemical and medical science, as well as the love of truth, that he has forgotten the importance and superiority of self-interest. He is likewise misled by some old-fashioned honesty, by showing "that almost every change which can be considered as an improvement, as well as some alterations which are the reverse, have been copied from, or at least suggested by, the Dublin and Edinburgh Pharmacopœias, or by Dr. Duncan's Dispensatory." For the honour of our northern and western brethren, we hope that there are some things, either in the original Pharmacopœia or Dr. Powell's Translation, which are novel and perfectly original. Leaving, however, our author and Dr. Powell to settle this matter, and also the Doctor's confounding sulphuric with nitric acid and *vice versâ,* as well as the innumerable mistakes he has committed relative to muriatic, acetic, benzoic, and nitric acids, and solutions of ammonia, we have more pleasure in transcribing the following scientific description of sulphat of potash, for which Mr. P. candidly acknowledges the aid of that ingenious and able crystallographer Count de Bournon.

"The primitive crystal of sulphat of potash is a pyramidal dodecahedron with isosceles-triangular faces, meeting at the summit in an angle of 66° 15′, and at the base in 113° 45′. This form is considerably modified, both by the occurrence of additional faces, and the partial or total disappearance

appearance of the primitive ones. The edges formed by the union of the pyramids are very often replaced by narrow planes, forming a short prism: but the crystals more usually become prismatic by the elongation of two opposite faces on each pyramid, rendering the dodecahedron cuneiform; and the crystal then appears to be a long rhomboidal tetrahedral prism of about 66° and 114°, with tetrahedral pyramids, the prism being formed by the four elongated trapezoidal faces, and each of the pyramids by four of the faces of the dodecahedron which remain triangular. Sometimes two of the edges of the prism are replaced each by a plane, and it then becomes hexahedral; and when the same occurrence takes place with the four edges, it is rendered octohedral; frequently also two edges of the pyramid are replaced each by a plane, giving them six instead of four planes. These are not the only forms which this salt assumes; but it would be difficult to give an intelligible description of the rest without figures.

"The quantity of sulphat of barytes which 100 grains of this salt yield on the addition of muriat of barytes, is erroneously stated by Dr. Thomson to be 128 grains. I obtained a precipitate which, dried by a red heat, weighed 136·7 grs. being only 1·45 grain more than the proportion mentioned by Kirwan; indicating about 32·8 of sulphuric acid, if with Dr. Thomson we allow 100 parts of sulphat of barytes to contain 24 of sulphuric acid. This salt then consists of 32·8 sulphuric acid with 67·2 potash and water, instead of 31 sulphuric acid with 69 potash and water, as stated by Dr. Thomson."

The College has introduced the supersulphat of potash into their materia medica; but their directions for preparing it are as usual very defective, and Mr. P. naturally supposes that "neither the College nor its Committee have ever examined the true supersulphat of potash." The author received "some extremely well defined crystals of this salt from Messrs. Howard, of Stratford; their form was distinctly rhomboidal; but as quartz sometimes occurs of this figure, although, as well as sulphat of potash, it is more usually dodecahedral, he supposed it possible that the rhomboid might in this case be derived from the dodecahedron, and consequently that these crystals might be common sulphat of potash." Count de Bournon however examined their crystalline form; and on comparing it with that of the sulphat above described, it appeared that "the rhomboid differed so materially from that which would be derived from the dodecahedron of sulphat of potash, that these salts

salts must differ totally from each other, either in the nature or proportion of their constituent parts. The primitive crystal of supersulphat of potash is an acute rhomboid of 74° and 106°, the summits of which are sometimes replaced by planes perpendicular to the axis of the crystal; and when this occurs at the small diagonal of the crystal, it assumes the appearance of an octohedron." Mr. Phillips then procured some supersulphat of potash from Apothecaries' Hall, which he found tasted at first sour, and reddened blue vegetable colours; but the acid taste was soon succeeded by a bitter one, and the salt evidently consisted of a prismatic variety of common sulphat of potash, mixed with a considerable quantity of slender prismatic crystals, which proved to be common nitre. As a proof of this, 100 grs. of true supersulphat saturated 25 grs. of dried subcarbonat of soda; 100 grs. of sulphat mixed with excess of acid saturated only half a grain, and a similar quantity of the reputed supersulphat, from the Apothecaries' Hall, saturated about one grain. Hence Mr. P. ascertained that 100 grs. of the salt prepared and sold under collegial authority for supersulphat of potash, consists of "58 sulphat of potash and 42 nitrat of potash. When, therefore, two drachms of this compound are exhibited, instead of the like quantity of supersulphat of potash, two and a half scruples of nitre are given, exceeding by a whole scruple the largest dose mentioned by Dr. Powell." This, no doubt, is wonderfully accurate and scientific! The College chemists, and their translator, annotator, or illustrator, must be considered as public benefactors in exalting the character of their country for profound science and accuracy. The confounding of two salts, however, which differ both in the quantity and the quality of their constituent parts, even should it cost a few score of lives, or augment the number of patients a few thousands annually, can only be considered a frivolous circumstance, as perhaps not every third member of the College ever cast an eye on these salts, or could tell what they were if laid before them on a page of their Pharmacopœia.

If colleges be doomed either to sleep or wake only to blunder, it is some consolation that the present errors have given occasion to very accurate crystallographical descriptions of several chemical products, which have hitherto been but imperfectly depicted. The "form of the primitive crystal of supertartrat of potash is a rectangular octohedron, having two of its faces more inclined than the other two; the former meeting at the summit in an angle of of about 60°, and at the base in about 120°; and the latter at the summit in 50°, and at the base in 130°. When the superabundant acid has been saturated so as to produce tartrat of potash, the primitive form is altogether different; it is a rectangular tetrahedral prism, of which the height being one, the sides of the terminal faces will be to each other as four to five; most commonly the summit is dihedral, from the *replacement* of one of the solid angles of the primitive crystal by a plane (*i. e.* dihedral in consequence of one of the solid angles being replaced or rather superseded by a plane), which makes the terminal face an angle of 162° 38′: frequently the triangular faces of the pyramid become very narrow trapezoidal faces, by the increase of two opposite planes: and this is one of the more usual varieties of this salt. 36 parts of supertartrat of potash require 15·7 parts of subcarbonat of potash for their saturation, instead of 12 parts as directed by the College."

The *soda tartarizata* of the College, or more properly *tartras sodæ et potassæ*, is vaguely described by Dr. Powell as its crystals being prisms of eight or ten unequal sides; but its "primitive crystal is a right rhombic prism of 80° and 100°, the edges of the prism are differently replaced so as to render it sometimes hexahedral, or octohedral, but more frequently decahedral or dodecahedral." "The primitive crystal of sulphat of soda appears to be a right rhombic prism of about 72° and 108°. It is frequently difficult to distinguish the planes of the prism, on account of their being deeply channeled. The terminal faces of these crystals are often replaced by a dihedral summit with triangular faces, ocrruing on the edges of 72°; and this variety has probably given rise to the opinion, that the primitive form of this salt is a cuneiform octohedron: but as the planes of the summit make with the edges of 72° an angle of about 137°, they meet each other at 86°, whereas in the supposed crystal it would be 108°. The pyramid also frequently becomes hexahedral, by the occurrence of four additional faces upon the edges of the terminal faces, with which they form an angle of about 125°."

There are many more important facts and observations in this volume, but the above are sufficient to convince our readers that it is very well worthy their perusal. With the exception of two or three inaccurate expressions, and a few manipulations in which dispatch has superseded œconomy, it evinces such talents, chemical knowledge, and great accuracy, as would do honour to any chemical philosopher.

