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THE

PHILOSOPHICAL MAGAZINE

AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

GEOLOGY,

AGRICULTURE,

MANUFACTURES AND COMMERCE.

BY ALEXANDER TILLOCH,

M.R.I.A. M.G.S. M.R.A.S. MUNICH, F.S.A. EDIN. AND PERTH, &c.

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

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PHILOSOPHICAL MAGAZINE AND JOURNAL.

I. *Experiments made upon the hard Water at Black Rock near Cork, with a view to render the Waters of Limestone Districts more applicable to domestic Uses.* By Edmund Davy, *Professor of Chemistry and Secretary to the Cork Institution.*

To Mr. Tilloch.

There is a striking similarity in the appearance and in the composition of the great arrangements of Nature in different countries, and it extends to the waters, as well as to the atmosphere and the solid strata of the earth. The chemical constitution of water is uniformly the same in every part of the globe, and all the variable properties it exhibits in different situations, whether evanescent or permanent, are principally owing to a diversity of soil. All natural waters contain a variable quantity of foreign ingredients, which they derive from the strata through which they flow, or the atmosphere by which they are surrounded. The purest springs usually rise in beds of gravel, or in siliceous or argillaceous rocks, and they contain, for the most part, only a minute portion of saline matter, which is principally common salt. The waters of limestone or calcareous districts generally contain a much larger quantity of solid ingredients. The substances they frequently hold in solution are carbonate and sulphate of lime, and these earthy salts occasion that peculiar quality in waters, commonly known by the name of *hardness*. Hard waters, as is well known, do not readily dissolve soap, or form a good lather with it; on the contrary, they partially decompose it, and a light flocculent substance is produced, which is insoluble in water. Hence such waters, in their common state, are unfit for washing, and for other domestic purposes; and they are conceived to be very inferior to soft waters, for making vegetable infusions, such as tea, coffee, &c.

The waters in the *limestone* strata, in the county of Cork, so far as I can learn, and especially those situated in such strata on the south side of the river Lee near Cork, belong to the class of hard

hard waters; whilst the waters that occur in the grauwacké, or the siliceous and argillaceous rocks, particularly those on the north side of the river, are perfectly soft, more pellucid, and better tasted.

An intelligent friend who resides at Black Rock, lately informed me that the want of soft water was much felt in that neighbourhood; and that a sufficient supply, especially in dry seasons, could only be obtained with difficulty and at a considerable expense. To obviate the inconveniences which in those respects were felt by a family of my acquaintance, I recommended the use of a little potash or soda in the water, from an idea that its hardness arose from the presence of an earthy salt, and that the addition of an alkali would separate the earthy matter, and render the water soft. This suggestion was immediately adopted, and potash is still employed, with perfect success, to produce the desired effect.

It occurred to me, that a chemical examination of the water at Black Rock might probably lead to some simple modes of obviating its hardness, and making it more generally applicable to domestic uses. With these views I undertook some experiments upon it; and I am not without hopes that the result of my investigation will be of some utility, not only to the inhabitants of that district, but also to those in other parts of the country where limestone prevails, and imparts to the waters that flow through it, those peculiarities that distinguish the water at Black Rock.

The Black Rock water I examined, was procured at different times from a deep well in the village of Ballintemple, about a mile and half from Cork, and two hundred paces from the river. The village is situated on secondary limestone. I do not know the extent of the limestone district in this part of the county, but I understand it reaches in an easterly and westerly direction, with little or no interruption, a distance of many leagues, and abounds, in many places at least, with a variety of organic remains, some of which are rather novel and curious.

Though I have not made an accurate chemical analysis of the water in question, I presume my experiments will be found sufficiently minute for practical purposes. I purpose briefly to notice the effects of different chemical agents upon the water;—to point out the causes of its hardness;—and to deduce from facts and experiments, some simple, and I trust efficient modes of rendering it more subservient to the common purposes of life.

1. As soon as the water was taken from the well, it was put into bottles or stone jars, well corked, and usually examined immediately on its arrival in Cork. The water, on being drunk fresh from the pump, is said to be rather brisk and pleasant, though

though there are some who do not think its flavour agreeable. After it has been exposed to the air a little time, it acquires a degree of flatness, and produces in the mouth an unpleasant taste, as though it contained putrid matter. The insipid and disagreeable flavour of the water after exposure to the atmosphere, seems to be owing to a loss of air from a diminution of atmospheric pressure; for no sooner is the water raised from the well, than small globules of air are disengaged from it, and this effect continues for some time.

2. The water gradually changes litmus paper, and gives to its blue colour partial tints of red: but this change does not take place after the water has been boiled. The mineral acids, as the muriatic and sulphuric, disengage from the water minute globules of gas; indeed the water by simple exposure to the air evolves gas; and this effect is promoted by the addition of an acid, and destroyed by that of an alkali. Limewater in small quantity occasions an immediate cloudiness in the water, which soon disappears, and the water resumes its former transparency; but when added in large proportion, there is a permanent deposition of carbonate of lime. Even after the water has boiled, limewater occasions a milkiness in it; but this effect is not produced after the boiling has been kept up for a quarter of an hour. These results prove that the water contains an uncombined acid; that this acid is the carbonic, and that continued boiling is necessary to expel it.

3. After the water had been exposed in a large shallow glass vessel to a warm atmosphere, upon the top of the Observatory of the Institution, (during the greater part of two fine summer days in June,) an extremely thin and scarcely perceptible earthy crust began to form on its surface. This crust dissolved with effervescence in diluted muriatic acid, and its earthy matter was precipitated by the fixed alkalies in their caustic and carbonated state, but not by ammonia. Hence it was carbonate of lime originally held in solution, as would seem from the foregoing statements, by carbonic acid gas. The separation of carbonate of lime in this case is to be referred to the diminished solvent power of the water, arising from the loss of a portion of carbonic acid, and of water, by evaporation. The water, after the exposure mentioned, still rendered limewater milky, and afforded with alkaline substances, a precipitate having similar properties to the earthy crust already noticed. And these circumstances seem to prove that the water still contained carbonic acid gas and carbonate of lime.

4. The water immediately decomposes an aqueous solution of soap, and a white flocculent substance swims upon the surface. Hence the water in its common state is unfit for washing. If

 a solution

a solution of fixed alkali, in its common caustic or carbonated state, is added to the water, a cloudiness is produced, and there is a gradual deposition of earthy matter, which on examination proves to be calcareous. The water in consequence of such treatment becomes soft, does not decompose soap, and may be used for washing and other domestic purposes.

5. If the water is boiled, and the boiling continued for fifteen or twenty minutes, a quantity of air is disengaged, which renders limewater milky, and there is a deposition of earthy matter, which exhibits all the characters of carbonate of lime. After this treatment the water becomes soft, and may be employed for washing and other culinary purposes: limewater is now incapable of impairng its transparency, and it is scarcely affected by the pure or carbonated alkalies. Hence, it seems, the carbonic acid gas and carbonate of lime contained in the water may be separated by continued boiling.

6. The water when treated with nitrate of silver becomes cloudy, indicating the presence of a portion of common salt. Pure ammonia added to the water separates a very minute quantity of earthy matter, and indicates a little magnesian salt, which is probably the muriate. Nitrate of barytes produces a very slight effect, and shows a trace of alkaline or earthy sulphate. Neither prussiate of potash nor solution of oak bark occasions any change in the water after several hours; from whence the absence of iron in it may be inferred.

From the foregoing statements, which are founded upon experiments carefully made and repeated, I venture to conclude, that the Black Rock water contains an excess of carbonic acid gas, which holds in solution a portion of carbonate of lime. This earthy salt gives to the water its peculiar characters, and especially its *hardness*, which is its distinguishing quality. The water certainly contains other foreign ingredients, such as muriate of soda, and a little magnesian salt; but these substances (common to almost every water) are scarcely worthy of notice, because they exist in quantities too small to be sensible to the taste, or to produce any medicinal effect. From some comparative experiments I have recently made upon the pipe-water commonly drunk in Cork, and the Black Rock water, there seems to be a great similarity, not only in the foreign ingredients common to both, but also in the actual quantities they contain; with the exception of the carbonic gas and carbonate of lime peculiar to the Black Rock water. And if the pipe-water were impregnated with an excess of carbonic gas and a portion of limewater, an artificial water would be formed similar to the natural water at Black Rock.

The preceding experiments seem to lead to two simple modes of of improving the Black Rock water, by which it may be rendered soft, and more generally applicable to the purposes of life. One is, by the use of an alkaline substance;—the other, by continued boiling for at least a quarter of an hour. And it may be proper to make a few remarks upon each of those methods.

1. It is well known to chemists, that waters naturally *hard*, from whatever cause their hardness may arise, are rendered soft by an alkali; and the fixed alkalies, in their caustic or carbonated state, afford a *general* remedy, applicable to every particular case of the kind, that can occur. Boiling, on the contrary, is only a *particular* remedy for *hardness* in waters, in cases when it arises, as in the water at Black Rock, from carbonate of lime held in solution by carbonic acid gas. The acid being volatile, and its combination with the earthy salt retained by a feeble affinity, their union is destroyed by continued boiling; the earthy salt (the cause of the *hardness* in the water) being precipitated, and the acid expelled. In other cases, where the quality of *hardness* in waters is occasioned by the presence of sulphate or muriate of lime, &c. no changes can be produced in such waters by *boiling*, and the agency of an alkali is indispensably necessary to render them soft. The addition of any alkaline substance to the Black Rock water, such as potash or soda and their carbonates; the substances known in commerce by the names of kelp, barilla, pearlash, &c. will all neutralize the fixed air in the water, precipitate its earthy matter, and render it soft. I found about ten grains of pure dry soda sufficient to render a gallon of the water perfectly soft; but the use of this substance is precluded, from its expense, and the difficulty of procuring it, in this part of the country. I conceive about twenty grains of the common potash or soda of commerce would answer the same purpose. As any of the alkaline substances before enumerated may be used with success, a few practical trials would be sufficient to enable any one to decide upon the alkali that is the most efficient and œconomical. In cases when the water is to be rendered soft by an alkali, for the purpose of washing, no danger can be apprehended from a slight excess of alkali; on the contrary, it would be an advantage; for the alkali, as is well known, is the efficient cleansing principle in soap. It would, I think, be advisable to add the alkali to the water previous to its being heated; and to stir it until it is dissolved. A friend of mine was informed that limewater might be used for improving the water by separating the fixed air it contains. Lime is almost the only common substance possessing alkaline properties, that cannot be employed for such a purpose; the existence of the smallest quantity of it in a water is sufficient to give it a certain degree of hardness. It is to the salts of lime, particularly

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the sulphate and carbonate, that the greater number of *hard* waters owe this peculiar quality.

2. It is to be remembered that simply boiling the Black Rock water is not sufficient to render it soft. To produce this effect, the boiling must be continued for fifteen or twenty minutes. And this fact serves to explain a seeming paradox. It is generally admitted that hard waters do not draw tea so well as soft waters: yet the hard water at Black Rock is said to answer extremely well for this purpose. But in most cases, it may be observed, the water is unintentionally allowed to boil at least fifteen or twenty minutes previous to the making of the tea; and thus the hard water, without any design, is rendered soft. I have however heard occasional complaints of the tea not drawing well, or much worse than usual. In such instances, it seems reasonable to suppose that the water really was hard, having been merely brought to boil, and immediately poured upon the tea. Hence the expediency of allowing the Black Rock water to boil for some time previous to making tea with it.

From the experiments I have made upon the Black Rock water, I am of opinion that, after it has been boiled the requisite time mentioned, it is well adapted for every domestic and manufacturing purpose. It loses its nauseous flavour, and almost the whole of its earthy matter, and becomes perfectly well tasted. Certainly, boiled water is rather insipid for drinking, when compared with spring water, owing, it is conceived, to the separation of air. But this defect might be easily remedied:—If, for example, the boiled water, when cold, were made to ooze slowly through a filtering vessel, or passed through a large stone or a piece of wood pierced with a number of small holes, or even through a very fine sieve, the water by such exposure would absorb pure atmospheric air, lose its insipidity, and would afford to water-drinkers a pleasant and wholesome beverage.

Cork Institution, June 16, 1818.

II. *Account of a North American Quadruped supposed to belong to the Genus Ovis; by* George Ord*.

Rocky-Mountain Sheep—Ovis montana.

In the Journal of Lewis and Clark, there is an account of a quadruped which appears to have not excited that attention which it merits. The following extracts are made from the above-mentioned works: "Saw the skin of a mountain sheep, which

* From Journal of the Academy of Natural Sciences of Philadelphia.

which the Indians say lives among the rocks in the mountains: the skin was covered with white hair, the wool long, thick and coarse, with long coarse hair on the top of the neck and the back, resembling somewhat the bristles of a goat." Vol. ii. p. 49.

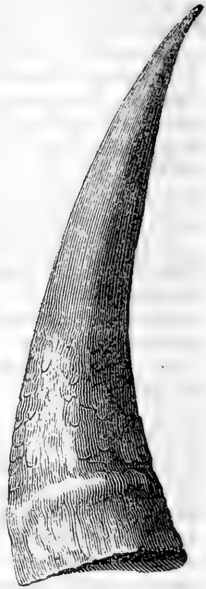
"The sheep is found in many places, but mostly in the timbered parts of the rocky mountains. They live in greater numbers on the chain of mountains forming the commencement of the woody country on the coast, and passing the Columbia between the falls and rapids." Vol. ii. p. 169.

The latter passage was written while our travellers wintered at the mouth of the Columbia river. But on their return, at Brant Island, an Indian "offered two sheep skins for sale: one, which was the skin of a full-grown sheep, was as large as that of a common deer; the second was smaller, and the head part, with the horns remaining, was made into a cap, and highly prized as an ornament by the owner. The Clahelellahs informed us that the sheep are very abundant on the heights, and among the cliffs, of the adjacent mountains; and that these two had been lately killed out of a herd of thirty-six, at no great distance from the village." Vol. ii. p. 233.

"The Indians assert, that there are great numbers of the white buffalo, or mountain sheep, on the snowy heights of the mountains west of Clark's river. They generally inhabit the rocky and most inaccessible parts of the mountain, but, as they are not fleet, are easily killed by the hunters." Vol. ii. p. 331.

In the above passages, we are made acquainted with the important fact, that besides the Argali or big-horned sheep, we have another species in North America of the genus Ovis. The smaller of the two skins, which the Indian offered to sale at Brant Island, was purchased by Capt. Lewis, and was presented by him to the museum of Philadelphia. It is undoubtedly the skin of a young animal: it measures three feet from the insertion of the tail to the neck, its breadth is twenty-six inches; the tail is short, but it was probably not skinned to the end; along the back there runs a ridge of coarse hair, about three inches in length, and bristled up in the manner of that of the common goat; this ridge is continued up the neck, forming a kind of mane, and is thicker, coarser, and longer there than that of the back; the whole of the skin is closely covered with short wool, of an extreme fineness, surpassing in this quality that of any breed with which I am acquainted, not excepting the wool of the Merino lamb—a coat of hair conceals this wool, but on dividing the former with the hands, the latter lies so thick that the hairs are scarcely visible: the ears are narrow, and taper to a point, they are nearly four inches long; the whole is white; the

the horns appear to have stood on the top of the head, somewhat in the manner of those of a goat, or of those on the figure of Shaw's Pigmy Antelope, Gen. Zool. vol. ii. plate 188, and vignette on the title-page. But one * horn is now attached to the skin, and that measures three inches and three quarters in length, on the fore part; it is slightly recurved, cylindrical and acuminated, its base is somewhat tumid, and, with its lower half, is scabrous, its upper part smooth, obsoletely striated, and of a black colour.



A cut of this horn, of the size of nature, accompanies this account, by which figure it will be evident to the naturalists, that the above described sheep is a distinct species. It is true that the animal was young, and we have no positive evidence that when full-grown or old the horns do not increase in size, so as to resemble those of some well-known species or varieties of the genus. One of Lewis and Clark's men informed them that he had seen the animal in the Black Hills, and that the horns were *lunated* like those of the domestic sheep. The Indians asserted that the horns were *erect* and *pointed*. The latter account is the more probable, as it has been remarked by travellers, that in describing those natural productions with which they are conversant, our Indians seldom deviate from the truth.

We would incite the attention of our citizens to this important discovery; for although the Spanish missionaries in 1697 made mention of this sheep, and it is again noticed in Venegas' History of California†, yet these accounts were discredited. It is Captain Lewis to whom belongs the honour of having been the first to assure his countrymen, by the exhibition of a genuine specimen, that the animal does exist. How subservient to the wants and pleasures of mankind it may be rendered by domestication, we cannot at present declare; but there is room for conjecture, that the introduction of this new species of a race of quadrupeds immemorially ranked among the most valuable of the gifts of the

* The other horn is in Peale's Museum.

† Vol. i. p. 36, English translation, London, 1759.

Creator,

Creator, will confer a lasting benefit upon the agricultural and manufacturing interests of the community.