Information

Information requested respecting the Writers of the Mathematical Questions, and their Answers, in The Ladies' Diary.

The annual publication called *The Ladies' Diary*, or *Woman's Almanac*, has every year for upwards of a century contained a certain number of mathematical problems, to be answered in the Diary of the following year. The publication of these has answered several valuable purposes; in particular it has awakened the attention of many to the study of the mathematical sciences, who would not otherwise have thought of them. The questions have served to exercise the ingenuity and call forth the exertions of young mathematicians, some of whom have in time arrived at great eminence, as cultivators of mathematical learning; and, lastly, the work has served as a repository for the preservation of many curious mathematical disquisitions, which, but for this mode of publication, would never have been known to the world.

The beneficial influence which The Ladies' Diary has exerted upon the state of mathematical science in this country, has been long felt and acknowledged, and has been particularly noticed by the writer of the very valuable analysis of the *Mécanique Céleste*, given in the Edinburgh Review. Speaking of the comparative state of mathematical knowledge in England and on the continent, he says: "A certain degree of mathematical science, and indeed no inconsiderable degree, is perhaps more widely diffused in England than in any other country in the world. The *Ladies' Diary*, with several other periodical and popular publications of the same kind, are the best proofs of this assertion. In these, many curious problems, not of the highest order indeed, but still having a considerable degree of difficulty, and far beyond the mere elements of science, are often to be met with: and the great number of ingenious men who take a share in proposing and answering these questions, whom one has never heard of any where else, is not a little surprising. Nothing of the same kind, we believe, is to be found in any other country. The geometrical part has always been conducted in a superior style; the problems proposed have tended to awaken curiosity, and the solutions to convey instruction, in a much better manner than is always to be found in more splendid publications."—(See *Edin. Rev.* vol. xi. p. 282.)

A collection of all the mathematical questions as well as other parts of the Diary, from its beginning to the year 1772, was published about that period by its present ingenious

genious and learned editor Dr. C. Hutton, late of the Royal Academy, Woolwich. That work however being now out of print, and the stock of questions considerably increased, Mr. T. Leybourn, editor of the Mathematical Repository, has issued proposals for publishing, by subscription, all the mathematical questions and their answers, from the commencement of the Diary to the present time. Besides the valuable notes given in Dr. Hutton's edition, the present editor intends to give others, and in particular he means to give, as far as he can, brief notices of any circumstances he may be able to learn respecting such authors of the answers to the questions as are dead, and even of such as are alive when it can be done with propriety.

But as many of the authors have now been dead for a number of years, and have not been known beyond the particular circle of their friends, he is aware that this part of the work can only be rendered tolerably complete by the assistance of such friends to his undertaking as may be capable of giving the information here specified. He ventures, therefore, through the medium of The Philosophical Magazine, to solicit communications respecting the authors of the mathematical parts of the Diary. These may be addressed to him at the Royal Military College, Great Marlow, Bucks.

Original Vaccine Pock Institution.

The following Resolutions must be interesting to our readers, being an extract from a very recent publication of the Original Vaccine Pock Institution, in Broad-street, Golden-square, containing the results of eleven years practice, which has the candour to publish the defects as well as the advantages of vaccination.

1. That it does appear in the practice of this Institution that the small-pox has occurred subsequently to vaccination in the most distinct manner, in the proportion of about one of 550 in 5000 patients.

2. That in these cases of failure the small-pox was in none very severe, but, on the contrary, in most of them milder than usual in even the inoculated small-pox.

3. That there have not occurred any alarming vaccine cases, excepting in a very small proportion from inflamed and sore arms.

4. That in many hundreds subjected to the counterproof of re-inoculation with variolous matter, not one has taken the small-pox.

5. That

5. That many hundreds have been re-inoculated with vaccine matter without being able to reproduce the cow-pock.

6. That according to considerable experience of this Institution, persons are alike unsusceptible or susceptible of the small-pox and cow-pock after variolation or vaccination.

7. That re-inoculation with vaccine is preferable to variolous matter, because it is desirable to avoid the risk of infecting others by disseminating small-pox infection; and because the vaccina is rarely attended with danger or with severe symptoms.

8. That the cow-pock matter and variolous matter are distinctly different species of matter, in the just sense of the term species or kind.

9. That there is now good evidence, that in the cases of failure the constitution had generally been rendered less susceptible of violent action from the agency of the small-pox matter.

10. That it does not appear that any new disorder is liable to be occasioned after vaccination, excepting probably certain eruptive complaints, which, however irritating, have in no instance produced any serious injury; and it appears that certain diseased states which come on after the cow-pock do not supervene so frequently as after the small-pox.

11. That although it does appear from the London bills of mortality, that 420 fewer deaths per annum, on an average, have occurred during the twelve years of vaccination than during the twelve preceding years, yet the conclusion that the diminished mortality has been occasioned by vaccination is liable to error, but the probability is in favour of the new practice.

Mr. Richard Walker, of Oxford, whose valuable communications on various branches of science have frequently appeared in The Philosophical Magazine, is about to publish "An Epitome of the Practice of Physic and Surgery, exhibited in a Systematic Arrangement of Diseases and Remedies, upon a Plan entirely new; in which are pointed out those Diseases which are *curable*, and those which admit of *Palliation* only; and the most appropriate Means of effecting each of these Intentions; with Observations and various suggested Improvements."

Mr. Walker has been upwards of 20 years Apothecary to the Radcliffe Infirmary. The results of his extensive practice and long experience must, therefore, excite considerable interest in the medical world.

Mr.

Mr. Syer, Surgeon, City Terrace, City Road, has just published a very useful medical work, entitled "A Treatise on the Management of Infants; containing the General Principles of their Domestic Treatment, with the History and Method of Cure of some of their most prevalent and formidable Diseases."

The above work is calculated to interest not only the junior branches of the medical profession, but those also who are principally employed in the management of children; and, we have no doubt, will prove a most valuable addition to our present stock of books on domestic medicine.

LXXXVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

On Nov. 28, and Dec. 5, the conclusion of Mr. Brande's Researches on the Blood was read. The result of the author's experiments is, that very little iron exists in the blood, that the quantity is so very small as to render it improper to attribute the colour of blood to the iron it contains, and that its influence must be much less than has been generally supposed.

Dec. 12. A paper by Mr. Home on the Structure of the Ear of the Whalebone Whale (*Balena Mysticeta*) was read, in which that anatomist described the nature of this organ in whales, the situation and dimensions of the tympanum, and adjoining parts.

Dec. 19. The first part of a paper by Dr. Herschel on the Comet was read. This astronomer, in the course of his observations on the comet, noticed something like a distinct luminous body about the centre of its head, or what some astronomers would call its nucleus. He observed this luminous part change its relative position in the head, sometimes appearing nearer, at others further from the side next the sun; at the same time he discovered considerable difference in its brilliancy. Hence he was led to infer that the comet enveloped a real planetary body; and after a series of observations, on the 16th of October, when the comet was 114 millions of miles from the Earth, he ascertained that this body was 428 miles in diameter, and surrounded with a cometic atmosphere. For this purpose he viewed it with seven, ten, and twenty feet telescopes, containing magnifiers of various powers, from forty to those which magnified 600 times. The reading of the remainder of this curious paper was deferred till a future meeting, and the Society adjourned till Thursday, January 9, 1812.

LXXXVII. *Intel-*

LXXXVII. *Intelligence and Miscellaneous Articles.*

A fresh eruption has taken place from Mount Ætna, which is thus related in the French Journals:—" On the 27th of October, several mouths opened on the eastern side of the mountain; these openings, situate almost in the same line, and at equal distances, presented to the eye a spectacle the most imposing—torrents of burning matter, discharged with the greatest force from the interior of the volcano, illuminated the horizon to a great distance. One of these apertures was a considerable distance from all the others. The former was about 300 toises beneath the crater, and about one mile from the point called Gamel Laco: five others were situate in a line in the direction of the Valley of Oxen (del Bove). The eruption of these last five lasted the whole night; an immense quantity of matter was discharged from them, which was driven to a considerable distance. They however ceased the following day to cast forth any lava. The first aperture continued still, on the 15th of November, to emit torrents of fire; and even at the time when this mouth had the appearance of being stopped, there suddenly issued from it clouds of ashes, which descended in the form of rain upon the city of Catana and its environs, and upon the fields situate at a very great distance. The current of the lava was still very slow, inasmuch as in the space of nine days it had scarcely passed over three miles, and had only reached the rock called della Capra (the Goats). A roaring resembling that of the sea in the midst of a tempest was heard in the interior of the mountain. This sound, accompained from time to time with dreadful explosions resembling thunder, reechoed throughout the valleys, and spread terror on every side. Such was the state and situation of Mount Ætna on the 18th ult. The eruption still continued, and caused a dread of the most terrible disasters."

NEW COMET.

" Imperial Observatory, Paris, Dec. 9, 1811.

" A new comet has just been seen in the constellation of Eridanus—it was seen by M. Pons at Marseilles on the 16th ult. The thick weather did not permit its being seen at Paris before the 5th instant; on that day, at 11 hours 1 minute of mean time, its right ascension was 64. 23, and the declination 13. 34. S. The apparent motion of the comet in right ascension is retrograde and very slow, and the motion in declination carries it towards the northern hemisphere.

sphere. It is not visible by the naked eye; with very good glasses, its nucleus, the light of which is vivid, is seen surrounded with a light cloud, but not the slightest trace of a tail is to be seen.

"It was announced that, on the 4th of November, at 7 P. M. there was seen at Berne, in the direction of E.N.E. a comet above the horizon; and it was added, that the tail was turned directly on the side of the horizon. It will not be unnecessary to remark, that these indications cannot in any measure relate to the comet discovered by M. Pons, first, because on the 4th of November this comet was still below the horizon at eight at night, and next, because it rises in the S.E. and has no tail. It is probable that the astronomer of Berne mistook the nebulus of Andromeda for a comet."

This additional sublime stranger was seen on the evenings of the 22d and 23d of December at the Glasgow Observatory. Its position has varied considerably in declination from that given by the French astronomers for the 5th of December. It is now very near the equator. Its appearance, when viewed in the ten feet Herschelian, with a power of 250, is extremely beautiful. The nebulous cometary mass is condensed, appears bright, notwithstanding the vicinity of the moon. There is at present a fine double star a little to the south-west of it. The rapidity of its motion is evident, even in the common telescope, in the interval of one day. Its situation has been carefully determined on both evenings by a series of azimuths and altitudes with the great astronomical circle constructed by Mr. Troughton. It is still in the extended constellation Eridanus.

M. Vauquelin has constructed tables, by the inspection of which, the proportion of concentrated sulphuric acid contained in any mixture of that acid and water, indicated by the different degrees of the areometer, may be correctly ascertained. This is sometimes of considerable importance to the manufacturer of soda, and to other consumers of this acid, since diluted acid is often more advantageously employed, and the quantity of concentrated acid is not exactly in proportion to the degree of the areometer, on account of the mutual penetration which takes place at the moment of combination of the acid with water; a circumstance which occasions such manufacturer to pay a greater price for the weaker acids than is proportioned to the quantity of real acid they contain. This table is the result of many

many experiments made for the purpose. The concentrated acid is taken at 66° of the areometer.

1. Acid at 60°.	
Sulphuric acid	84,22
Water	15,78
2. Acid at 55°.	
Sulphuric acid	74,32
Water	25,68
3. Acid at 50°.	
Sulphuric acid	66,45
Water	33,55
4. Acid at 45°.	
Sulphuric acid	58,02
Water	41,98
5. Acid at 40°.	
Sulphuric acid	50,41
Water	49,59
6. Acid at 35°.	
Sulphuric acid	43,21
Water	56,79
7. Acid at 30°.	
Sulphuric acid	36,52
Water	63,48
8. Acid at 25°.	
Sulphuric acid	30,12
Water	69,88
9. Acid at 20°.	
Sulphuric acid	24,01
Water	75,99
10. Acid at 15°.	
Sulphuric acid	17,39
Water	82,61
11. Acid at 10°.	
Sulphuric acid	11,73
Water	88,27
12. Acid at 5°.	
Sulphuric acid	6,600
Water	93,400

Specific gravities of the above mixtures corresponding to the different degrees of the areometer:

		Sp. Grav.
Acid at	5 degrees .	1,023
	10	1,076
	15	1,114
	20	1,162
	25	1,210
	30	1,260
	35	1,315
Acid at	40 degrees .	1,375
	45	1,466
	50	1,524
	55	1,618
	60	1,725
	66	1,842

This last table is useful to those who have not an areometer at hand, since, the specific gravity of any given acid being ascertained, the proportion of concentrated acid contained therein is shown by turning to the corresponding degree of the areometer in the former table. The strength of the acid at any intermediate degree may easily be found by the rule of three.

M. Lesauvage has published at Paris an Inaugural Dissertation, entitled, *Recherches sur les Effets du Verre et des Substances vitriformes portées à l'intérieur des Organes digestifs;* in which he has proposed the following question: "Whether glass and vitriform substances, more or less,

coarsely

coarsely powdered, or even in small fragments, are able during their passage through the alimentary canal, to produce irritation, erosion, laceration, or any mechanical alteration upon the parts with which they come in contact? He endeavours to refute the opinion of those who think that any injury can arise from taking fragments of glass or vitreous substances into the stomach, and relates a number of experiments made upon himself, by frequently swallowing sharp pieces of broken glass, both on an empty stomach and after meals. From these experiments he infers, that glass, and analogous substances, produce no chemical action on the digestive organs of living animals; nor are they acted upon by the fluids or gases contained in these organs; that the mechanical effects, said by authors to have been produced by irregular fragments of glass on the intestinal tube, have been merely imagined and never really seen, and that still less can any such effects arise from glass in powder, whether coarse or fine. That experiments purposely made upon living animals prove beyond doubt, not only that those substances are incapable of mechanically injuring the alimentary canal, but that they do not produce the slightest irritation in it; and that any one may easily satisfy himself by trying the experiment, that they do not produce the least uneasy sensation. He concludes, therefore, that it is erroneous to retain in the class of poisons, already too numerous, a particular order of mechanical poisons, acting by irritating and lacerating the organs to which they are applied, since it is proved such an order has no existence in nature.

LECTURES.

Theatre of Anatomy.

Lectures on Anatomy, Physiology, Pathology, and Surgery, by Mr. John Taunton, F.A.S. Member of the Royal College of Surgeons of London, Surgeon to the City and Finsbury Dispensaries, City of London Truss Society, &c.

In this Course of Lectures it is proposed to take a comprehensive view of the structure and œconomy of the living body, and to consider the causes, symptoms, nature, and treatment of surgical diseases, with the mode of performing the different surgical operations; forming a complete course of anatomical and physiological instruction for the medical or surgical student, the artist, the professional or private gentleman.

An ample field for professional edification will be afforded by the opportunity which pupils may have of attending the clinical

clinical and other practice of both the City and Finsbury Dispensaries.