Since writing the foregoing, I have seen the three first volumes of the *Nouveau Dictionnaire d'Histoire Naturelle,* which work is now publishing in Paris; and in the article *Antelope,* I find a description of an American quadruped, which is in the collection of the Linnean Society of London. This description appears to have been extracted from a *memoire,* read before the Philomatique Society of Paris, by M. de Blainville, wherein the author proposes a new arrangement of the ruminants with hollow and persistent horns, and a subdivision of the *Genus Antilope;* and classes the above animal under the name of *Rupicapra americana.*—(*Bulletin de la Societé Philomatique,* 1816, p. 80.) As I have not the satisfaction of seeing the Bulletin, I must be content with the information conveyed in the article in the *Nouveau Dictionnaire.* The specimen is said to be of the bigness of a middling sized goat; the body is entirely covered with long *pendent* hair, silky and totally white, but not curled; the head is elongated, without a muzzle or naked part; the ears of a middling size; the forehead not protuberant; the horns are short, tolerably thick, black, slightly annulated, they are round, almost straight, bent backwards, and terminated in a blunt point (*pointe mousse*); the legs are short, stout, and supported on short and thick hoofs; the tail is hardly perceptible, perhaps on account of the length of the hair. M. de Blainville inclined to the opinion that this animal is the same as the Pudu of Molina, Shaw's Gen. Zool. vol. ii. p. 392.

It is probable that the specimen belonging to the Linnean Society is of the same species as that brought by Captain Lewis; and it is further probable that M. de Blainville was not permitted to examine his subject as closely as was requisite, otherwise the important circumstance of the thick coat of wool, beneath the outer covering of straight hair, would not have escaped his attention. As to the horns being obtuse, this may have arisen from an accident, or some other cause.

It is much to be wished that some traveller would bring a living specimen of this singular quadruped, or at least a dead specimen, in such a state as should enable the naturalist to determine, with precision, its characters. From the information derived from Captain Lewis, and from the descriptions above, we cannot, with propriety, arrange this animal with the antelopes; and if it should not prove to be a true *Ovis,* it will, probably, constitute a new genus, and take its station, in the systems, between the sheep and the goat.

III. *Account of the Gold and Silver Mines of Hungary.* By RICHARD BRIGHT, *M.D.**

THE early history of the Hungarian mines is involved in some obscurity, but it is probable that the Saxons or Germans who came to Hungary about the twelfth century first explored these mineral treasures. The Emperor Charles Robert founded Schmölnitz, and brought mining to some perfection. This state of prosperity seems to have ceased in some degree at the beginning of the sixteenth century; but Ferdinand the First, and a succesion of kings who followed him, improved it greatly. During the period of their greatest prosperity, it is said that the mines of Hungary have given occupation to above 30,000 persons, of whom above 10,000 are reckoned in the districts of Schemnitz and Kremnitz.

Schemnitz.—In order to give a general view of the mining district of Schemnitz, it must be mentioned that the whole of the mountain mass is a species of claystone porphyry, here called the *saxum metalliferum*; the mountain caps being pretty generally of *grunstein*, a species of basaltic rock.

The mineral district is of considerable extent. I have no exact information on this subject, but suppose, from the marks which were pointed out to me as showing the limits of the metallic country, that the whole might be included in an extent of five or six square miles.

There are five principal mineral veins (or courses) which run almost parallel to each other nearly east and west, each from ten to twenty fathoms in thickness, at the distance of from 60 to 300 or 400 fathoms from each other, and are connected by various small branches; they have been followed to between 200 and 300 fathoms in depth. When, however, we speak of the great veins being ten or twelve fathoms in thickness, it must not be supposed that the vein of ore extends to this width;—all that is meant is, that to this breadth the nature of the rock varies from that of the mass of the mountain, and in this part feldspar generally prevails over all the other component parts. This mineral course or vein is throughout intersected by metallic veins of various sizes, some from two to four inches thick, of rich ore, with quartz, calcareous spar, &c., and thence branching off in small collateral veins sometimes hardly larger than a thread, and scarcely affording a trace of the ore. Every little appearance is however followed by the miner with hope, though his pursuit often ends in disappointment. It is but seldom that these indications lead him beyond what are called the walls of the great vein or gangue.

* From Travels from Vienna through Lower Hungary.

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In these extensive courses there are twelve royal mines, which extend over a space of about 2200 toises by 900, or nearly 1200 English acres, besides a number belonging to private individuals, who are obliged to dispose of all the ores they obtain to the royal smelting works at a fixed rate. The whole of these mines have a communication with each other at what is called the Emperor Francis's adit or level, at the depth of 180 toises, or nearly 200 fathoms, which is the lowest point at which they have hitherto been able to give the water a free egress:—to this therefore they are obliged to raise all which collects in the deeper workings. The whole length of this subterraneous canal from the valley into which it opens, is said to be above twelve miles. They have for the last thirty years been at work upon a new water-level at a considerably greater depth than this, to be called after the Emperor Joseph. It opens into the river Gran, and is supposed to be the lowest possible level at which the water can be drawn off. Although it is as yet far from being finished, some of the mines have already experienced benefit from it.

Owing to several causes, the returns from these mines are by no means so regular as they formerly were. This arises partly from the actual exhaustion of the minerals, and in part from the financial circumstances of Austria, which are supposed to render it unable to carry on the works with the former ardour, and more particularly prevent it from paying the private mining companies with sufficient liberality to encourage their exertions.

According to the usual stipulations, these private adventurers are to deliver their ores in a state fit for the smelting furnaces, and to receive 19 florins and $\frac{12}{20}$ (equal to about 2*l.* 8*s.* sterl.) for every mark (eight ounces) of pure silver, which is in fact worth twenty-four florins of the silver currency of Austria; but under the present circumstances (April 1815) when the paper currency is depreciated to about one-fourth in value of the silver currency, these companies are paid $9\frac{16}{20}$ florins in silver, and the other moiety in paper, which reduces their reimbursement to less than twelve florins in silver currency per mark. The consequence is, that they do no more work than is just sufficient to preserve an undisputed right to their mines; for, by the mining laws, any person who discovers ore on a part of the mountains not yet appropriated as a mine, may work it for his own advantage; but if he fail to dig a certain small quantity every fourteen days he loses his right, and any other person may possess himself of it.

The result actually is, that where before the Turkish and French war there were nearly 100,000 marks, that is 50,000 pounds (of sixteen ounces) weight of silver brought in the course of a year into the mint at Kremnitz, the average quantity does

not

not now exceed one-third part of that amount, and the whole weight of gold seldom exceeds 100 marks per month. Of this silver, the royal mines in the district of Schemnitz yield about 25,000 marks annually, and about 300 marks of gold; the rest is collected from the royal mines about Kremnitz, and the adventures of private companies.

Mine of Windshacht.—The principal objects of curiosity in the mine of Windshacht being the machinery, I was put under the care of the Ober Kemst Meister or chief director of the machines; and being dressed in a miner's jacket, overalls and cap, and a leathern apron, we proceeded to the mouth of the shaft, where is erected the *Bremse* machine as it is here named, by which the ore is drawn up, and all materials for constructions or repairs let down into the mine. This machine consists of a double overshot water-wheel, on which the water falls from a reservoir supplied by pipes from the hill which lies above it; and as the water is made to fall upon one side or the other of the wheel, it moves either in one direction or the contrary. The axle of the water-wheel is connected to a cylindric beam supported at each end by masonry, round which is constructed a gigantic reel, upon which two cables are coiled in contrary directions: thus the communication from the surface to the bottom of the pit is carried on, the one winding and drawing up, whilst the other unwinds and lets down. To regulate the motion of this massive engine, a fly-wheel of considerable diameter is connected with the axle near to the end at which the water acts; and it is by two beams, the one above and the other beneath, which can be brought in a moment to press upon this fly, that the motion of the whole machine is checked and brought under command. The management of the regulator, as well as the care of directing the water upon one side or other of the water-wheel, is intrusted to one person, who standing at the mouth of the mine directs the whole with ease and certainty.

My next object was the machinery by which the water is lifted from the deeper parts of the mines to the height of the Emperor Francis's level: we therefore descended that part of the Leopold schacht which is appropriated to it. The shaft we went down was completely perpendicular; and the whole of this descent was performed by means of ladders; each ladder about ten steps in length. Having descended the first, we came to a platform of boards, on the opposite end of which was a trap-door which opened upon the second ladder; and having descended ten steps more, we arrived at another platform, and so on. In this way we went down seventy-two klafters (fathoms), during the whole of which we were close to the machinery, in a constant noise, and amidst the continued dropping of water, which soon found its way

way through all our clothes. I now employed myself in endeavouring to comprehend the parts and the mode of action of this great instrument; and then having attended to the manner of working of a few parties of miners, we proposed to reascend, which we did by another shaft appropriated to the officers of the mine.

This shaft was perfectly dry, and strongly cased with a framework of timber from the top to the bottom. The necessary supply of timber is a source of prodigious expense in the Hungarian mines. The rock-stone is of a nature so liable to decompose, that they cannot employ it in walling these perpendicular shafts; and the wood-work, however strong, seldom lasts above fifteen or twenty years, and in parts where the current of air is not good, is destroyed in a much shorter time.

As we approached the surface the cold became very severe, and the sides of the pit were covered with ice. It is through this shaft that the current of fresh air passes into the mine; and I was told that the intensity of the cold was sometimes such as scarcely to be borne.

The miners are usually divided into three parties, each remaining under ground eight hours at one time: those who have the care of the machinery remain twelve hours. The whole number of persons employed in this mine is about 400.

The machine which I had been viewing, and which was first constructed at Schemnitz about the year 1749 by the chief engineer Höll, was before the improvement of the steam-engine considered the most valuable for raising water out of mines which had ever been brought into use.

It is worked by water exerting its force to establish its equilibrium in an inverted siphon, and acting upon a moveable piston by its hydrostatic pressure. To apply it, it is necessary to have the command of water considerably above the engine; and this is effected at Schemnitz by forming strong embankments in high mountain valleys, and thus creating large reservoirs in which the winter rains and melted snows collect. Many of them are seen in the approach to the town. From these ponds the water is conducted by small canals, and falls through water-tight cast-iron pipes erected perpendicularly in the mine shaft. When it has fallen a certain depth (in this case about forty-five fathoms) it is checked in its progress downwards, and forced, by the weight of its whole column in the descending pipe, into the bottom of a perpendicular cylinder of considerable diameter, in which it raises a water-tight piston. As the piston ascends, it carries with it two bars of wood, moving perpendicularly on the outside of the cylinder, to which are attached four or more pump-rods, each working a pump at a different level; the first raising the water from the bottom to a certain height, whence it is raised one

stage

stage higher by a second, and so on stage by stage, to the required elevation on the level of the main adit. At the moment when the piston has been forced up to a given point, it acts by a simple collateral communication upon the stop-cock (which had been turned so as to suffer the water to enter the cylinder), and checks its progress downwards; adjusts it again so as to cut off all communication with the descending pipe; and opens a passage, through which the water contained in the cylinder is at once discharged. The piston of course descends, carrying down with it the two wooden bars connected with the pump-rods, and in the act of falling, by means of the same collateral mechanism, closes the passage through which the water was discharged from the cylinder, and, opening the communication between the cylinder and the descending column of water in the pipe, permits it to enter, and by its pressure again raise the piston. In this way the simple piece of machinery maintains itself in constant and powerful action. The ease and regularity of working is aided by a balance-beam connected by a chain with the head of the large piston and pump-rods.

The machine is set in motion, or stopped, by turning a cock fixed in the descending pipe, by which the current of water is either permitted to pass into the machine, or its course entirely impeded. The handle of the cock is always within reach of the attending engineer. The quantity of water thrown into the cylinder is likewise regulated by it, and consequently the velocity with which the pump-rods act.

The water discharged from this engine is conveyed further into the mine, where it again serves to give motion to other machinery, until, having reached the level of the Emperor Francis's adit, it there escapes with the water which it had been the means of raising from the deepest workings.

There are now three of these engines employed to keep the mines free from water: they have not however been found at all times sufficient, and a fourth is now constructing. The whole quantity of water raised by the three in twenty-four hours, is 49,365 eimers, each eimer containing 60·811 Paris pints, or about sixteen gallons.

The pipes containing the long column of descending water are cast in lengths of six or seven feet. They are not very firmly joined together; the joints being secured only by broad iron rings, fixed over the junction of each length by wooden wedges, which in case of any unusual pressure of the water are thrown out, and the pipes themselves prevented from bursting; which, if they were fastened together by flanches and screws, might sometimes happen.

Before leaving Windschacht, I was taken to the engineer's office,

office, where numerous plans and sections of the mining district were laid before me. The whole country is intersected, at different levels, by the galleries of mines, forming one stupendous subterranean labyrinth, so well understood, however, that the exact limit of each adventurer's right is known, and the moment the ore has been traced to that boundary the workman stops; nor may he proceed until a compact has been made with the neighbouring proprietor. Besides general maps, there were particular plans of each mine, with the most accurate surveys of all its parts.

Stephani-schacht.—We descended into this mine in the same manner as we had done into that of Windschacht. When arrived at a certain depth, we turned into a gallery dark and dismal, and pursued for several hours its various windings. The workmen, with parties of whom we fell in at intervals, are divided into companies of eight each, and are paid not only according to the quantity but the quality of the ore they collect, so that they are themselves interested in the research; as may indeed be easily perceived by the different tone of voice in which they speak when they have hit upon a good or a bad vein. When they find any pieces particularly rich, of which they are very accurate judges, they lay them aside in a bag, that they may not be lost in the general mass. The ore, when dug out, is placed in a small oblong box or wheelbarrow, in which it is conveyed with wonderful rapidity and skill along narrow planks to the shaft, and is there laden into the large buckets of the machine, by which it is drawn up to the surface.

The rock here is clay porphyry passing almost into grunstein. It is much harder than at Windschacht; and is often firm enough to become a building stone. The timber used in this mine is consequently less, and the shafts by which we ascended and descended were only secured by a kind of strong trellis-work. In some parts of the great vein the feldspar was so predominant as to render it almost white, interspersed with distinct crystals of hornblende; but the vein soon passed again so completely into the nature of the surrounding rock, that it was difficult to say where the one ended or the other began. At the depth of the Emperor Francis's level, which is here above seventy fathoms, my conductor pointed out to me a singular appearance. The massive porphyry is interspersed with nodules of the same substance, but much more compact than the surrounding rock, and sometimes presenting, though indistinctly, the appearance of crystallized facets. The size of these balls, as well as their frequency, varies in different parts from two inches to one-tenth of an inch in diameter, in the space of a few fathoms to which this singularity is confined. This occurs in a place where several threads of ore which intersect the gangue unite.

The direction of the great vein is from E. to W., or, as the miners say, "they work it toward the sixth hour," and is elevated at about an angle of 80°. It is at its greatest width at the depth of seventy fathoms, being there nearly twelve fathoms, and continues so as far as it has been worked downward, which is about forty fathoms lower. As it ascends towards the day, it becomes narrower, a circumstance which I was informed is here rather an exception than a general rule.

This is the richest vein at present worked, and yields almost one mark of silver from every centner of ore. During the last fortnight the quantity of ore obtained had been 319 centners, and this yielded by assay 282 marks of fine silver. The preceding fortnight it produced 239 centners of ore, which gave 262 marks of fine silver. At a former period, this mine in the course of twenty-eight years produced half a million of marks of pure silver.

In working such large veins it is found necessary to begin from below and go upwards, and to fill up as much as possible as the miners ascend; for, were they to begin from above, there would be no place in which to deposit the unproductive matter; a prodigious mass of superincumbent rock would keep the workmen in perpetual danger, and by falling down might put a stop to all proceedings in the mine.

Having now observed the mode in which the ore is collected, and raised to the surface, and the means by which they free the mine of water, I will follow the ore to the operations which it afterwards undergoes.

When brought from the mine it is carried to a building, where men, women and children, sitting at tables, select the rich ore from the poor, break it into pieces about the size of hazel nuts, and sort it, according to its worth, into different parcels. The value of these heaps is then ascertained by an assay, and the pay of the workmen regulated by the product. This is estimated on the number of half ounces or *loths* of silver contained in the *centner* or hundred and ten pounds of ore, and this varies from the minutest quantity to one hundred loths or more. If it contains above two loths, it goes immediately to the smelting works; but if it be poorer, it is previously submitted to the *pochwerk* or stampers, where it is pounded and washed, and the most valuable articles concentrated.

The process at the pochwerk is nearly as follows:—The ores are thrown by small quantities into a long trough, through which a gentle stream of water is constantly running. A row of stampers,

ers, perhaps twenty-four in number, alternately raised by cogs, placed spirally round a cylinder which moves behind them, fall perpendicularly,* and in constant succession, upon the ore so placed in the trough. The water passing through the trough carries with it the particles separated by the operation of the stampers on the ore, and, being conducted through a number of small winding channels, has time to deposit them before it runs finally away; the smallest being suspended till they reach the extremity of the canals, whilst the larger are deposited sooner. The whole is then easily separated, according to its fineness and weight, and is taken to a set of inclined planes, each about ten feet long by four broad, having boards set edgeways at their sides. Above each is a trough in which the ore is put, and into which a gentle stream of water is made to fall, and which passing on carries with it the pounded ore, and running softly down the inclined plane, over the whole surface of which it is spread, deposits the particles equally; but, being nearly uniform in size, those which are left nearest the top are the richest. During the whole time a man stands by, and with an instrument of wood, like a rake without teeth, gently moves the surface of the last deposited matter, that it may thus again be exposed to the action of the water, and any of the lighter particles may still make their escape. When the quantity collected on these inclined planes covers the whole about eight inches deep, it is divided into three parts; that which is nearest the top being the richest, that at the bottom the poorest. The whole is then removed with shovels into three separate heaps, and each undergoes the same process three times. The different portions are now again submitted to the assay: the richest are sometimes found to contain six or eight loths the centner; and if any is so poor as not to contain two, it is carried back to be mixed with the ore under the stampers, but the richer parcels go to the smelting-house.