The Winter Course will commence on Saturday, January the 25th, 1812, at Eight o'Clock in the Evening *precisely*, and be continued every Tuesday, Thursday, and Saturday, at the same hour.

Demonstrations regularly in the forenoon by Mr. Wm. Dunning.

Particulars may be had, on applying to Mr. Taunton, Greville Street, Hatton Garden.

Dr. Clutterbuck will begin his Spring Courses of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, on Monday the 20th of January, at Ten o'clock in the Morning, at his house, No. 1, in the Crescent, New Bridge-street.

The different Lectures are given on alternate days: viz. Practice of Physic, on Mondays, Wednesdays, and Fridays; Materia Medica and Chemistry, on Tuesdays, Thursdays, and Saturdays, at the same hour.

Metereological Observations made at Clapton in Hackney, from Oct. 24 to Dec. 19, 1811.

Oct. 24.—Overcast morning followed by small rain and a fair evening, air misty, and wind gentle from the south.

Oct. 25.—Much dew on the grass; and a clear day with a few fleecy *cumuli*. The mercury in the barometer descended rapidly towards evening, followed by gales of wind and showers from S. W.

Oct. 26.—Barometer unusually low; gentle showers in the morning; fair by day with clouds in various stations; hard showers of rain and hail at night: wind southerly.

Oct. 27.—Gentle showers early, then fair day with petroid *cumuli* and *cumulostrati, cirri* and *cirrostrati*. Showers again at night, from S. E.

Oct. 28.—Clear morning and N. W. wind; *cirri* above *cirrostrati*; — towards evening *cumulostratus* beneath a thick veil of *cirrostratus* produced partial *nimbification* and showers, which became hard at night, with squalls from S. W.

Oct. 29.—Showers with fair intervals: fair night with light cirrocumulative clouds; also a sheet of the *cirrostratus* appeared extending N. W. and S. E. Wind S.W.

Oct. 30.—Much rain with south-east wind, followed by showers from south-west; in the fair intervals of which ap-

 peared

peared confused flimsy *cirri* with *cirrostratus*, *cumulostratus*, and fleecy *cumulus* in successively lower regions. A bright meteor appeared about half past ten P. M.

Oct. 31.—Clear morning, with a line of *cirrostratus* east and west, followed by much cloud showing features of *cirrus* and *cirrocumulus* with *cumulus* below, and hazy air. The temperature increased at night, with wind and rain from S. W.

Nov. 1.—Wind and rain from S.W. very warm.

Nov. 2.—Long and gentle showers from S.W. various clouds in the intervals; high wind by night, which blew over large masses of fleecy *cumulus.*

Nov. 3.—A strong wind from S.W. in the morning blew over large masses of *cumulus* and *cumulostratus;* through the breaks *cirrus* appeared in a lofty station; as the day advanced the wind fell, the quantum of cloud decreased, and a sort of irregular confluent *cirrocumulus* became most prevalent; in passing to this cloud the *cirrus* presented various appearances. Some brilliant meteors at night.

Nov. 4.*—Showers before light; fair day; a shower about three in the afternoon, petroid *cumuli* intersected by *cirrostratus* and others.

Nov. 5.—Wind and rain from S.W. all the morning; about three o'clock it became fair and calmer, and the night was clear with some light confluent cirrocumulative clouds.

Nov. 6.—Calm misty morning followed by much rain; it held up for several hours during the day, but returned at night with light gales from S.W.

Nov. 7.—Fine day and cooler than hitherto; *cirrus* spread about in loose masses; also in some places in tufts: features of *cirrocumulus* appeared; and also *cumulus.* A little before nine a large meteor was seen by several persons in the neighbourhood: its motion was irregular. Wind W.

Nov. 8.—Rain more or less all day, with calm air.

* In the progress of nimbification the following phænomena may be frequently observed: The *cirrus* losing its cirriform figure becomes a confused and dense veil swelling downwards to meet *cumulus* below, which rises irregularly upwards; but the change to *cumulostratus* precedes visible inosculation: after the *nimbus* has formed, a crown of cirrose structure may be seen extending upwards as if conducting electricity from an upper region, while the shower is nourished from below by fleecy *cumuli*, which float under and become lost in it.

I have observed that when *cumuli* pass to *cumulostrati*, they move much slower though in the same wind: Is this to be attributed to their increase of density in proportion to the surface they present to the wind?

Nov.

Nov. 9.—Thick yellow *stratus* followed by fair but misty afternoon: the fane indicated S. but it was quite calm.

Nov. 10.—Much rain in the morning, afterwards fair. Wind westerly.

Nov. 11.—Clear morning: light *cirrus* spead aloft while *cumuli* appeared below, afterwards *cumulostratus* and very slight showers: the wind got up about two o'clock S. and N.W.

Nov. 12.—Fair day, with various clouds. Wind N.W.

Nov. 13.—Showers early, clouds in two strata, with gentle showers again: clear cool night.

Nov. 14.—Mist, followed by much rain; afterwards *cumulostratus* at different heights; dark flocks of *cumulus* called *scud* below. Clear night with a few little meteors.

Nov. 15.—Fair day, with cool breeze, *cirrus* and *cumulus.* Clear cold windy night: a few shooting stars. Wind W.

Nov. 16.—Rain early, then *cirrus* and *cirrocumulus:* clear, cold and windy night, with a few falling stars. N.W.

Nov. 17.—Sky veiled nearly all day, with thick *cirrostratus,* features of *cirrus* and *cirrocumulus:* the temperature increased in the evening, which was dark and cloudy, with some rain. Wind gentle from N.W.

Nov. 18.—Calm misty day with small rain. Wind W.

Nov. 19.—Loose *cirrus* and *cirrocumulus* and *cumulus* after a foggy morning. Wind S.W.

Nov. 20.—Clear morning and variable wind. Linear *cirri* stretched along in different directions, with some features of cymoid *cirrostratus.* Clear frosty night with some falling stars.

Nov. 21.—Clear white frost with various features of *cirrus* and *cirrocumulus* followed by yellow fog; a reddish colour appeared all around by twilight: dark night. Wind W.

Nov. 22.—Masses of *cumulus* by day, clear frosty night, and North wind.

Nov. 23.—Clear frosty day, and wind northerly.

Nov. 24.—*Cirrus* spread about aloft, *cirrostratus* and flocky *cumuli* in successively lower regions: the same order of clouds continued till after sunset, but the night became clear: wind northerly.

Nov. 25.—Still day, confused and lofty *cumuli* followed by cloudy afternoon, with some drops of small rain. N.

Nov. 26.—Cloudy and damp; wind below N.W. above which was an upper current from north, as appeared by a small rarified air balloon sent up at about nine in the morning.

Nov. 27.—Overcast day with gentle wind from W.

Nov. 28.—Cloudy and hazy day: wind S.W.

Nov.

Nov. 29.—Very calm morning with a white *stratus*, in which a small rarified air-balloon launched at nine o'clock was soon lost; it indicated a wind from N.W. Afterwards it cleared, when a loose kind of *cirrocumulus* was discerned, followed by increased warmth of the air, which this cloud generally forebodes.

Nov. 30.—Foggy morning and cloudy day. By night lofty, thin, and confused clouds caused a lunar corona. About midnight a very little white meteor leaped horizontally across the moon.

Dec. 1.—Overcast and misty with small rain before noon: the wind increased towards night and became high from S.W.

Dec. 2.—Hard shower early, afterwards strewed *cirrus*. Wind S.E. and N.

Dec. 3.—Hazy and cloudy: windy night. S.W.

Dec. 4.—Fair and windy; snow fell in the night. S.W.