Ores which are so disguised by clay as to prevent the sorters from judging of their value, are previous to their undergoing the before-mentioned processes thrown into troughs having grateings in their bottom of different degrees of fineness. They are kept in constant motion by women, who use wooden shovels for the purpose, whilst a stream of water running over the ore washes the smaller pieces, together with the dirt, through the first grating into the next; and so on through several troughs, by which the whole become separated according to size, the water finally carrying off all the earth and finer particles. But this is not suffered to run waste. It is conducted through a long succession of canals, where it forms its deposits as in the pochwerk. The larger pieces are then returned to the sorters, and classed

with

with the other ores. But the smaller pieces are separated by putting them in sieves, which are repeatedly plunged into water. The water penetrates from below; and as the displaced fragments of ore again subside, the heaviest and generally the richest fall to the bottom. This being several times repeated, the sorters are enabled to make a tolerably accurate division, by removing with a shovel the upper half, that which remains being retained as valuable.

The greater part of the ores at Schemnitz contain a large proportion of lead, some copper, with sulphur, arsenic, and other minerals, and a small proportion only of silver. These are smelted in the furnaces, which are erected upon the spot; but those which contain a large proportion of silver are taken to the silver furnaces at Kremnitz.

The works which I visited near Schemnitz are denominated lead furnaces, although their object is not to obtain the lead, except in combination with the silver and gold, and as a means of procuring these precious metals.

The ores, having by the operations of the pochwerk been separated from a large part of their earthy impurities, are roasted, in order to drive off the arsenic, sulphur, and other volatile matters. This is done either in open furnaces, in which it is piled in alternate layers with wood, or in reverberatory furnaces, in which only a moderate degree of heat is kept up. This roasted ore is then removed to a blast furnace, the bellows of which are worked by water. It is here mixed in layers with charcoal and various slags and scoria of former processes, all of which contain more or less lead, and contribute to the easy and perfect fusion of the ores. The heat in the furnace having by the constant working of the bellows been greatly raised; at the end of a given period, if the process is found to be perfected, an opening is made in the lowest part or eye of the furnace, by piercing a stopper made of clay and charcoal powder, with which the aperture had been closed when the furnace was charged. Through this opening, the liquefied metal, which had collected in the bottom of the furnace, runs into a circular cavity, or bed formed of charcoal and clay. This fluid metal consists of the lead, to which the silver and the gold, if any, have a strong attraction, and are intimately united; and of copper and any other metallic substance, such as iron, that may have been combined with the ores, and have not been oxidated in the furnace. As the lead containing the silver remains fluid at a much lower temperature than copper; the latter separates with the other impurities, and quickly forms a porous crust, or slag, upon the surface, which is removed by tongs as soon as it acquires the thickness of half an inch. This is repeated till these crusts cease to collect, when the

the lead holding the silver and gold remains in the bed nearly free from any alloy of copper. What little still remains is afterwards separated, by submitting the metallic mass to a heat sufficient to melt the lead, but leave the copper. The ingots are then removed to the silver furnaces, of which there are three; one at Schemnitz, one at Schernovitz, and one at Neusohl; each ingot having been previously most accurately assayed. This is likewise done in respect to each parcel of the rich ores which are sent raw to either of these silver works.

The slags which have been removed from the surface of the lead in these processes are often very rich in copper, some containing as much as 100 loths in a centner. They are all removed to the copper works at Altgebirg near Neusohl, to be refined.

These being the whole of the operations which are conducted at Schemnitz, neither pure silver nor gold ever makes its appearance there, except in the small quantities produced in the laboratory from the assays.

The footing upon which the mining school or college of Schemnitz is conducted is very liberal. It is a Royal foundation; and every one who has first obtained permission from the Board of Mines at Vienna, which I believe is never refused, may have the full benefit of all the lectures, and all the practical knowledge which these extensive mines are calculated to afford. The complete course of study occupies three years; and those who wish to obtain certificates, such as are required to entitle them to seek for employment as officers of the mines, must go through regular and severe examinations. The lectures are on chemistry, mineralogy, mathematics, mechanics, and other branches of natural philosophy—drawing of plans, maps, and machinery, &c.; also on botany, and the knowledge connected with the cultivation and preservation of forests, and the conversion and application of timber—a science which the Germans call *forstwissenschaft*, and which is of great importance in these countries, which depend upon their forests for fuel; and more especially in mining districts, where so much valuable timber is necessarily consumed in the construction of machines and mine-shafts, and in the support and preservation of galleries and communications under ground. The students have, besides, the free use of the laboratory, and constant access to every thing which is going on in the mines, and in the various works connected with them, and with the preparation and smelting of the ores. They have likewise permission to form collections of minerals to any extent for their own use; but they are prohibited, under the pain of expulsion, from extracting the metals and applying the pro-

 duce

duce to profit. The students generally form themselves into associations of two or three, for the purpose of carrying on their chemical and metallurgic processes in the laboratory with greater care and advantage; and I was much pleased when Professor D'Höring one day pointed out to me various repositories appropriated to each, for placing away the apparatus, and every article necessary for conducting these experiments and assays, the whole of which are provided for them by the public fund.

The number of students at this college varies a good deal, and at present, owing to the general and long-continued disorder in public affairs, is at a low ebb—it seldom, however, falls short of from 200 to 300. Many go regularly through the whole course, but others attend only the lectures which are connected with their particular pursuits.

Kremnitz.—Kremnitz is situated, like Schemnitz, in the midst of mountains. It consists within the walls of thirty-five houses, one of which is the mint. They are arranged round an open space where the market is held. There are some streets and many detached houses without the walls, and at the distance of about a quarter of a mile in the valley are situated the silver furnaces.

It will be remembered that the silver at Schemnitz was left, some in the state of rich ore, and some, after it had undergone the process by which it was concentrated in the metal which had issued from the lead furnaces. The greater part is sent here, but some to Neusohl and Schernovitz, to be resmelted with the ores of this neighbourhood, which contain a much larger proportion of gold, and the metals are here finally separated and refined.

Each parcel of ore, and every ingot of metal, before it is delivered to the furnaces, is again assayed by the proper officer in the following manner:

From each parcel of ore a certain number of ounces are taken in such a way as will give an average sample of the whole. This is heated to drive off all the moisture, and then reduced in an iron mortar to a fine powder; a known quantity of this is put into a small crucible, and to it is added about twice its weight of pure lead in grains of the size of small shot, and known to contain no silver. If however it be very refractory, a mixture of one part borax and two of glass of lead, or the vitreous slag of former assays, is added to facilitate the fusion. The crucibles thus charged are so arranged in the furnace, that no mistake can arise respecting the parcel to which each assay belongs. A strong heat is then raised in the furnace, which is continued until the fusion is complete. They are then taken out, and when cold,

cold, by a few blows on an anvil the vitreous matter or slag which surrounds the whole contents of the crucible is broken off, and a button of lead retaining the silver is found. This button is placed in a small vessel or cupel made of bone ashes, and again submitted under a muffle to a considerable heat, by which the whole is melted, and the lead, being considerably oxidated, is absorbed by the cupel, a little shining pearl of silver alone remaining, from the weight of which the whole of this precious metal contained in the parcel of ore is calculated. By a similar process the richness in silver of the ingots is likewise ascertained.

The silver in both cases contains gold, the quantity of which is still to be determined. This is effected by placing it in small flasks or retorts of glass, and pouring upon it twice its weight of nitric acid; which being exposed in a sand-bath to a gentle heat, the silver is completely dissolved, and the gold falls down in powder. This precipitate is carefully washed, and put into a small conical crucible fitted with a cover; a gentle heat is applied, and the pure gold remains in a spongy mass at the bottom. The solution is then evaporated to collect the silver. This mode of assay differs but little from the processes used in respect to the ores and metal in the gross.

The first part of the operations used on the ores is similar to that at Schemnitz. When the fusion is complete, and the metal is let out of the furnace into the circular bed prepared for it in the ground, ingots of lead rich in silver are continually added as long as they will melt and unite, the crusts of slag being removed as they form on the surface. In this way a mass of metal is obtained holding thirty, forty, or even fifty loths of silver in the centner, which is then laded into flat moulds to cool. These are assayed previously to their being placed in a reverberatory furnace, fitted with a large iron cover suspended by chains by which it is elevated and lowered at the will of the operator; and a brisk flame being made to play over the metal, the lead is quickly oxidated on its surface: this is removed, and a new surface being exposed to the action of the flame and air, the same is repeated until nothing remains on the sand forming the bottom of the surface, except the silver holding gold, which is taken out by ladles and poured into ingot moulds. This precious alloy is thence removed to the laboratory adjoining the mint, to undergo the operation of "*parting*," or the separation of the gold from the silver.

The ingots are here melted down, and the metal whilst fluid poured into water, by which it is granulated, or divided into almost leaf-like pieces which are in appearance exceedingly beautiful. These being dried are put into large glass retorts extremely well luted. Nine marks are placed in each vessel, with about twice that weight or somewhat less of nitric acid of the specific gravity of 1·20, previously purified of any combined sulphuric or muriatic acid it might contain, by dissolving in it a portion of perfectly pure silver. These retorts are placed in a sand-bath, with receivers properly fitted to collect any acid fumes which may pass over. The silver is in this process taken up by the nitric acid, forming a clear solution, which being decanted off, leaves as a residuum the gold, in the state of protoxide, having the appearance of a brown powder. This, when collected in a crucible and exposed to a low heat, assumes its yellow colour, but without metallic lustre; the particles adhering but slightly together. It being however perfectly pure, nothing further is necessary than to fuse and cast it into ingots.

The transparent solution of silver in the nitric acid is now poured into retorts standing in the sand-bath, and, being gently heated, is distilled over into receivers in a state fit to be employed in fresh solutions, leaving the silver in a sponge-like metallic form chiefly at the bottom, but likewise adhering in a thin coat to the sides of the retort, without lustre, but beautifully white. Fresh quantities of the solution are then poured into the retorts, and the distillations repeated until they are nearly half full of dry nitrate of silver: a considerably greater heat is then applied, in order to decompose the metallic salt. The retorts are then broken, and their contents with the adhering pieces of glass and a portion of black flux run down in black-lead crucibles, and the silver cast into ingots.

The laboratory is of great extent. Five or six banks of sand, for so they may well be called from their size, extend across the chamber, beneath which the fire is conducted in flues; and upon the whole extent, processes of solution and evaporation are constantly going on. Here are also all the furnaces and apparatus necessary for performing the several operations before mentioned.

Both gold and silver in their greatest degree of purity are found too soft for circulation in coin: before, therefore, they are made into money, the standard is reduced by the addition of alloy either of silver or copper, to give the necessary degree of hardness and durability. In the inferior silver coins much more is added to increase their weight and dimensions, as well as for other sufficient and perhaps not less obvious reasons. In the Austrian pieces of twenty kreutzers there are only nine loths, thirteen grains of silver, to five loths six grains of copper. In the gold coins, however, the proportion of alloy is exceedingly small. A ducat weighing 53·85 grains contains only 0·56 grain of copper, with 53·29 of gold; and according to the ancient

cient standard, even not more than one-third of this small quantity; and yet this has such a perceptible effect, that it is necessary to procure for this alloy the purest and most malleable copper which can be obtained from other mines, the copper of Kremnitz not being found sufficiently so for this purpose. Copper, as an alloy to gold, makes the coin much harder and less liable to wear than silver, which is the alloy used in the ducats of Holland.

The silver having been melted in combination with copper, as its alloy, is cast, in moulds of iron the sides of which are kept together by a clamp and screw, and which are placed erect in iron sockets, into bars nearly two feet long, two inches thick, and four inches wide. These bars are drawn out between iron rollers, after frequent repetitions of heating in a furnace, and rolling to a given thickness, by which they are greatly extended in length, but little increased in width. After each time of passing the roller, the silver is plunged into cold water.

A screw press is then used to stamp out the blanks of the required size, which having been previously dipped in a dilute acid, to restore their colour, are separately weighed.

The impress on the edge is next made by a small hand instrument placed horizontally, consisting of a circular plate moveable by a handle on its centre, within an external fixed ring, leaving a space between the two equal to the diameter of the blank to be milled. On a portion of this extensive circle corresponding in measure to the circumference of the piece, the device intended to be impressed is cut or fixed. The blank is placed in the intervening space; and by moving the plate, which presses tightly upon it, the piece is made to describe a complete revolution round its own axis, and, moving in close contact with the outer ring, receives the impression on its edge.

In order to complete the coin, nothing now remains but to stamp these pieces with their proper dies. This is done by means of powerful fly screw-presses, such as are generally made use of for the purpose; and are so constantly employed in various processes of our hardware and plating manufactures at Birmingham and Sheffield, that they need not be described.

The method used in the gold coinage is precisely the same. The whole both gold and silver coins are again separately weighed, packed, and sent to the Treasury at Vienna for circulation; but certainly not at this time for general use, as none were to be met with in common currency.

For inspecting and counting the copper coins they have a ready expedient. Trays, having their bottoms indented with one or two hundred hollows, are filled with money; and being shaken, each hollow receives a piece. The rest are swept out: the known number

number being at one view inspected, the tray is overturned into the proper receptacle, and is instantly ready to receive a fresh supply.

With respect to the quantities of gold and silver which have been obtained by the Government from the mines of this district, and coined at Kremnitz, Delius in his work upon Mining has calculated that from the year 1672 to 1680, the single royal mine of Piberstollen at Schemnitz gave 427,600 marks of silver, and 2,657 marks of gold. In 1690 the gold from Schemnitz amounted to a little more than 1872 marks coined into 132,425 ducats. In 1779, 2429 marks of gold, and 92,267 of silver were brought to the mint from the whole district. And by a statement published at Vienna in the *Vaterländische Blätter* of 1808, it appears that the whole produce of the mines of Upper Hungary, between the years 1797 and 1806, amounted to

16,821	marks	4 loth	29 dr.	27 gr.	of gold,
658,519		0	52	19	of silver,
135,443	centner	83	perfunds of lead.		

The whole value in the currency of the country being 16,728,368 fl. 22 kr. But I am not quite certain what mines are included in this estimate.

IV. *Account of the Process of Amalgamation used at Halsbrück near Freyberg in Saxony, for the Extraction of Gold and Silver from other Ores. By* Richard Bright, *M.D.**

The operation of amalgamation was first used in the mines of South America, where it was introduced between the years 1560 and 1570. There, however, the process was at first conducted in a very imperfect manner, and was attended with a great loss of mercury as well as silver. In that country it underwent successive modifications and improvements, and it was there first discovered, that a very effectual method of conducting it was by boiling the mercury and the ore together in water.

Although proposals had been more than once made to the Court of Austria, it was not till 1784 that the method of extracting silver by the aid of mercury was adopted in Europe, at which time Baron Born was authorized to make extensive trial of its efficacy in the mines of Hungary. The process as established by him at Glashutte near Schemnitz, and afterwards introduced into the other mines of Hungary, Transylvania, and Bohemia, was in substance very nearly that which is employed at Freyberg, except that for some time the formation of the amalgam took place under the influence of heat, the mixture being

* From the same work as the preceding article.

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put into copper boilers in the form of an inverted cone, rounded at the bottom and open at the top, in which an instrument was made constantly to revolve; thus keeping the whole in agitation, while a very moderate heat was applied under the boilers; and this process having been continued for from ten to twenty hours according to the nature of the ore, the whole of the silver was found to be disengaged from the ore and taken up by the mercury. Baron Born made many attempts to conduct this part of the process without the assistance of fire; but it was Gellert who first perfectly succeeded, and of his success the very complete machinery of Freyberg was the result. From many comparative calculations and experiments which have been made, it has appeared that the saving in the consumption of wood, and in the lead wasted by the common processes of smelting and refining, is so great, that ores of silver and gold which are of a nature fitted for this process can be worked at nearly half the expense by amalgamation; and it is satisfactory to be assured that the mercury, so far from producing the deleterious effects upon the health of the workmen which were at first dreaded, is in fact by no means so hurtful as the heat and fumes which are to be encountered in the usual operations of the smelting furnace.

The process now used at Harlsbrück is as follows: The ores, after having been sorted, stamped, and washed in a manner similar to that which has been described, are brought in separate lots to the mills, where each lot is sampled with much care. These samples are divided;—the one part is assayed by the assay-master of the mine from which the ore was brought; the other by the officers of the works,—a very necessary check to prevent errors, and more particularly in this case, as the amalgam works belong to Government, whilst many of the mines are worked by companies of individuals.