Dec. 5.—Fair; snow on the ground. Wind N.

Dec. 6.—Cloudy for the most part. Wind N.

Dec. 7.—Misty and cloudy. S.W.

Dec. 8.—Hazy and cloudy; rain at night. S.

Dec. 9.—Much rain with strong wind from S.W.

Dec. 10.—Wind with some rain; fair by night. S.W.

Dec. 11.—The lower current of wind N.E., above it a current blew from E. as appeared by a small balloon launched from Clapton. Cloudy evening.

Dec. 12.—Yellow *stratus* followed by clouds and wind, with some rain. W.

Dec. 13.—Cloudy and hazy, afterwards wind and small rain.

Dec. 14.—Clear morning; some rain at night. N.N.W.

Dec. 15.—Cloudy and hazy, followed by rain in the evening.

Dec. 16.—Fair wind, light (particularly at night), from the W.

Dec. 17.—Cool morning; *cirrocumulus* was followed by warmer evening, with some small rain.

Dec. 18.—Cloudy and misty.

Dec. 19.—Early appeared linear and plumose *cirri*, with hazy atmosphere; cloudy day followed with S.W. wind.

Clapton, Dec. 19, 1811. THOMAS FORSTER.

METEORO-

METEOROLOGICAL TABLE,
By Mr. Cary, of the Strand,
For December 1811.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Nov. 27	43	45°	43°	30·40	0	Small rain
28	43	47	40	·35	10	Cloudy
29	42	46	45	·28	12	Cloudy
30	46	51	44	·25	16	Cloudy
Dec. 1	44	54	50	·08	0	Small rain
2	53	46	38	29·52	0	Stormy
3	43	49	40	·71	6	Cloudy
4	42	50	33	·30	16	Fair
5	32	30	24	·70	16	Fair
6	24	36	37	·95	6	Fair
7	48	50	50	·68	0	Rain
8	54	54	43	·43	0	Small rain
9	43	48	42	·02	0	Stormy
10	42	42	40	·18	7	Fair
11	39	42	33	·70	12	Fair
12	33	42	50	·86	6	Cloudy
13	51	55	54	·67	6	Cloudy
14	38	43	35	·82	10	Fair
15	36	46	46	·86	9	Cloudy
16	35	40	40	·29	10	Fair
17	40	42	36	·50	8	Fair
18	33	46	46	·75	6	Cloudy
19	51	53	50	·66	10	Fair
20	50	52	50	·70	0	Small rain
21	50	46	33	·80	0	Small rain
22	28	33	29	30·19	12	Fair
23	43	48	43	·01	7	Fair
24	43	43	32	·10	0	Small rain
25	29	33	28	·20	6	Fair
26	28	33	25	29·70	0	Sleet

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

⁂ In our last Number, in the article on the different Theories of Arches or Vaults, p. 287—391, for *vaussoir* read uniformly *voussoir.*

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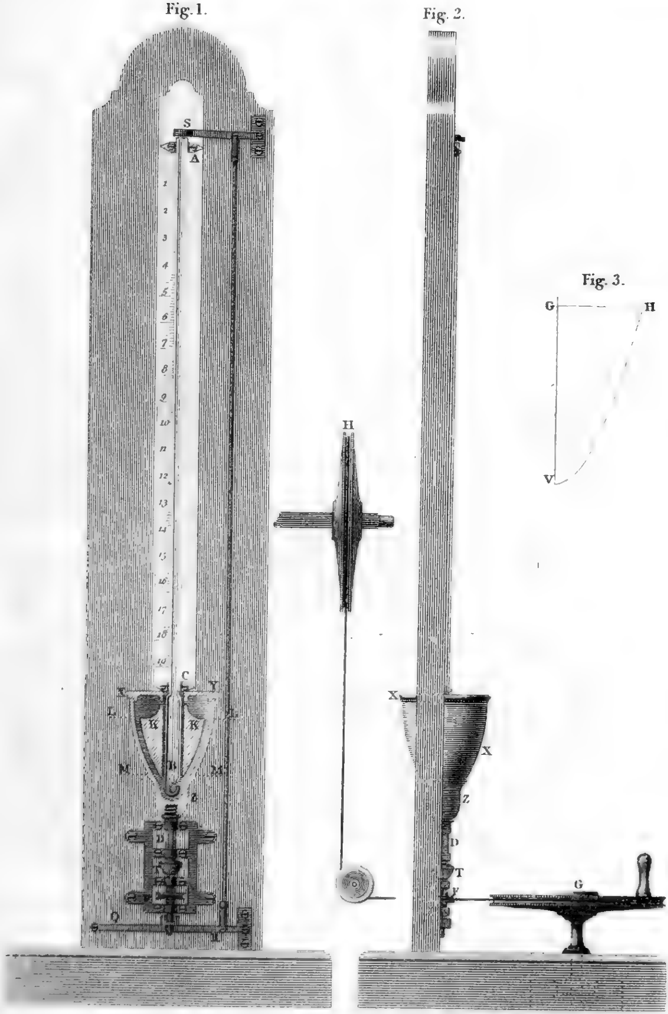
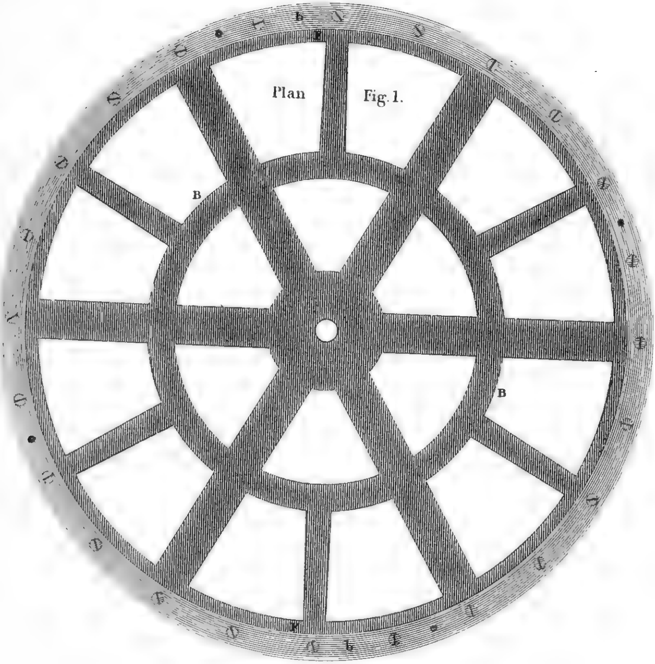
END OF THE THIRTY-EIGHTH VOLUME.

Printed by Richard Taylor and Co., Shoe Lane, London.

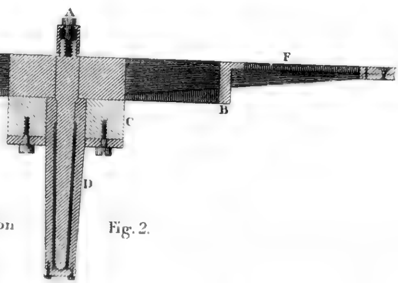
Mr. Donkin's Tachometer.





Plan Fig. 1.

Mr. Allan's Mathematical Dividing Engine.



Section Fig. 2.

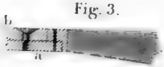


Fig. 3.

5 0 5 10 15 20

Scale of Inches.

Porter sculp.

Fig. 5.

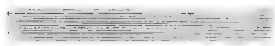


Fig. 3.