The ores are then appropriated either for the operations of the furnace, or of amalgamation, according to their qualities; those being chosen for the latter, which are the most free from an intermixture of lead and copper; and preference is likewise given to ores which yield from three to four ounces of silver in the centner, it being found by experience that such are best fitted for this process. The different parcels, the produce of which has been ascertained by assay, are therefore so fixed that the whole may average about this proportion. To this ore is added one-tenth of its weight of muriate of soda, finely sifted. This mixture is then parcelled out, in heaps of three or four hundred weight each, upon the floor of a chamber over the reverberatory furnaces in which it is to be roasted. Here it is dried for some hours, and is then passed down a pipe which communicates with the

the furnace below, over the bottom of which it is spread by means of an iron rabble or rake. The fire, which is of wood and a mixture of coal and clay, is contained in a division of the furnace separate from that which receives the ore, with which it is connected only by a large aperture, through which the flame and heat pass into the vaulted compartment containing the ore, and out at the chimney erected over the other end of the furnace. By this means a high degree of heat is given to the ore without any contact with the fuel, and the sulphur and other volatile matters which arise from it are speedily carried off. The workman attending the furnace keeps the ore in a constant agitation with his iron rake, to prevent its adhering together in hard lumps, especially when it becomes red hot, and, by changing the surface, more regularly to expose the whole to the operation of the flame and heat. This is continued, and the red heat maintained, for three or four hours, until there are no longer any signs of sulphur remaining in the ore. The whole is then withdrawn from the furnace. During this operation a decomposition of the muriate of soda has taken place, the acid forming new combinations with the earthy parts, and the oxides of the imperfect metals, and the soda with the sulphur which had not been expelled by the heat; whereby the union of the silver is rendered much less intimate with the substances from which it is to be separated.

The calcined ore, as it is taken from the furnace, is put when cooled into boxes, which are raised by a crane worked by water into an upper story, where it is sifted, and all the pieces which have caked together are separated from the rest. The cakes are broken, and being again mixed with a small portion of salt, are once more roasted; but the finer parts which have passed the sieve are conveyed down by pipes into the mills, where they are ground to an almost impalpable powder. These mills, of which there are several, are all turned by water; the mill-stones are of granite.

When thus prepared, the ore is carried in barrows to a chamber, where twenty chests present themselves arranged in rows of five each. These stand immediately over corresponding vessels or barrels, in which in the room below the amalgamation is to be effected, and which barrels are charged from the chests by means of moveable pipes.

It is now that the important part of the process takes place. The twenty barrels are arranged in four rows, each turning on its separate axis by a motion communicated by a water-wheel to two long shafts, each shaft passing between two rows of the barrels and furnished with cog-wheels, working in others fixed on on the axis of each barrel, but from which either barrel may be detached at pleasure, and its motion stopped without impeding the rest.

Each of these barrels is charged with ten hundred weight of the pulverized ore, about three hundred weight of water, and a small quantity of sheet-iron, which is added for the purpose of decomposing any muriate of silver that may have been formed during the process of roasting, and to prevent the subsequent formation of any muriate of mercury.

A gentle rotatory motion is communicated to the barrels for about an hour, to mix their contents intimately. Five hundred weight of quicksilver is then added to each, and the motion of the barrels accelerated to the rate of nearly twenty revolutions in a minute, and this is continued for sixteen hours. When by assay it is found that the separation of the silver is complete, the whole having formed an amalgam with the mercury, and none being left in union with the earthy parts or metallic oxides, the barrels are entirely filled with water, and they are set in motion again for about an hour, but with much less velocity, that the amalgam may separate completely from the rest of the mass, and be allowed to subside. The amalgam is then drawn off from the lowest side of the barrels, and conveyed along wooden channels to vessels prepared in another chamber to receive it. The remaining water is washed from the barrels into reservoirs.

The amalgam and surplus mercury, which flow away together, are put into leathern bags, which being pressed, suffer the uncombined sulphur to pass through the pores, leaving the amalgam, containing about one-eighth of its weight of silver, in the form of a paste composed of silvery globules.

The washings of the barrels, which are collected in four large reservoirs, are kept in a continued agitation, during which the mercury which remained entangled with the refuse subsides; and as this takes place, the upper strata of the water are successively removed till the mercury and amalgam, if any, alone remain. This generally occupies about eight hours. This mercury and amalgam is of course added to the rest, and from the liquor the sulphate of soda is afterwards obtained.

It remains now to collect the silver which is thus concentrated in the amalgam, by driving off the mercury. For this purpose a furnace of mason-work of a peculiar construction is employed. A tripod of iron is placed within it, which standing in a vessel of water supports an upright bar of about three feet in height, at the upper part of which are arranged five iron saucers holding portions of the amalgam. The whole of this is covered by a bell of cast iron which descends into the water.

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An annular iron plate or shelf is then applied round the bell externally at about half its height, and on this shelf a fire of turf is kindled. The door of the furnace is then closed, and the flame plays round the upper part of the bell, till the whole of that portion of it which surrounds the vessels or saucers containing the amalgam becomes strongly heated. The distillation of the mercury then takes place; it rises in fumes, which falling condense in the lower part of the bell and the vessel of water beneath. In about eight hours the whole of the mercury is separated, the furnace is suffered to cool, and the silver (containing however some metallic impurities, particularly copper,) is found forming beautiful spongy cakes in the iron saucers. This is afterwards melted and refined in the furnaces adjoining to the amalgam works, where much of the richer ores, and the produce of that which containing larger proportions of other metals had been reduced in the blast-furnaces, is likewise melted and refined.

The operation of which I have now given a sketch, is undoubtedly the most interesting object which Freyberg and its neighbourhood afford. The process of amalgamation, in itself so curious, is there more extensively and better performed than in any other part of Europe.

V. *Observations on the various Changes which take place on treating Uric with Nitrous Acid, and on a new Acid called "Erythric" thence produced. By Dr.* Gasper Brugnatelli *.

My father (Professor Lewis B.) being occupied in making experiments on the human urinary calculi, for a work which will be published in the present year, I wished to employ myself in examining some of the substances which are most generally found with such calculi. In studying the chemical constitution of uric acid, I was particularly led to observe some changes which it experienced under certain circumstances. Of those observations I now undertake to give a brief account, although not without that diffidence which juvenile inexperience in the chemical art should inspire. Uric acid differs from the greater part of the other known acids, by having a chemical constitution much more complicated, the number of its component principles amounting to four, which is not the case in the others. Hence it is natural to infer, that in whatever manner it may be decomposed, a great

* From Brugnatelli's Journal, 1st and 2d bimestre in 1818.

variety

variety of products must be obtained. This inference is particularly confirmed, when it is exposed to the action of nitric or nitrous acid; and the phænomena which accompany this action are so singular, as to excite curiosity respecting the chemical alterations which are its effects.

Scheele, the celebrated discoverer of uric acid, first observed in like manner the violent action of nitrous acid on this substance, and the red colour which its solution leaves on the skin, or which it acquires in evaporating; but by one drop of nitrous acid it is instantly destroyed. He likewise observed that this solution had always an acid taste, that it did not alter the metallic solutions, nor precipitate with muriate of barytes; but, on the other hand, it yielded in lime-water a white precipitate which was soluble in nitrous and muriatic acids without effervescence.

Bergman observed, that when treated with potash in excess it did not become turbid, but by digestion acquired a reddish colour which readily tinged the skin, and the solution thus joined to potash precipitated in a particular manner the metallic solutions*. He also considered as very remarkable the fine red colour which he obtained by treating uric with nitrous acid, and examined the circumstances which accompanied the appearance of this colour, and its destruction effected by acids or caustic alkali †. The same chemist and Scopoli afterwards observed that the reciprocal action of these acids produced a considerable quantity of oxalic acid ‡; a change which my father found to be greater and more rapid, if, instead of nitrous acid, chlorine were used §. These observations directed Fourcroy to determine what were really the changes produced on uric acid by chlorine, which there was every reason to believe could not be very dissimilar from those effected by nitric acid. He found that under water chlorine changed uric acid into ammonia and carbonic, oxalic and malic acids. The first acid formed is the malic, which with the continued action of the chlorine changes into oxalic; and this increasing, both acids are resolved into carbonic and water ‖. These are the changes which chemists have hitherto observed as taking place in uric acid when treated in this manner. To me, however, it appears that many others are produced, as will be seen by the subsequent observations. When a little uric acid dissolved in nitrous acid is reduced to dryness, having a red colour, and exposed to the flame of a lamp in a

* Scheele, *Mem. de Chym.* t. i. † Bergman, *Opusc.* t. iv. *Obser. de Cal. Urin.* ‡ Crell, *Ann.* 178. See Brugnatelli's Memoir on the Sediment of Urine, where the history of this discovery is related, p. 116. § *Ann. de Chimie*, xxxi. p. 133. ‖ Fourcroy, *Syst. des Con. Chim.* t. x. p. 222.

watch-

watch-glass, there is often seen in the centre a kind of spume formed of a brown or yellow colour. This is more conspicuous if the experiment be performed on a larger scale. In such a case, having projected the nitrous on the uric acid, without diluting it, until the rapid decomposition has ceased, after some repose, a copious deposit of minute grains is formed *. On evaporating the whole with a moderate heat, many suffocating white vapours are disengaged, which become still more numerous in the process. After a time, the whole mass acquires a yellowish colour and becomes fluid, but coagulates immediately if removed from the fire. Continuing therefore the heat, the vapours finally cease to be suffocating, and the mass acquires a brown colour, while the edges usually become of a rose colour. Urging the fire still further, white vapours continue to be disengaged; mean time the mass becomes a bulky charcoal, which with a crack is in an instant almost entirely destroyed.

But, stopping here to consider the brown matter above mentioned, it is at first so hard that with difficulty can it be removed from the vessel, although in a little time it attracts humidity and softens. Placed in water it dissolves, and communicates a citrine yellow colour, leaving behind a blackish matter. The solution has a slight acid taste, and reddens the blue tincture of vegetables. Caustic potash either immediately or after a slight concentration produces a flaky precipitate, and at the same time ammonia is sensibly disengaged. Subcarbonate of potash produces a similar precipitate, which has a colour inclined to red. Treated with lime-water, the solution requires to be more concentrated to produce the precipitate, which assumes the form of very light flakes, which, on being reduced to dryness, become yellow shining scales. Similar scales are obtained even by evaporating the original simple solution. These salts have a sweetish taste, and are much more soluble in warm than cold fluids; they are deliquescent; but it appears that this property is greatly augmented by a particular yellowish matter which accompanies them, and which greedily attracts the humidity of the air, deliquescing itself, dissolving them with it, and even rendering them more soluble in water.

The above solution decomposes immediately when brought into contact with a solution of lead or silver, and becomes turbid. If it be mixed with acetate of lead, the precipitate collected, and afterwards very well washed, and dried with a moderate heat, the salt of lead may then be decomposed by dilute sulphuric acid.

* Bergman, operating directly on the calculi of uric acid, observed the constant formation of this deposit. It is indeed immaterial to this experiment, whether pure uric acid is used, or that which is found well formed in human calculi.

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In effecting this decomposition, and taking care that in the fluid no sulphuric acid remains, an acid of a yellowish colour and sour astringent taste is obtained. It reddens the tincture of turnsole, and when evaporated does not form crystals, but attracts a little humidity. The salt of lead from which this acid is extracted, is very soluble in acetic and dilute nitric acid. From many of the above characters which this acid substance possesses, it may be concluded to be malic acid. It is not, however, easy to conceive how the malic acid can exist in the brown mass obtained by the long action of nitrous on uric acid, while in it there was no longer found any trace of oxalic acid; and as this acid is formed at the expense of the malic, which is altered by the nitrous acid, so much greater is the force that can thus entirely decompose the malic acid. This reflection made me suspect that the acid substance was not malic acid, but one of those acids which have much affinity with it, among which the illustrious Scheele distinguished the lactic acid *. The characteristic difference which he established between these two acids is the insolubility in alcohol of the calcareous salt of the former, and on the contrary the solubility of the calcareous salt of the latter. Hence, having observed that alcohol projected on the calcareous salt which I obtained became turbid with a drop of oxalic acid, I concluded it to be lactic acid, which I endeavoured to ascertain. As the salts of potash and of lead are soluble in alcohol, so also is the salt of potash which is obtained in an irregular form by the above method; and when the alcohol is evaporated by a gentle heat, it is found elegantly crystallized in long and slender needles. The regular form is likewise obtained when the pure acid is directly united with potash.

To the opinion that the acid obtained may be the lactic acid of Scheele, supported by the solubility of its salts in alcohol, it may be opposed, that this is perhaps owing to the presence of the particular yellow matter above mentioned; and in fact, experiments prove that it at least augments the effect. But it may be answered, that the acid obtained by Scheele should not be entirely devoid of this matter. Berzelius found lactic acid united to a particular matter in all the animal fluids, and it is the opinion of this chemist that the acid obtained by Scheele was very far from being pure. When the yellow solution is extracted from the original brown mass, there remains, as already noticed, a blackish matter. This dissolves rapidly in potash, and the alkali is in a great measure neutralized. The solution has a deep ruby colour, if it is concentrated and the potash in excess; otherwise the colour is a deep yellow. This solution, provided that it

* Crell, *Ann.* part ii, 1785, p. 303.

has not too much alkali, has a sweetish taste, and tinges yellow either blue or white paper. Acetic acid produces in it a light gelatinous precipitate, analogous in appearance to uric acid when obtained by a similar process. The substance precipitated has a yellow colour more or less deep; it does not readily crystallize like uric acid, but presents here and there some shining points, and in drying it contracts and breaks in many pieces. It scarcely alters the blue tincture of vegetables, and destroys the colour previous to reddening it. In cold water it is almost insoluble. Lime and ammonia dissolve it; but the addition of an acid to the solution precipitates it again, although of a less deep colour. Hence these combinations, like the urats, are always dissolved in an excess of base. The combination with potash produces yellow coagulations in the solutions of silver and lead. The above-mentioned substance which the acids precipitate burns with all the characters of animal matter. Nitric acid projected on it is decomposed, and when the solution is evaporated no red colour appears; but, on the contrary, a residuum of a yellowish colour is found. Hence therefore results a substance in many characters analogous to uric acid, but which in many other essential qualities is distinct from it. To this substance is also united that particular yellow matter which we have seen accompanying the supposed lactic acid. In decomposing with acetic acid its combination with potash, the acetate of potash which passes the filter is coloured yellow. By removing this salt, and evaporating the residue, we obtain matter equal to that which was produced by washing the original brown mass, if the acid combined with the ammonia which it contains be removed. This peculiar yellow matter is soluble in water and in alcohol, but in a much greater degree when hot than cold. By evaporating it slowly it maintains its colour, and is reduced to a mass having a gummy appearance, which seems disposed to crystallize and readily attracts humidity. But if it is evaporated more rapidly, placing it in a watch-glass over the flame of a lamp, and removed from the heat when it has acquired much consistence, it appears chiefly remote from the centre where it attains the colour and appearance of wax. If the heat be continued it burns, emitting the smell of animal matter; it swells extremely on being converted into charcoal, and finally, with a slight crack the whole is destroyed in an instant. Nitric acid poured on it rapidly decomposes, and the solution after some time yields a white granular deposit; evaporated, it entirely changes into a white mass at first sufficiently hard, but afterwards attracts humidity, when it is very difficult to reduce it to charcoal by heat. It would be difficult to determine the precise moment when each of the above-mentioned substances begins to be formed; and we can only form

form conjectures on the different appearances of colour, or the different odours, which are developed during the process. It would be equally difficult to ascertain, that with the same elements of these substances there may not be formed, and afterwards destroyed, other peculiar combinations. The animal substances are endowed with so much mobility, and suffer such notable alterations in consequence of a small change in their parts, although imperceptible to the senses, that we need not be surprised if in this case some of them should have escaped our attention. But without digressing from my subject, I can adduce examples in which nature betrays herself, and reveals in the mean time that in the course of her operations important changes are effected. These facts will not be reluctantly learned while they are accompanied by many interesting phænomena.

It it well known that the solution of uric in dilute nitric acid, reduced to dryness and heated, has the property of communicating to bodies a deep red colour. This dye is therefore very soluble in water, and we may obtain from it a beautiful liquor of a light ruby colour*. Being provided with an abundant quantity of this liquor, I evaporated it to obtain the colouring matter in a solid state. At a certain period of the process, the fire becoming somewhat strong, the rosy colour in an instant disappeared, and was succeeded by a yellowish hue. This change has occurred every time I repeated the experiment. It appears that in this case even the water had a part, since we know by other means this colouring matter may be obtained unaltered in a solid state. In seeking to discover some means of restoring the faded colour, I found that potash, ammonia, and lime answered this purpose, only the colour reproduced was more rosy and delicate. The potash in the smallest quantity produced the effect better than the others. In like manner it at the same time yielded a rosy precipitate, which when left to repose attracted all the colouring matter, and the solution remained discoloured and alkaline. If, when the precipitate is immediately formed, it be collected on a filter and dried, it retains a delicate rose colour, interspersed with very minute shining points. Those points, which have a most agreeable effect in the light of the sun, are found even beyond the space where the colour extends. Lime yields a deeper colour, and when collected on the filter has the appearance of velvet, and also presents brilliant points.

* I have almost always used the washings of the red spots left on the skin by the above solution, and it is often very deep. The red residuum which is obtained by the heat of the fire makes the washings naturally more easy to be changed, or to differ from themselves. It is remarkable that such red liquors, when slowly evaporated, yield prismatic crystals, and dissolving anew in water, we again obtain a reddish liquor.

Desirous of assigning some reason for the change of colour in the red liquor in consequence of heat, many arguments induced me to suspect that it must be owing to the influence of an acid. In fact, the acids produce a similar effect; and if at first they are unable to do it, the assistance of heat renders them immediately capable of effecting it. Besides, the crystallized points which we found detached from the colouring matter have all the appearance of a salt. It is certain that, if all that remains on the filter be burnt, the water which washes the carbonaceous residuum has alkaline characters. I have also observed that potash revives the colour of the first reddish liquor, but scarcely separates any of the coloured flakes: lime produces a similar effect. I have likewise seen that the coloured precipitates were insoluble in water, but very soluble in very dilute sulphuric acid. It is singular that this solution is effected with a species of effervescence, which appears to me to be increased in proportion as the brilliant points are more numerous. Hitherto however I have obtained them in too small quantities, and too impure, to subject them to that particular examination which they merit. At present, indeed, it appears to me that the above phænomena may receive a satisfactory explanation, supposing that the action of heat on the red liquor determines the formation of an acid, or puts it in a state to alter the colour in the above-mentioned manner; and that this acid may be scattered in very minute molecules, by uniting of which to a base, they may likewise give origin to those very small crystals. The solution, indeed, changed by heat, has a nauseous sweetish as well as acid taste, and does not sensibly redden the blue tincture of vegetables: but this may be attributed to the weakness of the supposed acid, in which the extraneous residuary matter may be more than sufficient to neutralize it. In obtaining the red colour by the action of fire, it was observed that, in finally drying, a yellow acid liquor destroyed the red colour previously formed as soon as it touched it. Neither are the metallic solutions inactive on this red liquor. Some make the colour yellow without affording a precipitate, as for instance, copper; others yield precipitates of the most beautiful colours, and separate all the colouring matter. Thus the solutions of silver, mercury and lead, yield sufficiently agreeable violet colours of different intensity, which fix themselves tenaciously on the paper in which they are collected. It is to be hoped that painting may derive something from such colours, as they serve to make sympathetic ink and other chemical sports. But it must be observed that they are changed as much by acids as by alkalies. In fact, the substance on which depends the faculty of dissolving uric in nitrous acid, and of becoming red with heat, has all the characters of an acid.

PART

PART II.—Having intimated the discovery of a new acid in the preceding part of this memoir, thanks to the care with which my father superintends my studies, I am now enabled to describe it more completely. This acid, as already observed, originates from the action of nitrous on uric acid, and is distinguished by the singular property of reddening when exposed to heat; for this reason I propose to call it *erythric*, from ἐρυθραίνειν, to redden. I now proceed to describe the properties of this new substance, but must observe, and regret, my imperfect success, as it is a compound which often changes and readily becomes of a quite different nature; and hence my disappointment in sometimes not being able to give an exact account of all the phænomena, and sometimes being obliged to abandon certain subjects without any research, to avoid entering into a too extensive and difficult field. Nevertheless, I hope that my observations will be of some utility to those who may subsequently examine this complicated subject under more favourable circumstances.

Mode of obtaining the Erythric Acid.

1. I have already related Bergman's observations on the rapid decomposition and deposit obtained by pouring nitrous acid on uric acid or urinary calculi. That deposit is the *erythric* acid, which I found disposed in regular figures; and to obtain it pure, the following is the easiest process to be adopted. Unite in the manner before mentioned the nitrous and uric acids; leave the mixture at rest until the numerous floating yellow flakes have settled at the bottom of the receiver; then pour off the liquor, collect the solid part on blotting paper, and dry it as much as possible; afterwards dissolve it in water, and evaporate it slowly in the air; by this method most beautiful crystals of pure erythric acid may be obtained. This liquor transmits nitrous vapours, and also contains erythric acid in solution.

2. Chlorine, iodine, and oxalic acids with uric acid can give origin to this new acid. Put uric acid in a bottle full of chloric gas, it will instantly be decomposed, and a substance which tinges the skin of a lively red colour will be produced. In like manner, a mixture of uric acid with iodine, or with oxalic, acid exposed to the action of heat, a decomposition is seen to take place, and finally a bright rosy residuum is produced. This appearance of colour indicates the formation of erythric acid.

Characters of the Erythric Acid.

3. The crystals of erythric acid have a rhomboidal form, are colourless and perfectly transparent; their taste is at first pungent, and afterwards becomes sweetish; exposed to the light of the sun they redden, and preserved in paper they impart to it

many reddish spots; exposed to heat they decrepitate, and also assume a red colour; left in contact with dry air, they effloresce, lose their transparency and become white. When found in this state they do not redden on exposure to the solar light; and if deprived of their water of crystallization, and exposed to the fire, they become yellow, and burn without reddening. Hence we may observe, that the presence of water is necessary in order to their becoming red.

4. The crystals of erythric acid are very soluble in water and in alcohol, without either impairing the transparency or changing the colour of those fluids. The watery solution has a sweetish taste, no smell; but it appears that it acquires a smell in time, which may be the index of its being about to undergo some alteration. It reddens the blue tincture of vegetables, and their colour may be restored by the alkalies. Lime-water becoming turbid discovers the presence of the smallest quantity of erythric acid.

5. The solution of erythric acid by spontaneous evaporation in the shade, crystallizes again without being altered: but if rapidly evaporated it becomes a solid red coloured mass, which is revived on dissolving in water, of which it colours a great quantity. In like manner the erythric liquid tinges the skin and other bodies red, more promptly than usual with the common solution of uric in nitrous acid.

6. To discover if in the act of changing any peculiar substance was evolved, the erythric acid was distilled with a strong fire. It does not boil out at a high temperature. Towards the conclusion of the evaporation, it became yellowish, afterwards red; but no product could be found in the simple water which was distilled.

7. In the solution of erythric acid reddened at the fire, a small portion only of the acid suffered change. In fact, the smallest drop renders turbid a great quantity of lime-water; and if by evaporation the solvent water is diminished, we see the erythric acid depositing itself. It is not, however, the same when the red solution is obtained by washing the spots left by erythric acid on the skin, cloth, &c.; in this case the erythric acid appears almost entirely altered, and lime-water scarcely discovers its existence, presenting after some time a thin net on its surface.

8. The colouring matter which reddens the erythric acid may be dissipated by heat. In fact, if the erythric acid be reddened in a watch-glass over the flame of a lamp, afterwards dissolved in water and again exposed to the same heat, beautiful red vapours are seen rising, particularly at night, and the fluid loses its colour. This fluid is found to be erythric acid, which may be again reddened. If the red liquor be rapidly heated in a retort, it loses its

its colour, and furnishes a fluid of a faint rose colour, and of a sweetish taste, which does not render lime-water turbid.

9. The red erythric acid in more or less time loses its colour, and in its stead usually yields white flakes. But if in obtaining the redness the heat be stronger than necessary, the red erythric acid changes colour much more easily and becomes yellow. This change, indeed, takes place instantaneously with the prolonged action of the fire; a strong smell of bitter almonds is then evolved, which is communicated to water in which the residuum is dissolved. This proves that carbon, azote, and hydrogen, as was easy to be imagined, enter into the composition of erythric acid.

10. The circumstances therefore of the formation of erythric acid not only induce the belief that it also contains oxygen, but that it contains it in an abundant quantity, so much and so rapid is the decomposition of nitrous on uric acid required to produce it. Moreover, it appears from the circumstances already mentioned, that the erythric acid acquires the red colour in consequence of a slight change effected in some one of its constituent parts, in which likewise water necessarily concurs. These considerations led me to try the action of the Galvanic pile on erythric acid, hoping by such means to throw some light on the unknown chemical changes that accompany the formation of the red colour.

Effect of the Galvanic Pile on Erythric Acid.

11. The pile which I used consisted of sixty pair of metallic plates with a superficies of two inches and a half square. At the negative pole a tumbler filled with a solution of erythric acid was placed, and another with distilled water at the positive pole. A platina wire communicating with the respective poles was immersed in the tumblers, between which passed a piece of amianth moistened with distilled water. The electric current was scarcely put in motion, when a phænomenon appeared which indicated that this experiment should succeed in the highest degree. Many bubbles of gas arose from the positive pole where the water was, and none, or scarcely one, and that with difficulty, issued from the other pole with the erythric acid. After about an hour the acid began to become yellow, and with the usual gradation of colour observed when the fire acted on it: finally, it acquired a deep red colour. After some time the disengagement of gas appeared copious even at the negative pole, but never so much as it was at the other. Although the erythric acid was become of a deep red colour, yet there existed a great quantity unaltered in the solution, but which after twenty-four hours was considerably diminished in volume. A portion also of the erythric acid was transported to the positive pole, as was indicated by lime-water.

 The

platina wire at the positive pole became of a yellow colour, and that at the negative was almost covered with a crust of red colouring matter.

12. The experiment was afterwards reversed, that is to say, a solution of erythric acid, reddened either by the pile or by heat, was placed at the positive pole, and pure water at the negative, arranged as before. The development of gas appeared from both poles. After a longer time than that which it had employed to redden, the liquor began to diminish in colour, and finally became, as at first, colourless. It was pleasing to see on the amianth a light rosy tint which terminated in a beautiful little red ring: it was insensibly moved towards the negative pole, and the tumbler of the positive pole was also marked by red rings or bands at the part towards the other pole and near the amianth. Changing the position of the tumblers, putting to the positive water, and to the negative acid without colour, the latter reddened and the colour vanished on the amianth.

13. These experiments seem to prove that the change in the red colouring matter of the erythric acid proceeded from the loss of oxygen which the acid sustained. Indeed at the negative pole, where it reddens, is the precise point where the developing hydrogen can subtract from it this principle; and from the positive pole the colouring matter is carried in the state of alkali to the other pole.

14. I have found another proof which confirms the opinion that the appearance of the red colour in erythric acid depends on the cause here assigned. Immersing red-hot iron nails in this acid, the red colour is immediately seen to appear. I also hoped to obtain a similar change with phosphorus. I put a small piece in erythric acid, and left it at the light of the sun; in the fluid no notable colour appeared, and the phosphorus only acquired a violet hue. Neither was the action of fire fit in this case to make the fluid become red.

Erythrats of Lime and Barytes.

15. Erythric acid poured into lime-water, as before observed, makes it very turbid; with the addition however of fresh erythric acid it is dissolved, but not with that of any other acid although weak, not even the carbonic acid, which is capable of decomposing the erythrat of lime. This salt is found in the form of light, white flakes, which are seen suspended in the fluid, and even rise to the surface if any extraneous substance is found in it.

16. This erythrat of lime has scarcely remained any time in contact with the air, when it experiences a change. It is found that at its expense a carbonat is formed, judging from the vivid effervescence

effervescence which takes place when dilute sulphuric acid is poured on it.

17. Dissolving the erythrat of lime in an excess of acid, a transparent, tasteless fluid is obtained. The oxalic acid discovers the lime, but to that carbonic acid must not be added; the alkaline carbonats however immediately render it turbid. Alcohol produces a similar effect, but it appears that it separates the neutral erythrat. The acidulous salt slowly evaporated yields crystals, in which the acids produce no effervescence whatever. The solution of these crystals does not become turbid with lime-water, and it has lost all the characters of the original salt. It being necessary to examine the changes which occur in erythric acid when united to bases, we must leave for the present the investigation of this phænomenon.

18. A drop of erythric acid produces a copious precipitate in barytic water. This erythrat presents phænomena analogous to those before observed in erythrats of lime. The erythric acid decomposes rapidly the sulphuret of barytes, and yields a violet colour.

Erythrats of Potash and of Soda.

19. Caustic potash immersed in the erythric liquid produces no precipitate, nor any remarkable change of colour; but their combination has a very sweet taste. The erythrat of potash renders lime-water turbid, and precipitates some metallic solutions, such as that of lead and silver, in white coagulated matter; on the contrary, with others it forms soluble coloured compounds, among which are distinguished the products from the solutions of iron by their beautiful blue colours.

20. The erythrat of potash changes its nature very easily. In fact, on examining it some time after it was formed, it was found that from being neutral, or even somewhat alkaline, it had become acidulous, as was indicated by turnsole; nor would it yield a blue colour with the salts of iron, unless some drops of potash were newly added.

21. The same and perhaps still more rapid changes take place, if the neutral erythrat be exposed to the light of the sun. In this state it is generally seen to become yellowish, and afterwards to redden. Removed from the solar light, and after a time, it loses the colour it had acquired; but if it remains exposed to that light it finally becomes a solid mass, sweet, of a lively red, and possessing much tenacity; dissolved in water, it gives to the liquid its beautiful colour. This red liquor does not so easily lose its colour by the action of the fire, as happens when it has no potash; but for this effect it is necessary to add much water.

22. The action of a moderate fire accelerates the above effects, the red hue appearing and becoming still deeper. Towards the conclusion

conclusion of the evaporation, if the fire ceases to act, a very tenacious sweet mass is obtained; but if the fire be continued, an abundant red spume is produced. This spume dissolves rapidly in water, disengaging numerous bubbles, and communicating the usual red colour: in alcohol it is scarcely soluble.

23. That the erythrat of potash is subject to an almost immediate change, is confirmed even by another proof. Putting potash on the crystals of erythric acid, they are dissolved; neutral erythrats are produced in the state of a white powder, which if left in contact with the air spontaneously reddens. In water, with which however it has not much affinity, it furnishes a solution which yields a blue colour with salts of iron; after some time it loses this property, and re-acquires it by means of the addition of fresh potash. But that which evidently demonstrates the change which take place, is the fact that, after the lapse of several hours, the deposit which was before in a great measure insoluble in water, becomes entirely dissolved if a small quanity of water be kept over it. This new solution requires the addition of potash to produce the blue colour with salts of iron.

24. The same things are produced, if instead of caustic potash carbonat be used. The erythric acid has the power of developing carbonic acid, and hence originates an alkaline erythrat, which like the others is subject to equal changes. Analogous phænomena are obtained with carbonat of soda; the erythrat of soda differs from that of potash in having a pungent taste, whereas the erythrat of potash is sweet.

25. From these it may be concluded, that erythric acid in contact with potash gives origin to a new acid endowed with a greater capacity of saturation, producing at the same time a peculiar matter which occasionally manifests itself with a red colour; circumstances which lead to the suspicion that the new acid may be even more oxygenated than the erythric. Let us see if the action of alcohol gives greater importance to this conjecture.

26. The erythrat of potash is so much the less soluble in alcohol the more it is alkaline. Thus, if in alcohol which contains dissolved potash erythric acid be poured, an abundant precipitate appears, which is speedily dissolved by the addition of a little more erythric acid, and again reappears by adding fresh alcohol. This, therefore, does not alter the erythrat of potash in which the acid is in excess. The alcohol precipitating the erythrat deposits beautiful arborizations, which are formed of uniform shining crystals. These rapidly dissolve in water, and give it a sweetish taste. The solution is highly alkaline, does not precipitate lime-water, nor give a blue colour with solutions of iron; it acquires in no manner a red colour, and contains, indeed, an acid

acid very different from the erythric. Alcohol keeps dissolved a peculiar matter which is discovered by evaporation. If the action of the heat be not too strong, it leaves a colourless mass which is very tough; otherwise it is reduced to a very white spume, as we have seen occurring in the erythrat of potash, where the alkali was in excess*. This spume dissolves in water, evolving many bubbles, and burns with all the phænomena which accompany the combustion of animal substances.

27. If, instead, alcohol be poured on erythrat of potash already altered, in this case it also becomes turbid, notwithstanding its acidity. The crystals which it produces are cubic, and dissolve in water. The solution is sweet, neutral, and, like the others, presents no phænomena which could induce the belief that erythric acid is present. In such cases, the alcohol with evaporation at the fire becomes red, and is finally converted into a red spume.

28. The erythrat of potash reddened by heat becomes violet with the addition of fresh potash†: thus the red erythric acid takes the same colour by adding to it an excess of potash. This combination left in the air loses its colour and crystallizes, giving origin to a salt similar to that which is obtained by means of alcohol. In like manner, if an alkaline solution of an alkalinule erythrat of potash be left to evaporate, we obtain crystalline groups of the same salt, that are involved in a glutinous matter.

Erythrat of Ammonia.

29. We recognise in this erythrat, phænomena analogous to those which were observed in the preceding. In adding ammonia to erythric acid, the union is accompanied by no sensible phænomenon. This salt precipitates with lime, yields a blue with solution of iron, but after a time loses this property, which fresh ammonia restores: exposed to the sun it reddens.