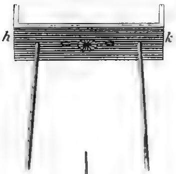
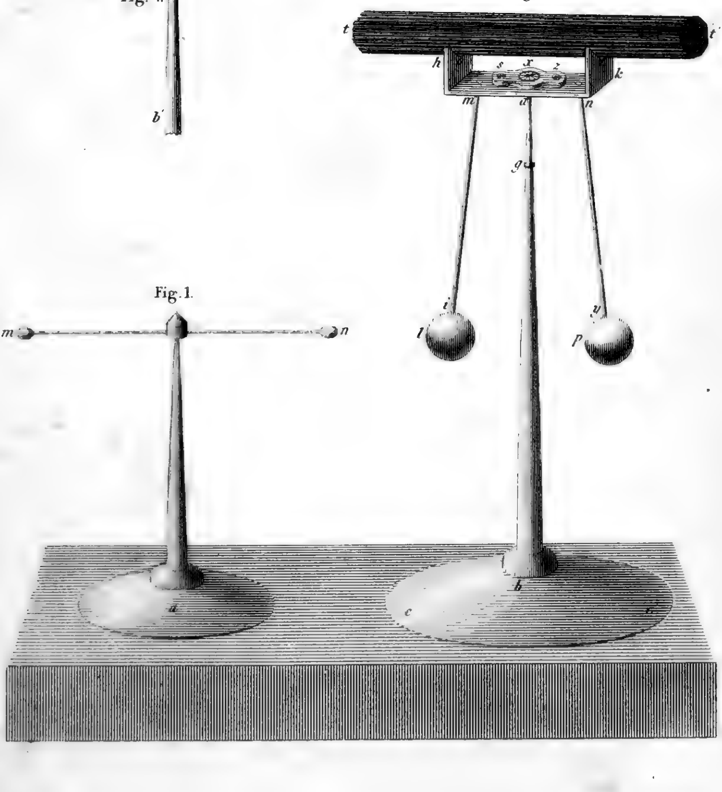


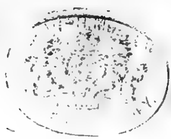
Fig. 4.

Fig 2.

Fig. 1.



S. Porter sculp



Mr. Moult's Filtering Apparatus.

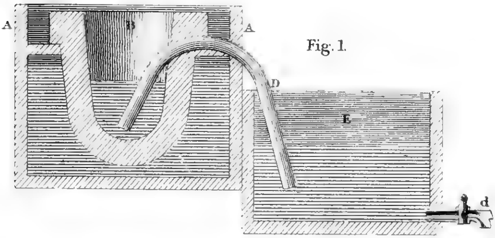


Fig. 1.

Mr. Smith's method of relieving a Horse which has fallen in the shaft of a loaded Cart.

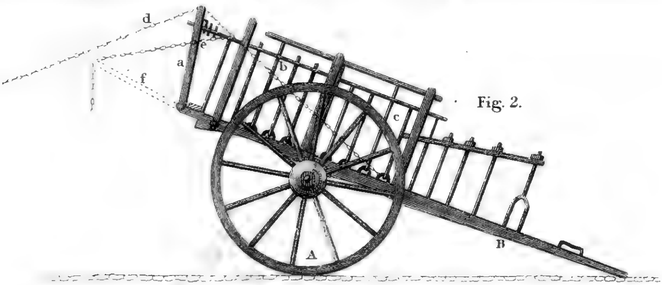


Fig. 2.

Mr. Taylor's Air Exhauster for Mines.

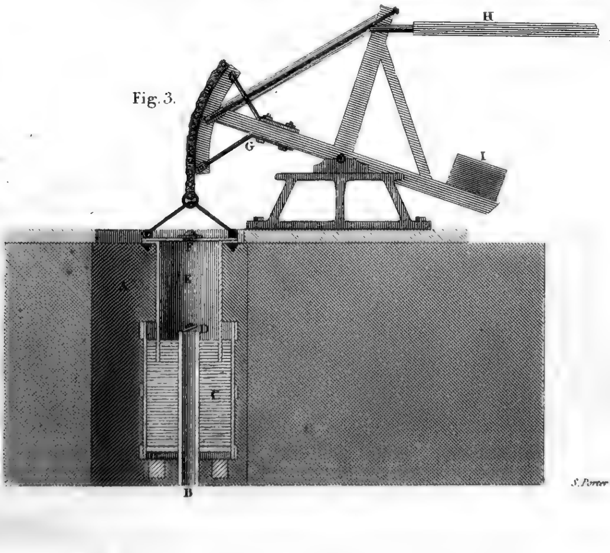


Fig. 3.

S. Porter sculp.

The Comet 1811.

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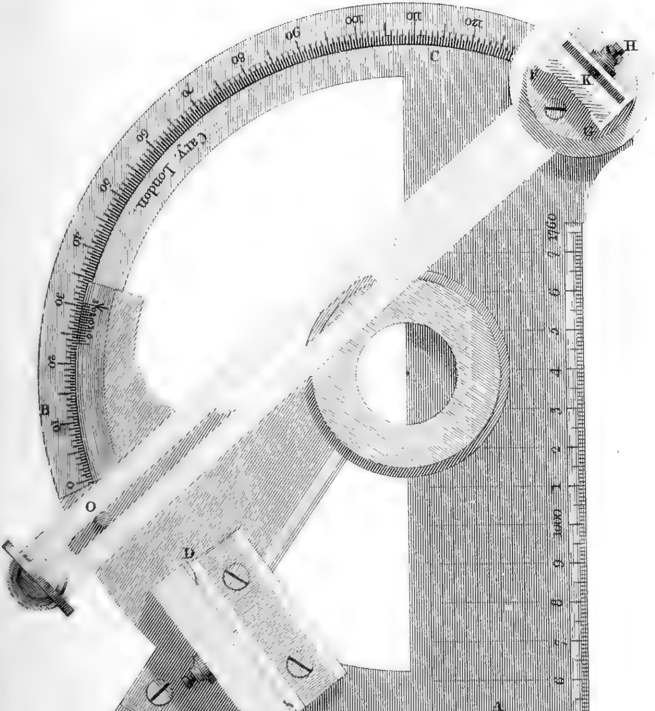
S. Porter sculp.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A
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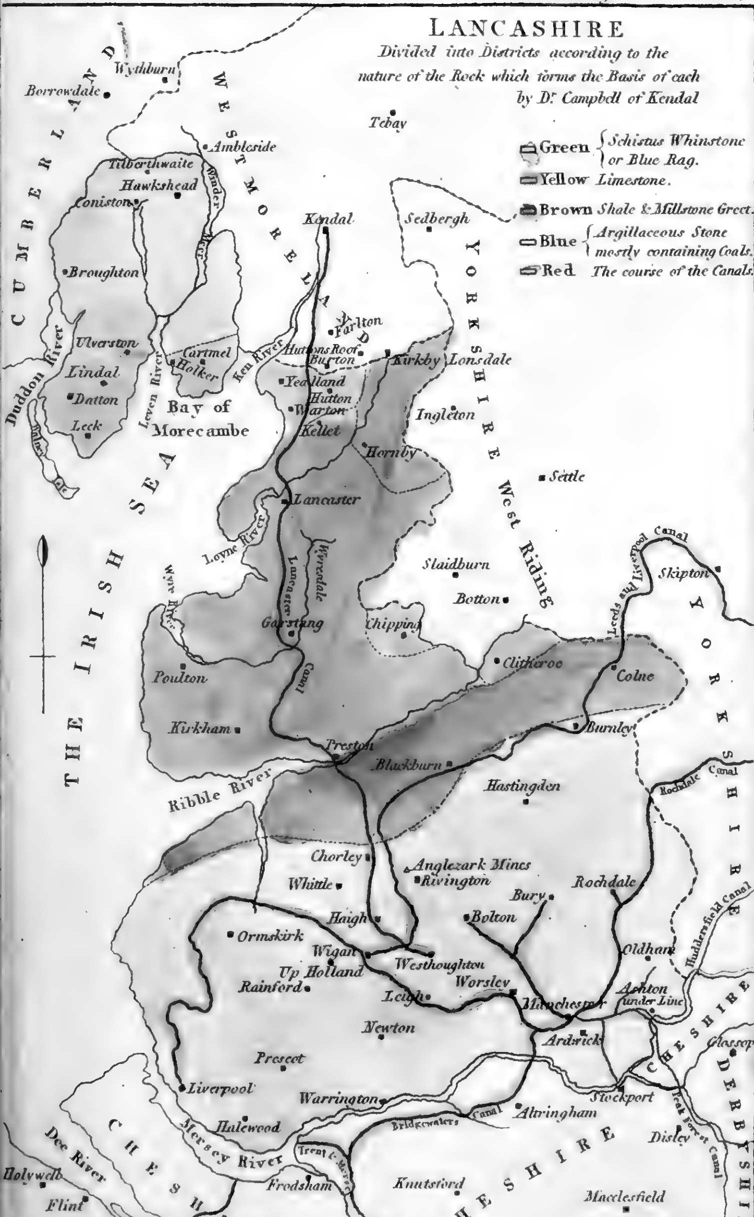
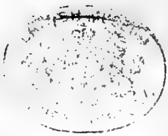
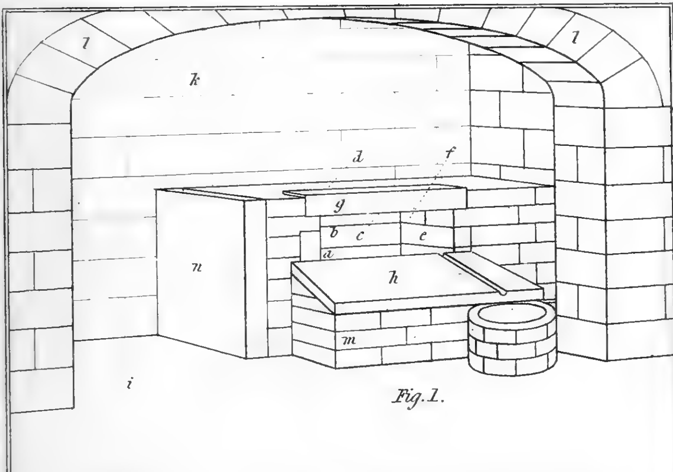