30. Ammonia poured on crystals of erythric acid dissolves it, and it becomes yellow; afterwards it spontaneously grows turbid, deposits yellow flakes, and remains of a rose colour, transmitting at the same time a peculiar disagreeable odour. Those yellow flakes dissolve in water, and give it a rosy colour; the solution possesses slightly the property of colouring salts of iron, and afterwards loses it.

* It is remarkable that the solid erythrat of potash, which often spontaneously reddens, yields a neutral solution, which does not redden on exposure to heat, but also produces this very white spume.

† The addition of ammonia to red erythrat of potash produced a very surprising phænomenon; it developed a disagreeable odour, and immediately, or after some time, yielded a black powder mixed with a substance which exposed to the light of the sun presented the beautiful green colour of the emerald. This singular change sometimes did not succeed, for which I can assign no reason.

Erythrat of Iron.

31. Erythric acid combined with iron presents so numerous and variable phænomena, that to explain them would require exclusively a long study. Boiling erythric acid over iron filings, the metal dissolves, and the solution varies in colour according to the concentration of the acid, and the action more or less strong of the fire. Thus sometimes it is yellow, sometimes purple, and sometimes of a most beautiful blue colour. The latter however it acquires in every case, by means of adding an alkali, which does not produce any other precipitate.

32. Similar combinations are obtained by boiling erythric acid over black oxide of iron. It seems, however, that in such a case we cannot immediately obtain the blue colour, but always if it had the citron yellow. To have that colour, the addition of an alkali is also necessary: after some time, indeed, the blue disappears, and a colour similar to the first returns.

33. Erythrat of iron concentrated by heat leaves green bands and deposits yellow grains, which with slow evaporation may also be crystallized in close prisms. It is worthy of remark, that in this erythrat, what the heat of the fire cannot do, that of the sun can; that is, communicate to it the blue colour. It is however fugitive, the yellow colour returns, and is ready to again become blue if exposed to alkali or the sun, and in these changes only a black powder is seen to be deposited. Erythrat of iron long exposed to the action of the sun is entirely in blackish matter, from which water is tinged red.

34. Erythric acid unites even cold with peroxide of iron: this solution has a yellow colour: if it be deep, alkali in a small dose produces a turbid coagulation, which on the addition of alkali dissolves and becomes blue. This coagulated matter, if left a long time quiet, spontaneously dissolves, and a yellowish matter reappears.

35. The erythrats of iron exposed to the electric current at the negative pole, cover the platina wire with a blue crust which afterwards tinges the whole liquid. This singular fact unites these phænomena with those which we have before observed in the simple erythric acid, and demonstrates that the various colourings to which this acid is subject, depend on a common cause modified by the bodies with which it is found in contact.

36. These experiments, although very incomplete, seem to me sufficient to show, that the blue colour which often accompanies the erythrats of iron is not a property of these, but most likely it belongs to a substance generated in the act in which the erythric acid changes into that other acid of which we have before spoken. In the passage of one acid to another originates, if

if I may use the expression, a secretion which in company with the erythrats of iron often tinges blue. It is with much reason that the alkalies make this colour appear, as they expressly promote such a secretion, which in their union with erythric acid is accustomed to manifest itself by a rose colour.

37. It is very probable that the alkalies with erythrats of iron may constitute triple salts; this may also happen with other metallic erythrats, as will presently appear. The triple prussiat of potash and the decoction of galls discover iron, from the erythric solutions producing the customary colours*.

Erythrat of Lead.

38. I have already observed that the erythrat of potash decomposes the solution of lead, and forms a white precipitate. A similar precipitate is likewise obtained from the same solution decomposed by the erythrat of potash, in which the acid is said to be altered. We shall now see if we can directly obtain these two different species of erythrat of lead, which will confirm our opinion on the mode with which erythric acid acts with bases.

39. Pouring erythric acid on litharge, it is only necessary to agitate them a little in order to produce their combination. Thick clouds are seen in the liquid, and the erythrat of lead, which is precipitated, is insoluble in acetic acid, and even in an excess of erythric acid itself. If this erythrat of lead be decomposed by sulphat of iron, and potash be afterwards added to it, the blue colour is obtained.

40. But if the turbid fluid obtained by agitating erythric acid with litharge be exposed to heat, after a slight ebullition the turbidness disappears, and the solution assumes a red colour. Continuing to boil it, the turbidness returns, and deposits a white powder, which is the erythrat of lead in which the acid is altered. The red liquor has a considerable quantity of it in solution, but simple water dissolves much less; therefore the red liquor with the addition of sulphat of iron and potash gives the blue colour, which is not produced by the aqueous solution.

41. From the second erythrat of lead may be extracted that acid which is generated at the expense of the erythric. This salt may be decomposed with dilute sulphuric acid in such a manner

* Whence is it that muriatic or hydrochloric acid, boiled on the most pure uric acid, gives a blue colour with triple prussiat of potash to such a degree as to create the belief that uric acid always contains iron? Can it ever be that these two acids should produce the same blue substance which was observed in the erythrats of iron? I shall only observe that Scheele's assertion of muriatic acid boiling on uric acid without any alteration, does not seem very correct. If the experiment be made, it will be found that caustic potash will develop from uric acid a very distinct odour of ammonia, which is a proof of its being in some degree altered.

that

that no turbidness results from the above solution of lead, nor any other of this metal; thus a colourless liquid of a pungent acid taste will be obtained. It reddens turnsole, but does not render lime-water turbid: united with potash, having acquired a sweet taste, it becomes insoluble in alcohol, and in a word possesses all those characters which belong to erythrat of potash spontaneously altered.

42. Erythric acid also attacks metallic lead, making the former boil on the latter: the acid after some time reddens, and it is discovered by sulphuric acid, that it contains lead in solution.

Other metallic Erythrats.

43. Erythric acid agitated with red oxide of mercury becomes turbid, and much more so by the action of fire. In time no colour appears in this, the salt is deposited in the progress of evaporation, and the liquid abandons it copiously in cooling; of the mercurial salt a very little remains in solution, and the precipitate is then entirely insoluble in distilled water.

44. In a similar manner the erythric acid acts with oxide of silver. Boiled also on the flowers of zinc, it dissolves the metal without changing colour, and becomes very turbid in cooling. Boiling it on the contrary over metallic zinc, it assumes a yellow colour on uniting with it. Potash at first produces a precipitate in this solution; but afterwards on adding it, the whole dissolves, and takes a beautiful rose colour.

45. Erythric acid boiled over copper acquires a yellow colour without dissolving any. It unites however in the cold way with the brown oxide. The erythrat of copper has a green colour; it crystallizes elegantly in the form of the plumage of feathers, gives a blue colour with ammonia, and, what is very singular, likewise yields a blue colour even with potash without producing any precipitate. Thus, potash forms with the solutions of metallic erythrats combinations still soluble, which renders it presumable that a triple salt is formed.

CONCLUSION.

46. No one who considers the effects of nitrous on uric acid can see them without surprise at the multitude of products which are derived from them. The greater part of these, however, is the fruit more naturally peculiar to other bodies; on the other hand, erythric acid is that which exclusively belongs to the decomposition of uric acid; and hence is the more valuable and eminent product. Erythric acid is a substance so singular for its physical changes, rather than for its chemical properties, that the lovers of the natural sciences will willingly make it the object of their study. In these observations I only proposed to myself to recognise and account for the principal phænomena which erythric

thric acid presents; many others remain to be noticed which my limited time did not permit. I have demonstrated that the reddening of erythric acid depends on a loss of oxygen; that in the union of this acid with bases the same modified causes produce various colours, and that contemporaneously the erythric acid is transformed into another peculiar acid. Though these facts are not perhaps in all parts proved with the highest rigour, yet I hope that they have such a degree of probability, that chemists will not refuse to admit their truth, until new researches shall demonstrate their fallacy.

Appendix.

In the first part of this memoir I have noticed some phænomena which occurred in the red washings of the spots made by a solution of uric in nitrous acid. Now that erythric acid is known, it is much easier to account for these. I had then observed that the washings exposed to a strong fire lost their colour. This, indeed, ought to take place, heat having the power of dissipating or destroying the red colouring matter (8·9). I have then found that the alkalies and earths renew this colour. This may be easily explained, admitting that the very small quantity of erythric acid (7) which remains in those solutions is found disposed to become altered from the bases, and hence to produce new colouring matter (21). Finally, I have remarked that some metallic solutions precipitate the colouring matter, rendering it of a violet colour, and others make it yellow without producing a precipitate: for this I cannot adduce any plausible reason, and only consider that the solutions of those metals, which with erythric acid produce insoluble salts, are those which precipitate the colouring matter. Of the rest, I must frankly confess that the above-mentioned appearance and disappearance of colours are not always constant: this, however, in such compound and voluble substances is not very surprsing.

VI. *Account of an electrical Increaser for the unerring Manifestation of small Portions of the Electric Fluid. Invented by* Henry Upington, *Esq. of Blair's Hill, Cork. Communicated by Dr.* Pearson.

Letter from Mr. Upington to Dr. Pearson.

Blair's Hill, Cork, Feb. 24, 1817.

Sir, — The electrical *increaser* for the unerring manifestation of exceedingly weak and small portions of the electric fluid, respecting which I took the liberty of addressing you on the 4th instant, was constructed by myself in the year 1810, for my private experiments; and at the same time I communicated its properties

properties to the late Earl Stanhope, in the course of a correspondence with which for many years he had honoured me.

I am very much obliged by your polite offer of presenting Mr. Tilloch with certain explanatory extracts from my letters to his Lordship upon this subject; which extracts I now inclose you, together with a drawing of the *increaser*.

Earl Stanhope, after reading my letters "with attention," was pleased to consider the instrument of "great utility;" and I shall feel much gratification should it be so esteemed by you, sir, as well as by every other scientific person for whose perusal this paper is intended.

I have the honour to be, sir,
Your most obedient servant,
HENRY UPINGTON.

George Pearson, Esq.
M.D. F.R.S., London.

Extracts from Mr. U.'s Letters to Earl Stanhope.

"Blair's Hill, Cork, Oct. 12, 1810.

"Your Lordship must no doubt be aware, that although our most improved condensers [composed of two parallel perpendicular metallic plates of six inches diameter each, one insulated, the other not; connected at pleasure with two similar plates of one inch and a half diameter each, attached to a gold-leaf electrometer] will discover the existence of exceedingly weak and diffused electricity (atmospheric for instance), yet a minute portion of excited fluid, such as that produced by the contact of two small pieces of metal, too weak to affect an electrometer, must require the assistance of an *increaser* to manifest its presence.

"Your Lordship must also be aware, that every instrument of this kind hitherto invented, including even *Cavallo's* among the number, is so very defective as scarcely to merit our consideration; while that species of multiplier called *a doubler* is totally useless, affording, as it does, the most equivocal results. The ingenious philosophical instrument-maker Mr. Cuthbertson was indeed so sensible of the inefficacy of every known doubler and multiplier, that he has not even hinted at either *term* in his recent publication called "*Practical Electricity.*" An instrument, therefore, to answer this desirable end has much engaged my attention; and I have the pleasure to say that I have succeeded to my wish.

"To give your Lordship a comprehensive idea of its properties, I shall view it in a three-fold capacity, viz. as *a source*, *a carrier*, and *a reservoir*; the stationary brass plate A of one inch and three quarters diameter, vertically erected on a varnished glass pedestal, serving as the *source*; a revolving insulated brass plate

plate B of same diameter serving as the *carrier*; and the combined condensers *c* C (the former one inch and three quarters, the latter six inches in diameter) as the *reservoir*.

"In using this instrument, it is obvious that the subject intended for examination must be brought in contact with the *source* A, where the fluid thus communicated will undergo, if the air be dry and the insulation good, but very little dissipation for a minute and upwards. Every time the *carrier* B passes by (that is, directly opposite and within a suitable distance of) this *source*, the projecting pin of B the *carrier* must touch the little lever of the perpendicular uninsulated brass rod D, and part it instantaneously: thus, without depriving the *source* of any portion of its fluid, there is imparted to the *carrier* an apparently equal portion of the *opposite* electricity, which it deposits on the *combined reservoir c* C by lifting the projecting lever of the smaller plate *c*.

"I have made repeated experiments with this instrument, and found that if the *reservoir* C be full six inches diameter and properly adjusted, the combined reservoir *c* C will retain, provided the communicated electricity be sufficiently weak, about 250 deposits: but here it should be observed, that if the surface of the body presented to the *source* for examination, be great, and its electricity weak, thé *carrier* should be set directly *opposite* the *source*, while the contact is made;—the source and carrier forming thus, in conjunction, a small condenser."

"December 13, 1810.

"In reply to your Lordship's observations, I can assure you that I have left nothing undone to bring this instrument to all possible perfection. I found by repeated trials that about 250 deposits were the absolute *ne plus ultra*. When more were attempted, so as to attain the *complete maximum* of the reservoir, the instrument appeared less perfect, the result being sometimes equivocal; for do what we please, the electric substances which must necessarily be used to insulate the plates, will on certain occasions retain for a considerable time a sufficient residuum to affect our electrometer: how, therefore, should the *communicated* electricity be distinguished from the *inherent?* Upon no occasion, then, should the operator proceed without previously ascertaining what number of revolutions may, at that time, be commanded. I recollect, one frosty day, to have produced *spontaneous* electricity (whether from the air, the earth, or pillars, I cannot tell,) by 230 deposits, which consequently obliged me to limit my revolutions to 200, during an experiment which I was then performing.

"From all the foregoing facts it would appear, my Lord, that a larger reservoir than that of C would be quite unnecessary, the com-

communication of too great a number of deposits having no other tendency than that of marring our operations. The size of my present one, as I have already said, is about six inches in diameter; and even with *this*, the pillars must neither be over-heated nor rubbed: while, to fit the instrument immediately for a second experiment, it will be necessary to touch every plate [all the plates are unvarnished *brass*] for at least a dozen seconds, with a *similar* metallic substance.

"I must not close this letter without pointing out to your Lordship a striking circumstance which for a long time escaped my observation. If the electricity communicated to the *source* be tolerably *intense*, it will be almost if not wholly *imperceptible:* it will overcome the resistance opposed by the plate of air, strike the *carrier*, and pass into the earth. A small piece of well excited sealing-wax would, in most instances, overcome a plate of air of one inch diameter and perhaps one-sixth of an inch in thickness. For the most delicate experiments, I should consider the one-fortieth of an inch about the distance best suited. This will discover the electricity of a piece of glass when moderately heated,—for *heat*, beyond denial, as this instrument will prove, excites to action the electric fluid in bodies of almost every description. *Cold*, as every electrician knows, produces an opposite effect: Water frozen to 13° below 0 upon Fahrenheit, becomes an electric.

"May not these well-ascertained facts be more closely applied than usual to the doctrine of *thunder?* and should we not therefore look to the torrid and frigid zones for the *grand* solution of the phænomenon?"

Explanation of the Plate.

COLOURS.

Brass unvarnished—represented by the	yellow.
Glass varnished	black.
Mahogany	brown.

Parts of the Instrument.

A. The *source*, a brass plate (one inch and three quarters diameter) to which the body for examination is applied.

B. The revolving *carrier*, a brass plate (one inch and three quarters diameter) which drawing the electric fluid from the *source*, without impoverishing that source, deposits the fluid so drawn upon the *reservoir*.

c C. The combined *reservoir*, likewise brass, the smaller plate *c* being one inch and three quarters diameter, the larger C six inches. The accumulated fluid must be ultimately concentrated upon the smaller one, by a process similar to that observed in using the ordinary *combined condensers*.

D. A brass

D. A brass pillar whose lever is depressed by the *carrier* B in its revolutions.

E. A multiplying wheel—very useful—to obviate, as much as possible, the effects of dissipation.

FF. The brass plates between which the *carrier* is mounted.

G. A countervailing weight, to render the movement of the *carrier* both light and equable.

H. A glass pillar; by taking hold of whose handle I, the hind plate of the smaller reservoir *c* may, when necessary, be turned backward. The elevated position of this pillar is most important; the ascending humidity of the hand, so fatal to all delicate experiments, and to which our ordinary condensing electrometers are subject, being thus avoided. On packing up the instrument, this pillar should be unscrewed, and taken from off the plate.

I. The handle of the glass pillar H just described.

K. A brass rod suspended from the hind plate of the smaller *reservoir c*, to which it is screwed. This rod is connected with, and disengaged from, the earth at pleasure, by means of the horizontal brass lever L, which turns from right to left upon a centre pin.

L. The horizontal brass lever just mentioned.

M. A gold-leaf electrometer, by which the accumulated fluid is examined. A rounded pin, which insures its temporary communication with the larger reservoir C, is concealed from the eye. The *chain*, though *bright*, by which it was accidentally connected with the smaller *c* at the time of drawing, is much inferior to a *polished wire* when delicate experiments are in question.

At the back of this instrument, in the base, adjusting screws are properly stationed to regulate the necessary distances between all the parallel plates; while, for the security of the hind plates of C and *c*, which may be turned backward by means of hinges attached to their respective bases, two suitable *stops* are introduced.

For the examination of atmospheric and every other kind of diffused electricity which yields a sufficient *quantity*, the combined reservoir *c* C may, when the operator pleases, be employed alone in the usual manner, like our ordinary condensers.

N. A glass pillar supporting a horizontal brass rod, which rod is situate at some distance behind all the plates.

OOO. The last-mentioned horizontal brass rod, occasionally connected by a chain or spiral brass wire with the back of the *source* A; thus enabling the operator (the electrometer being always in such case withdrawn from the larger reservoir C*), to throw back, by contact, the whole contents of the smaller *c* upon the *source*; converting the instrument, by this process, into a species of *doubler*. These latter parts of the instrument, N and OOO, must however be considered as mere objects of *curiosity*: all doubling operations (no matter what number of revolutions are employed) being subject to equivocal results.

* C may be said, in this instance, not to appertain to the instrument.

VII. *On the New Astronomical Circle at Greenwich.*

To Mr. Tilloch.

Sir,—On referring to the article "Transit Circle," in Dr. Rees's new Cyclopædia, I found the following observations relative to the new astronomical circle which was, a few years ago, fitted up by Mr. Troughton for the Royal Observatory at Greenwich. "Mr. Pond has already published one volume of Observations taken with this instrument, and has therein given a plan and section of the circle, which is six feet two inches in diameter. But, as Mr. Troughton intends to give a complete description thereof, himself, as a paper suitable for the Philosophical Transactions, which, to be published therein, must be an *original* communication, Mr. Pond was *not at liberty* to describe the drawing which he has given as a frontispiece to his first volume." And the writer goes on to state (what may be readily anticipated by any one), that as Mr. Pond had not thought proper to describe the circle, he (the writer) of course was prevented altogether from giving any description of it.

Why Mr. Pond was not at liberty to give a description of an instrument which was in his own possession, and which in fact belonged to him, in his official capacity, does not appear; but I should conceive it was merely a matter of courtesy between him and Mr. Troughton. As several years however have now elapsed, and Mr. Troughton has not given the promised description, it is presumed that the same reserve is not now necessary. Indeed, I have been informed that Mr. Pond has recently furnished the French with *a complete drawing and description of this instrument*; and that this circumstance had great weight in the distribution of the medals with which the astronomer royal has been lately honoured. This, if true, surely requires some explanation; and, if not true, ought to be formally contradicted. The instruments at the Royal Observatory at Greenwich are furnished at the public expense; and the Government have, with a munificence unequalled in former times, provided that esta-

blishment

blishment with many costly instruments. As the public money therefore has paid for these things, the public (I mean the *British* public, and not the people in *France*) have a right to know how that money has been expended: and I am persuaded that no scientific person will regret that a portion of the public expenditure of the country has been appropriated in this way. I am sure however that *every person* will regret that any information upon those subjects, for which he has so dearly paid, should be withholden from *his own* country and given to *another*. Mr. Pond must be aware that there are many scientific persons *in England*, who are desirous of seeing a more detailed account of the astronomical circle above mentioned, and who cannot gather that information from the partial account which he has at present thought fit to publish: and it would have been more satisfactory to them to see such a description first published in *this country*, than first to meet with it as an appendage to some of the future volumes of the *Connaissance des Tems*, or as a prominent article in one of the *foreign* journals.

I am, sir, your obedient servant,

July 14, 1818. ARISTARCHUS.

VIII. *On Chemical Philosophy. By Mr.* MATTHEW ALLAN, *Lecturer.*

[Continued from vol. li. p. 432.]

Essay VI.

I SAID in the last Essay, that it was necessary to be still more particular, in order to prove that the differences in phænomena arise not from different powers, but from the substances and circumstances, and the quantity and intensity of one power acting upon or in them;—that it was necessary to prove, by particular detailed explanations, that there is but "one grand agent in nature, which creates or destroys, unites or separates, preserves or diversifies, the forms of matter." And what can be more evident? Every body, in changing its form of existence, changes also its capacity for heat or for this power, or its capability to contain or retain a greater or less quantity. The difference of capacity, between these different states of existence, is exactly equal to the quantity necessary to produce the change, and of course to support the change of existence it has itself produced. For instance; If one pound of water heated to 172° will only melt one pound of ice, and be itself reduced to the same temperature, that of 32°, then water at 32° contains 140° more of caloric than ice at the same temperature; and if so, it is evident that 140° of caloric disappear and become latent in the solution and conversion of

ice

ice into water; and that this 140° is the difference in the quantity required in each of these different relative states of existence; the quantity, in short, which is necessary to produce the liquid form of water, as well as to support this change of form which it has produced. The same principle is seen in the further solution and conversion of this element into vapour, steam or gas: 950° of heat disappear and become latent in every portion of water passing into this state; so that though water and steam are both at the same temperature 212°, yet the steam contains 950° more of caloric than the same weight of water at that temperature: the caloric is hence said to become latent, or the energies of this quantity necessary to produce the change are occupied or suspended in supporting it in this new state of existence it has produced.

This I conceive is a clearer expression of *latent heat*, *free caloric*, *caloric* of temperature, *capacity*, &c., for it not merely expresses the fact, but the explanation of that fact at the same time. Indeed, in this respect, Dr. Black stated the fact, as far as it was then known to him, in such a clear and beautiful manner, that I wonder how this should ever have been a subject of controversy, or that Dr. Irvin and Dr. Cleghorn should imagine they gave any better explanation of it, by saying that "heat disappears and becomes latent, *not* in effecting the change, but as the effect of that change." This to me conveys a very imperfect conception of the change; it is at any rate a very confused, partial and imperfect expression of the fact. Heat disappears, not merely as a consequence, but as a cause. It is necessary to apply heat to effect the change, which heat disappears, and again reappears on the steam returning to its former state: what then can be so plain, as that the quantity necessary to produce the change, disappears or becomes latent, because it has to support the change produced?

It thus appears that one quantity is necessary for the solid, another for the liquid, and another for the gaseous states; and this quantity differing in every different species of matter, we find substances assuming the liquid and gaseous forms at every possible point of temperature. It would carry us too much into detail, to contemplate the beauty and utility of this law. In these Essays we shall therefore merely confine ourselves to the illustration afforded by the instance already mentioned. Give to ice the quantity necessary for the production of water; the properties and active energies of this quantity are then employed in supporting this new state of existence; it cannot therefore act upon any thing else, or produce the sensation or effects of free caloric; it is said to be latent. Similar also is the case with its further conversion into steam, which, like the gases, is a simple

solution

solution of a substance in caloric, with this difference, that the steam is separable from its solvent caloric at a temperature below 212°, and, of course, at the common temperature of our atmosphere. To prove that this is the difference between the permanent gases and vapour, I need only mention the fact, "that steam does not scald so much from high pressure as from low pressure:" this seems *à priori* contrary to our expectations; but it is a fact, that however high the temperature is raised above 212°, it will not scald or burn more than common atmospheric air heated to the same temperature, because the active energies of this power are occupied or suspended by the water held in solution. It is in fact air: but the moment it is liberated and lets go the water, then its energies are not suspended, but unoccupied; it instantly becomes free caloric, or caloric of temperature, and scalds or burns, or passes with all its energies into some other substance, producing its changes and effects upon it according to its quantity and intensity, and the nature of such substance; and as this separation more readily takes place at a low than at a high pressure (temperature), the one must scald or burn sooner than the other. In making the different gases by heat, we find the pipes in the first instance burning hot, and afterwards becoming comparatively cool, because the first thing which is driven off is steam; which being abstracted by the temperature of the surrounding medium, is set at liberty to act on the pipes, or any thing else that comes in its way; but when the permanent gases come over, which are not so separable, but require that we should have recourse to elective attraction to separate them, this same heat then becomes latent, and the pipes cool, and in this latent state carry the heat and flame in any direction we choose. When, however, as in carburetted hydrogen gas, it is made to unite with oxygen, this heat, together with that which holds the oxygen in solution, is liberated, and produces heat and flame. Thus, while some have said, heat and flame are from oxygen, others have said they are from hydrogen. I say they are from both. And how often do we find truth differing from and agreeing with all parties!

The fact is, the gases are formed by the solution of substances in this caloric, exactly on the same principle, only more attenuated, as ice is melted by heat, and forms water; and consequently when they change their form of existence, they give up this power, and when on some concentrated point, as is always the case when separated by elective attraction, heat and flame appear.

Had it not been for the impressions which the doctrine of attraction and repulsion had made on the mind, it would, I am persuaded, have been quite natural for the chemists of the time

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of Lavoisier, and the discovery of the gases, to consider and speak of the solution of substances in this power, as that which formed the gaseous state of existence. Indeed, in proof of this, we find them with great difficulty refraining from doing so, affording a remarkable instance how, even in the strongest minds, nature and truth will struggle with prejudice and error. "It is probable," says Lavoisier *, "that the separation of the particles of bodies, occasioned by caloric, depends, in a similar manner, on a certain combination of attractive powers, *which, in conformity with the imperfection of our knowledge, we endeavour to express by saying that caloric communicates a power of repulsion to the particles of bodies.*" In another place he says: "It is extremely difficult to form an accurate notion of this repulsion acting upon very minute particles, placed at great distances from each other." In my opinion, it is utterly impossible. Perhaps the common and modern notions of attraction and repulsion, either prevented him from seeing as much distinctly, or deterred him from expressing it; but he is often obliged to come very near the truth in such passages as these: "In each species of gas, I shall," says he, "distinguish between the caloric which in *some measure* answers the purpose of a solvent, and the substance which is in combination with caloric, and forms the base of the gas." Why say "*in some measure*," and not at once in plain terms *it is the solvent?* Indeed almost all the chemists of that period, notwithstanding this attachment to former views, generally speak of the solution of bodies in caloric: in fact, so simple and obvious is the idea, and so evidently must it have obtruded itself on their minds, that it is wonderful they could adopt any other mode of expression. What other reasons had they for adopting the word *caloric?* or how otherwise could the Lavoisierian theory of combustion be intelligible? Chaptal and Fourcroy speak of the solution of substances in fire and water as similar. Fourcroy says, "Caloric sometimes adheres so forcibly to bodies, that it prevents their combining with others. Thus many are dissolved into gas or other elastic fluids, as steam; some will neither unite with other bodies, nor with one another, so long as they retain this state of invisible solution in caloric; so that recourse must be had to double elective attractions, to effect their combinations." And again: "The attraction of caloric for some bodies is so great, that it is frequently employed with advantage for separating these substances from the compounds into which they enter, and for analysing and decomposing compound bodies. This is what we do in distillation, and in all the decompositions effected by fire alone, or caloric ap-

* See page 25, Elements.

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plied to compound substances." The different elements of these compounds are gradually dissolved in the order of their solubility; and when we come to chemical affinity, we shall show how this explains the decompositions and combinations of substances. It is evident that Fourcroy considered the gâses as the solution of substances in caloric; for he never speaks of the separation of the particles to a greater distance, as that which constituted the gaseous or liquid forms. Indeed Dr. Thomson has wisely given up this in the last edition of his System. It would therefore be the less necessary to insist on this, were it not that I conceive it is preparing the way for the explanation of some cases where no theory has yet been offered at all worthy of the name. I allude to galvanism in particular. How separating the particles of matter, by the power of repulsion, can ever produce the gaseous or liquid forms, is one of those things I could never understand, nor have I yet met with one who could. Men may profess to believe what is the faith of the schools; but unless the understanding be convinced, it is mere profession. The mere mechanical separation of the particles can obviously never change the quality of substances; but solution and combination with a power which produces every effect *must* change their properties.

The argument in favour of the doctrine of repulsion, which says, that but for such a power all bodies must be equally solid, is mere assertion without meaning—all bodies are not the same. It is said that the particles of matter do not touch each other, because some few can be pressed into smaller space; and that therefore they are held together in this state by some repulsive power: But neither of these inferences at all follows. Caloric fills up the insterstices, and caloric is something material; it may be beat or squeezed out, as all metals in this way evolve heat. It is very probable that the particles of matter themselves may not touch at all points;—but why talk about such points, when we neither know what they are, nor what they are like, as is too well proved by the various controversies respecting them? We shall, notwithstanding this, recur to the subject when we come to chemical affinity. Let us be careful, however, lest we waste our time and talents on mere speculative points. There is enough to do in philosophy without this.

"Heat," says Chaptal, "by combining with bodies, produces an effect the very opposite of attraction; and we might consider ourselves as authorized to affirm that it is a principle of repulsion, if *sound chemistry* had not proved that it produces these effects only by its endeavour to combine with bodies, and thereby necessarily diminishing the force of aggregation, as all other chemical agents do. Besides which, the extreme levity of caloric produces

produces the effect, that when it is combined with any body it continually tends to elevate it, and in this way overcome that force (gravitation) which retains it, and would precipitate it towards the earth."

I am, I repeat, the more anxious to establish this view, because I am convinced it affords the only satisfactory explanation of the operations and phænomena of nature and art. It is necessary to prove this, and first by an examination of Galvanism.

Colliergate, York, July 15, 1818.

[To be continued.]

IX. *Notices respecting New Books.*

Philosophical Transactions of the Royal Society of London, for the Year 1818. Part I.

The Transactions of the Royal Society since the commencement of the present year have been distinguished by several papers of great novelty and importance. The original and valuable experiments of Captain Kater on the pendulum, for which the Copleian medal was adjudged to him, are at the present moment particularly deserving general attention. We regret that the late period of the month at which this part of the Society's Transactions has appeared, obliges us to postpone till our next number, laying the particulars of these experiments before our readers. The following passage will show the general principle on which they have proceeded:

"Not feeling at all satisfied with the prospect which the use of a rod presented, I endeavoured to discover some property of the pendulum of which I might avail myself with greater probability of success; and I was so fortunate as to perceive one, which promised an unexceptionable result. It is known that the centres of suspension and oscillation are reciprocal; or, in other words, that if a body be suspended by its centre of oscillation, its former point of suspension becomes the centre of oscillation, and the vibrations in both positions will be performed in equal times. Now, the distance of the centre of oscillation from the point of suspension depending on the *figure* of the body employed, if the arrangement of its particles be changed, the place of the centre of oscillation will also suffer a change. Suppose then a body to be furnished with a point of suspension, and another point on which it may vibrate, to be fixed as nearly as can be estimated in the centre of oscillation, and in a line with the point of suspension and centre of gravity. If the vibrations in each position should not be equal in equal times, they may readily

readily be made so, by shifting a moveable weight, with which the body is to be furnished, in a line between the centres of suspension and oscillation; when the distance between the two points about which the vibrations were performed being measured, the length of a simple pendulum, and the time of its vibration, will at once be known, uninfluenced by any irregularity of density or figure."

The following, including this paper of Capt. Kater's, are the contents of the present part of the Transactions:

"I. On the great Strength given to Ships of War by the Application of Diagonal Braces. By Robert Seppings, Esq. F.R.S. —II. A Memoir on the Geography of the North-eastern Part of Asia, and on the Question whether Asia and America are contiguous, or are separated by the Sea. By Captain James Burney, F.R.S.—III. Additional Facts respecting the fossil Remains of an Animal, on the Subject of which two Papers have been printed in the Philosophical Transactions, showing that the Bones of the Sternum resemble those of the Ornithorhynchus paradoxus. By Sir Everard Home, Bart. V.P.R.S. — IV. An Account of Experiments for determining the Length of the Pendulum vibrating Seconds in the Latitude of London. By Capt. Henry Kater, F.R.S.—V. On the Length of the French Mètre estimated in Parts of the English Standard. By Captain Henry Kater, F.R.S.—VI. A few Facts relative to the colouring Matters of some Vegetables. By James Smithson, Esq. F.R.S.—VII. An Account of Experiments made on the Strength of Materials. By George Rennie jun. Esq. In a Letter to Thomas Young, M.D. For. Sec. R.S.—VIII. On the Office of the Heart Wood of Trees. By T. A. Knight, Esq. F.R.S. In a Letter addressed to the Right Hon. Sir Joseph Banks, Bart. G.C.B. P.R.S.—IX. On circulating Functions, and on the Integration of a Class of Equations of finite Differences into which they enter as Coefficients. By John F. W. Herschel, Esq. F.R.S.—X. On the Fallacy of the Experiments in which Water is said to have been formed by the Decomposition of Chlorine. By Sir H. Davy, LL.D.F.R.S.—XI. The Croonian Lecture. On the Changes the Blood undergoes in the Act of Coagulation. By Sir Everard Home, Bart. V.P.R.S. — XII. Some Additions to the Croonian Lecture, on the Changes the Blood undergoes in the Act of Coagulation. By Sir Everard Home, Bart. V.P.R.S. —XIII. On the Laws of Polarisation and double Refraction in regularly crystallized Bodies. By David Brewster, LL.D. F.R.S. Lond. and Edin. In a Letter to the Right Hon. Sir Joseph Banks, Bart. G.C. B. P.R.S."

Transactions of the Royal Society of Edinburgh. Part II. Vol. VIII.