LANCASHIRE
Divided into Districts according to the nature of the Rock which forms the Basis of each
by Dr. Campbell of Kendal
Green { Schistus Whinstone or Blue Rag.
Yellow Limestone.
Brown Shale & Millstone Greet.
Blue { Argillaceous Stone mostly containing Coals.
Red The course of the Canals.
CUMBERLAND
WESTMORELAND
YORKSHIRE West Riding
YORKSHIRE
CHESHIRE
DERBYSHIRE
THE IRISH SEA
Bay of Morecambe
Wythburn
Borrowdale
Ambleside
Tebay
Tilberthwaite
Hawkshead
Coniston
Kendal
Sedbergh
Broughton
Farlton
Huttons Roof
Burton
Kirkby Lonsdale
Ulverston
Cartmel
Holker
Lindal
Dalton
Leck
Yealland
Hutton
Warton
Kellet
Ingleton
Hornby
Settle
Lancaster
Slaidburn
Botton
Garstang
Chipping
Clitheroe
Colne
Skipton
Poulton
Kirkham
Burnley
Preston
Blackburn
Hastingden
Chorley
Anglezark Mines
Rivington
Whittle
Rochdale
Bury
Bolton
Haigh
Ormskirk
Wigan
Westhoughton
Oldham
Up Holland
Rainford
Worsley
Leigh
Manchester
Ashton under Line
Newton
Ardwick
Glossop
Prescot
Liverpool
Warrington
Stockport
Altringham
Halewood
Disley
Holywell
Flint
Frodsham
Knutsford
Macclesfield
Chester
Northwich
Duddon River
Leven River
Winder Mere
Ken River
Loyne River
Wyre River
Wyresdale
Lancaster Canal
Canal
Ribble River
Mersey River
Dee River
Leeds and Liverpool Canal
Rochdale Canal
Huddersfield Canal
Peak Forest Canal
Bridgewaters Canal
Trent & Mersey
Canal
A Scale of Miles
0 1 2 3 4 5 6 7 8

S. Porter.





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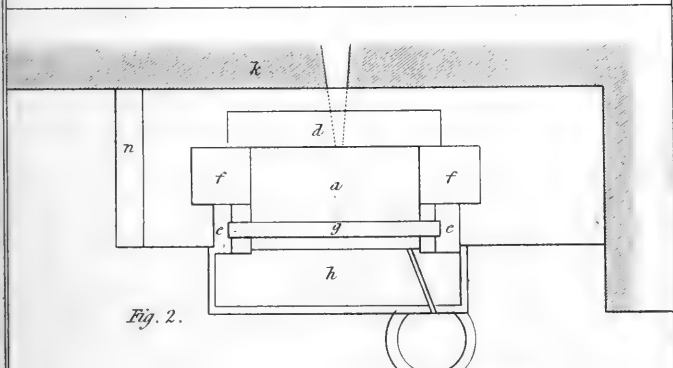

Fig. 2.

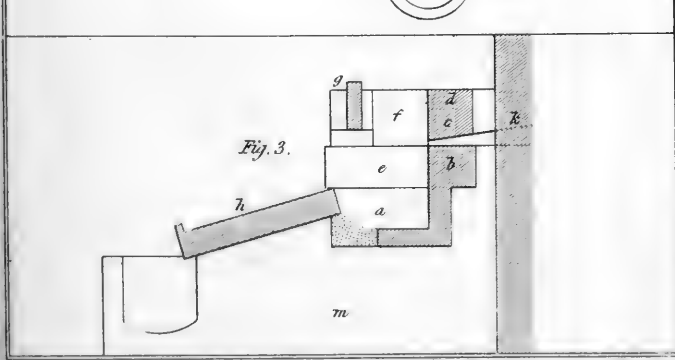

Fig. 3.

S. Porter sc.

Four rough Sections and a Plan of the Nailsea Coal-field, in Somersetshire, by Mr. Edwd. Martin, Colliery Surveyor.

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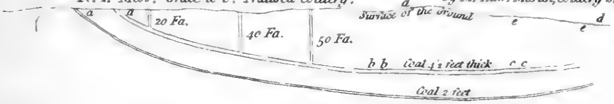
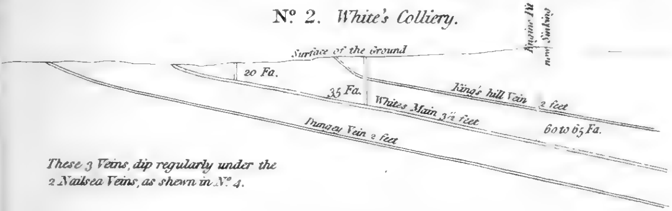
No. 1. Messrs. Grace & Co.s Nailsea Colliery.

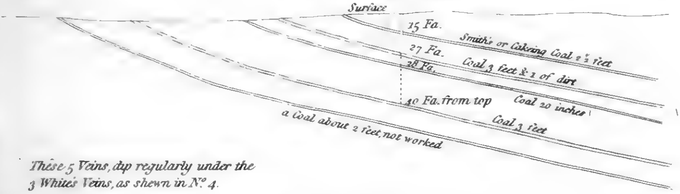


No. 2. White's Colliery.



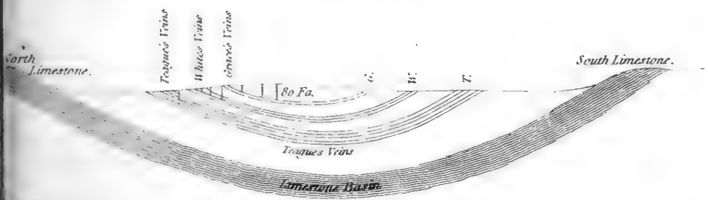
These 3 Veins, dip regularly under the 2 Nailsea Veins, as shewn in No. 4.

No. 3. Teague's Backwell Colliery.

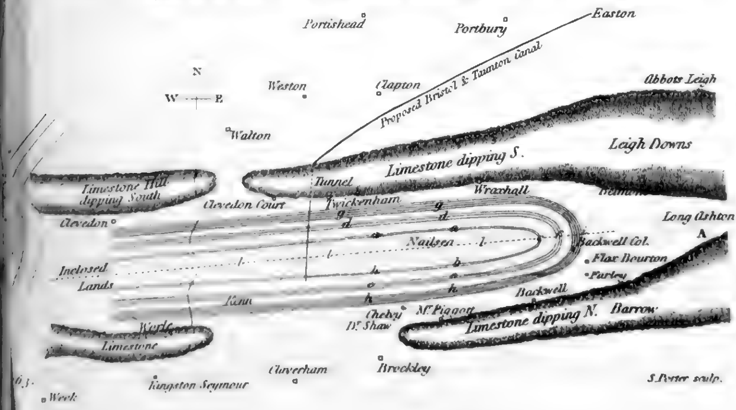


These 5 Veins, dip regularly under the 3 Whites Veins, as shewn in No. 4.

No. 4. A transverse Section, shewing how the above 10 Veins of Coal lie parallel under one another.



No. 5. A rough Plan of the N. and S. Limestone and the Nailsea Coal-bason between them.



S. Porter sculp.

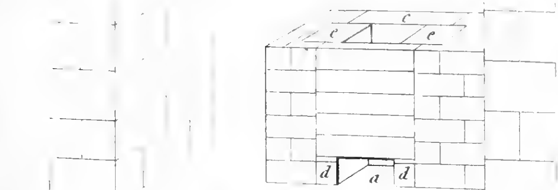


Fig. 1.

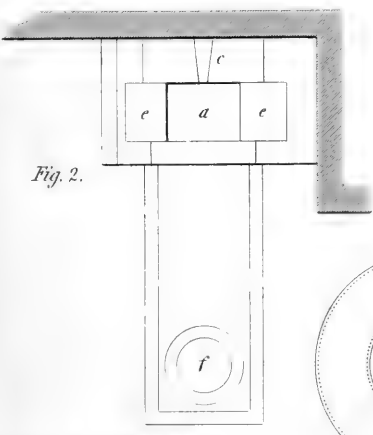
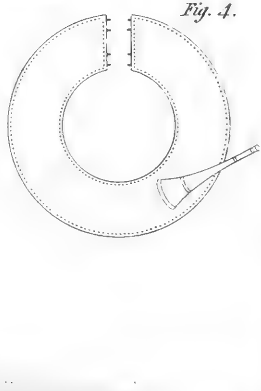


Fig. 2.

Fig. 4.



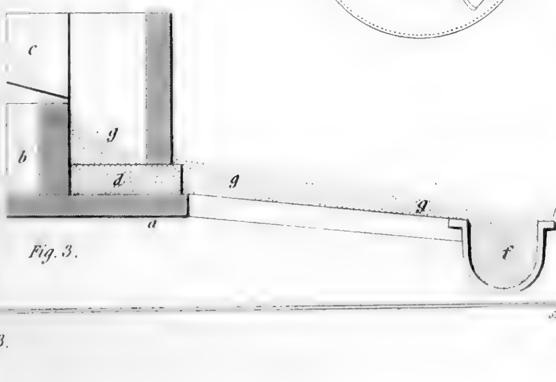
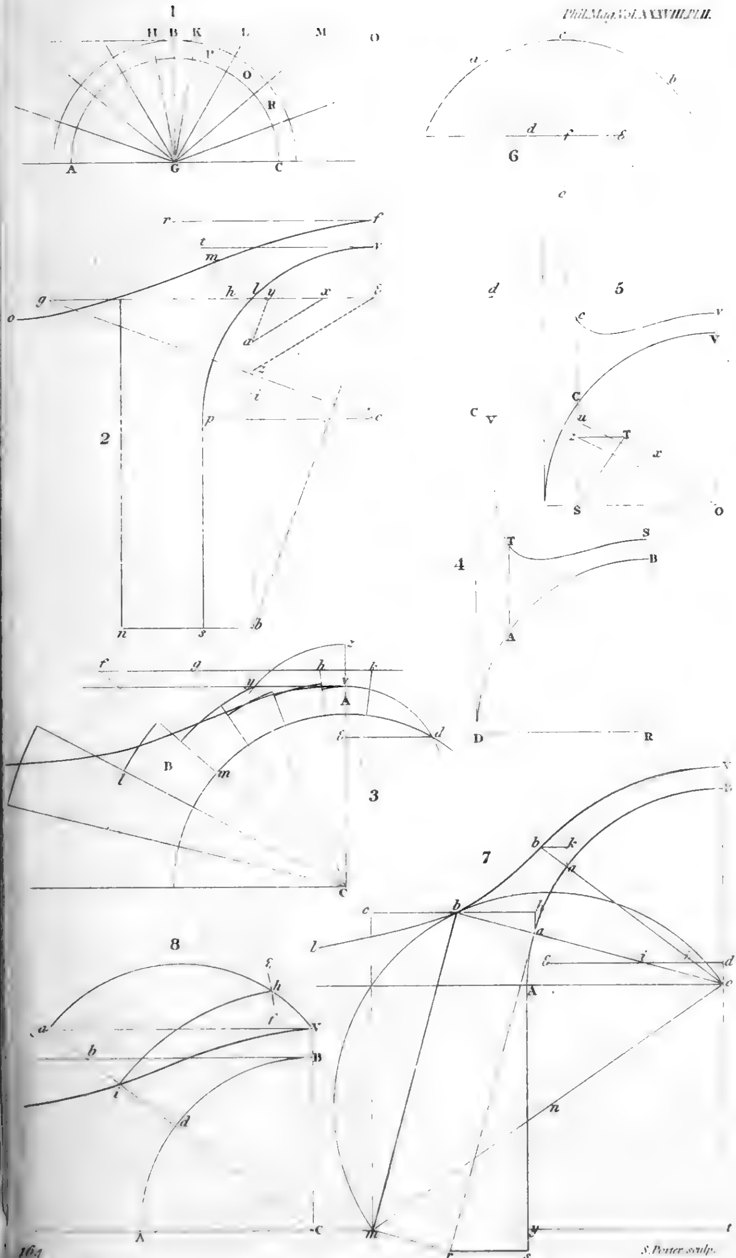


Fig. 3.



S. Porter sculp.

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*** Communications for this Work, addressed to the Editor, at No. 1, Carey-street, Lincoln's Inn, will meet with every attention.

R. TAYLOR AND CO. PRINTERS, SHOE LANE, FLEET STREET, LONDON.

VOL. 38. Philosophical Magazine. SEPTEMBER 1811

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