The Part of this Society's Transactions now published contains the following interesting articles:

"On the Effects of Compression and Dilatation in altering the polarising Structure of doubly refracting Crystals. By David Brewster, LL.D. F.R.S. London and Edinburgh.—Experiments on Muriatic Acid Gas, with Observations on its chemical Constitution, and on some other Subjects of chemical Theory. By John Murray, M.D. F.R.S. Edinburgh.—Experiments on the Relation between the Muriatic Acid and Chlorine; to which is subjoined, the Description of a new Instrument for the Analysis of Gases by Explosion. By Andrew Ure, M.D. Professor of the Andersonian Institution and Member of the Geological Society.—On the Laws which regulate the Distribution of the polarising Force in Plates, Tubes, and Cylinders of Glass that have received the polarising Structure. By David Brewster, LL.D. F.R.S. London and Edinburgh.—Remarks illustrative of the Scope and Influence of the philosophical Writings of Lord Bacon. By Macvey Napier, Esq. F.R.S. London and Edinburgh, and F.A.S. Edinburgh.—Sketch of the Geology of the Environs of Nice. By Thomas Allan, Esq. F.R.S. Edinburgh.—On certain Impressions of Cold transmitted from the Atmosphere, with the Description of an Instrument adapted to measure them. By John Leslie, F.R.S. Edinburgh, and Professor of Mathematics in the University of Edinburgh.—A Method of determining the Time with Accuracy, from a Series of Altitudes of the Sun, taken on the same Side of the Meridian. By Major-general Sir Thomas Brisbane, Knight, F.R.S. Edinburgh.—Observations on the Junction of the Fresh Water of Rivers with the Salt Water of the Sea. By the Rev. John Fleming, D.D. F.R.S. Edinburgh.—Memoir of the Life and Writings of the Honourable Alexander Fraser Tytlar, Lord Woodhouselee. By the Rev. Archibald Allison, LL.D. F.R.S. London and Edinburgh."

Elements of Chemical Science as applied to the Arts and Manufactures and Natural Phænomena. By J. Murray. pp. 294.

The work before us aims at no other praise than what we may safely accord it, that of exhibiting a lucid and comprehensive view of the principles of chemistry. The difficulties which stand in the way of any systematic arrangement of chemical phænomena are many and formidable; but Mr. Murray has nevertheless succeeded in forming a disposition of materials, which, distinguished by a good deal of novelty, conveys a very clear idea of the nature

ture and properties of the subjects treated. The system of arrangement the author has adopted is founded on electrical affections, and is consequently well calculated to facilitate the study of electro-chemical science. The work in this point of view is particularly deserving the attention of the chemical student, and is altogether a production which does much credit to the well-known ingenuity and research of its indefatigable author.

Memoirs of the Life and Writings of BENJAMIN FRANKLIN, *LL.D. F.R.S. &c. Written by himself to a late Period, and continued to the Time of his Death by his Grandson* WILLIAM TEMPLE FRANKLIN. Vol. III. 4to. pp. 570. 1818.

The present volume, which is the last of a very valuable and important work, comprehends the select political, *philosophical* and miscellaneous writings of Franklin. Some of the essays contained in the philosophical branch have already appeared; but by far the greater portion of it, including several of the latest and most ingenious of the Doctor's writings, are stated to be now for the first time printed from his own manuscripts.

Mr. W. Westall, who accompanied Captain Flinders on his voyage round the world, has lately executed a work, consisting of a variety of Views of the Caves in the North-west Riding of Yorkshire, with some of the most interesting scenes in their neighbourhood, particularly Malham Cove and Gordale Scar.

They are not only highly picturesque, but appear to be geologically correct representations of some of the most extraordinary scenes in this country; and it is a strange circumstance, that no work upon the same subject has before appeared, as the caves have for some years past attracted a great many visitors.

An Essay, which Dr. Jos. de Matthæis read in the Archæological Society at Rome, on the 29th of Feb. 1818, has now been published under the title of *Sull' origine de numeri Romani.* The author attempts to prove that the Roman numerals, as well as the ancient Etruscan, originated in the nails which these nations, in the earlier periods of their history, caused to have annually fixed by their magistrates, for other than chronological purposes, in the Temple of Jupiter, and in that of Nurtia, their Goddess of Fortune, at Vulsinium (Bolsena).

Just published, A Guide to Botany; or A familar Illustration of the Linnæan Classification of Plants; with coloured Plates. By James Millar, M.D. Editor of the Encyclopædia Edinensis, and of the 4th Edition of the Encyclopædia Britannica.

X. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 4. A PAPER by Sir Everard Home was read, containing a description of the Delphinus Gangeticus; also another paper by Dr. Granville, giving an account of the production of sulphuretted Azote in the abdomen by the decomposition of an albuminous dropsical fluid;—and a third paper by John Williams, Esq. explaining the influence of Galvanism upon germination.

June 11. A paper was read by Dr. Prout, describing a new acid principle prepared from the lithic or uric acid, which he denominates the *purpuric* acid.

Another paper by Sir William Herschel was read, On the relative distances of clusters of Stars, and the power of Telescopes in discerning celestial objects.

The President then adjourned the Society for the long vacation.

ROYAL INSTITUTE OF FRANCE.

ACADEMY OF SCIENCES.

Prize proposed—*To determine the chemical changes which fruits undergo during maturation, and afterwards.*

For the solution of this question it will be necessary to examine with care the influence of the atmosphere which surrounds the fruits, and the alterations which they receive from it.

The essayist may confine his observations to fruits of different species, provided consequences general enough can be drawn from them.

The prize will be a gold medal of the value of 3000 fr. The term of competition is limited to the 1st January 1819.

XI. *Intelligence and Miscellaneous Articles.*

CASE OF THOMAS GASKING.

To Mr. Tilloch.

IT is impossible to ascertain, otherwise than by casual observation, with what rapidity the powers of the human mind may be unfolded; and how closely infancy can be taught to pursue the footsteps of manhood in the arduous paths of science. Master (now Dr.) Crotch formerly astonished the amateurs of music, when five years of age, by his practical skill in that pleasing art; and Zerah Colburn lately attracted universal attention by by his uncommon proficiency in mental arithmetic. These instances of premature genius are surprising; but they are examples of practical knowledge, with which the reasoning faculty has little to do. Experience seems to show, that this distinguishing property of the mind requires time to unfold itself; and that it is as unreasonable to expect fruit from a tree before it has blossomed, as to look for a correct judgement in an infant. The maxim is admitted to be general, but it is not without exception; for a child nine years old is at present in Kendal, who has, by his progress in mathematics, united reason to practice. Thomas Gasking is the son of an industrious and ingenious journeyman shoemaker, of Penrith; and I now proceed to notice his literary attainments, which he has acquired in the course of two years. He has learned to read correctly and gracefully; he writes a good hand with surprising expedition; and he has made some progress in the English grammar. The boy went through this part of his education in a day-school at Penrith; but he is indebted for his mathematical knowledge to the tuition of his father, who, though in low circumstances, has laudably dedicated his hours of leisure to scientific pursuits, as I am informed. Little Gasking seems well acquainted with the leading propositions in Euclid; he reads and works algebra with the greatest facility, and has entered upon the study of fluxions. I am aware that this report will appear incredible to those who are acquainted with the different subjects which have been enumerated; but the following instance of his wonderful proficiency will, in all probability, remove any doubts that competent judges may entertain. A stranger gentleman, who was invited, with myself, to examine the boy, requested him to demonstrate the thirteenth proposition of the first book of Euclid; which he did immediately. The demonstration of the twentieth proposition of the same book was next proposed: he drew out the figure; and though he failed in his first attempt, he soon recovered the train of reasoning, and went through the demonstration correctly. Being asked, if he had two sides of a triangle and the angle included given, how he would proceed to find the third side? the process appeared quite familiar to him, and we found, upon inquiry, he was acquainted with logarithms, and was able to use them. In spherical trigonometry, he solved two cases of right-angled triangles by Lord Napier's rules. His skill, and the rapidity of his operations, in algebra, created more surprise than his knowledge of geometry;—he solved a number of quadratic equations with the greatest ease, and extracted the square roots of the numbers which resulted from his operations. Several questions were put to him which contained two unknown quantities; these he also answered without difficulty. Being asked if he had been taught

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the application of algebra to geometry, he answered in the affirmative, and immediately solved the following problem:—Given one leg of a right-angled triangle, and the excess of the hypothenuse above the other leg, to construct the triangle. He answered two or three problems relating to the maxima of numbers and of geometrical magnitudes with ease, and took the fluxions, which were not difficult, correctly. When the age of this child is compared with his scientific attainments, we can look on him in no other light than as a literary phænomenon, who promises to become an ornament to one of the British Universities, unless his progress should unfortunately be checked by indigence, or the vigour of his mind should be enfeebled by some sinister accident. JOHN GOUGH.

STEAM ENGINES IN CORNWALL.

From Messrs. Leans' Report for June 1818, it appears that during that month the following was the work performed by the engines reported, with each bushel of coals.

	Pounds of water lifted 1 foot high with each bushel.	*Load per square inch in cylinder.*
24 common engines averaged	23,836,654	various.
Woolf's at Wheal Vor ..	30,336,482	17·3 lib.
Ditto Wh. Abraham ..	34,352,013	16·8
Ditto ditto	34,846,939	6·0
Dalcouth engine	38,143,428	11·3
Wheal Abraham ditto ..	34,291,588	10 9
United Mines engine ..	30,165,260	15·8
Treskirby ditto	42,098,797	10·8
Wheal Chance ditto ..	35,797,348	11·5

NEW SOUTH WALES.

A discovery has been made in New South Wales, which must materially affect the future advancement of that colony. "A river of the first magnitude" has been found in the interior, running through a most beautiful country, rich in soil, limestone, slate, and good timber. A means of communication like this, has long been anxiously searched for without success, and many began to entertain an apprehension that the progress of colonization in New Holland would be confined to its coasts.

Mr. Oxley, the surveyor-general, was sent out with a party in an expedition to the westward of the Blue Mountains, to trace the course of the lately discovered river Lachlan, and to ascertain the soil, capabilities, and productions, of the country through which it was expected to pass in its course to the sea. Mr. Oxley left Bathurst on the 30th April 1817. He proceeded down the Lachlan until the 12th May, the country rapidly descending until

until the waters of the river rose to a level with it, and, divided into numerous branches, lost itself among the marshes. Mr. Oxley quitted the river on the 17th May, taking a S.W. course towards Cape Northumberland. He continued this course until the 9th June, when he was induced to change his course to north. On this course he continued till the 23d June, when he again fell in with a stream, which he could with difficulty recognise as the Lachlan, it being little larger than one of the branches of it where it was quitted on the 17th May. He kept along the banks of this stream till the 8th July, when the whole country became a marsh altogether uninhabitable. This unlooked-for and truly singular termination of a river, filled the party with the most painful sensations. They were full 500 miles west of Sydney, and nearly in its latitude; and it had taken them ten weeks of unremitted exertion to proceed so far. Returning down the Lachlan, he recommenced the survey of it from the point on which it was made the 23d June. The connexion with all the points of the survey previously ascertained, was completed between the 19th July and the 3d August. It was estimated that the river from the place where first made by Mr. Evans, had run a course, taking all its windings, of upwards of 1200 miles, a length of course altogether unprecedented, considering that the *original* is its only supply of water during that distance.

"Crossing at this point," says Mr. Oxley in his Report, "it was my intention to take a N.E. course to intersect the country, and if possible to ascertain what had become of the Macquarrie River, which it was clear had never joined the Lachlan. This course led us through a country to the full as bad as any we had yet seen, and equally devoid of water, the want of which again much distressed us. On the 7th August the scene began to change, and the country to assume a very different aspect. We were now quitting the neighbourhood of the Lachlan, and had passed to the N.E. of the high range of hills which on this parallel bounds the low country to the north of that river. To the N.W. and N. the country was high and open, with good forest land; and on the 10th we had the satisfaction to fall in with the first stream, running northerly. This renewed our hopes of soon falling in with the Macquarrie, and we continued upon the same course, occasionally inclining to the eastward, until the 19th, passing through a fine luxuriant country well watered, crossing in that space of time nine streams, having a northerly course through rich valleys, the country in every direction being moderately high and open, and generally as fine as can be imagined.

"No doubt remained upon our minds that those streams fell into the Macquarrie, and to view it before it received such an accession was our first wish. On the 19th we were gratified by falling in with a river running through a most beautiful country, and which I should have been well contented to have believed the river we were in search of. Accident led us down this stream about a mile, when we were surprised by its junction with a river coming from the south, of such width and magnitude as to dispel all doubts as to this last being the river we had so long anxiously looked for. Short as our resources were, we could not resist the temptation this beautiful country offered us, to remain two days on the junction of the rivers, for the purpose of examining the vicinity to as great an extent as possible.

"Our examination increased the satisfaction we had previously felt. As far as the eye could reach in every direction, a rich and picturesque country extended, abounding in limestone, slate, good timber, and every other requisite that could render an *uncultivated* country desirable. The soil cannot be excelled; whilst *a noble river of the first magnitude* afforded the means of conveying its productions from one part to the other. Where I quitted it, its course was northerly, and we were then north of the parallel of Port Stephens, being in latitude 32° 45′ S. and 148° 58′ E. longitude."

The course and direction of this river is to be the object of an early expedition.

MINERALOGY.

To Mr. Tilloch.

Sir,—Permit me to submit to your consideration, and to that of your mineralogical readers, the following remarks and suggestions respecting a mineral allied in some respects both to tremolite and to the zeolites; but which, in my opinion, should form a distinct species in mineralogical arrangements.

I am, sir, yours very respectfully,

June 29, 1818. Lithophilus.

In the fifth volume of the Edinburgh Transactions is a paper by Dr. Kennedy, containing a description, with an analysis, of a mineral found inclosed within a mass of prehnite in the basaltic rock upon which Edinburgh Castle is built. Dr. K. considers it to be a zeolite;—Mr. Allan in his Tables of Analyses denominates it *asbestous tremolite:* while in Professor Jameson's System of Mineralogy it is not mentioned under any appellation whatever. To repeat its characters would be foreign to my present purpose; but the following is Dr. Kennedy's statement of the results of his analysis:

Silica

Silica	51·50
Lime	32·00
Alumina	0·50
Oxide of tin	0·50
Soda	8·50
Carbonic acid, &c.	5·
	98·

with traces of magnesia and muriatic acid.

Now, in the first place, this substance cannot with accuracy be considered as a zeolite, for all species of that mineral contain at least one-tenth of alumina (*vide* Analyses by Klaproth, Smithson, Gehlen, Vauquelin, &c.); whereas the mineral in question does not contain above 1-200dth of that earth.

Secondly. Notwithstanding its similarity in specific gravity and in phosphorescence, it is impossible that it can be correctly classed as a tremolite: for the analytical experiments of Klaproth, Chenevix, Laugier, and other chemists, have demonstrated that magnesia is an essential ingredient of that fossil; while in the mineral analysed by Dr. Kennedy the quantity of magnesia was unappreciable.

Thirdly. No *other* mineral, as far as my knowledge extends, is at all analogous, in point of composition, with that to which these observations relate.

It would seem, from the above considerations, that this curious mineral constitutes a distinct species; and as it has hitherto been found at Edinburgh only, it is suggested that *Edinite* would be a suitable appellation for it.

THE LATE EXPLOSION OF THE COAL-MINE AT NEWTON-GREEN, AYR.

The particulars of this accident were noticed in our last number: the following explanation of it has been since given by the Ayr Journal:

"We are requested to state, that the safety-lamp which occasioned the explosion by which Mr. Millar lost his life, as mentioned in our last, upon being examined, was found to have a small defect at the socket. We are further informed, that a candle was used in the lamp, by the melting of which some tallow had fallen on, and adhered to, the wire-gauze. From these facts, and from that formerly stated of the lamp being excessively heated, it is concluded, that the combustion proceeded either from the communication of the gas with the flame through the defect in the socket—from the illumination of the grease on the gauze by the high temperature of the lamp—from the extraordinary heat of the lamp itself—or from a combination of these

circumstances,

circumstances, and not from any deficiency in the original invention of Sir Humphry Davy. We feel satisfaction in making this statement, as it thus appears, that the unfortunate accident which happened, ought not to lessen the confidence of miners in those lamps, when sufficient care is taken that they be not faulty, or imprudently used. We are also assured that, upon one occasion, a lamp used at the Ayr Colliery continued safe with the inflammable air burning in it for the space of three hours, and that at that colliery the greatest confidence has been placed in them by the workmen."

DESTRUCTIVE WATER-SPOUT.

On the 18th June a water-spout of immense diameter inundated great part of the arrondissement of Auxerre. The rain, accompanied by large hailstones, fell in torrents for thirty minutes. The whole harvest in nineteen communes is destroyed. In some quarters the water was six feet deep; at Fontenai a house was thrown down, and four children killed, and several other edifices were much damaged.

ANIMAL REMAINS.—MAMMOTH—CROCODILE.

There have been recently discovered in the parish of Motteston, on the south side of the Isle of Wight, the bones of that stupendous animal supposed to be the Mammoth, or Mastodon. Several of the vertebræ, or joints of the back-bone, measure 36 inches in circumference: they correspond exactly in form, colour, and texture, with the bones found in plenty on the banks of the Ohio in North America, in a vale called by the Indians Big-bone Swamp.—Also, in the parish of Northwood, on the north side of the island, the bones of the Crocodile have recently been found by the Rev. Mr. Hughes of Newport. They seem to have belonged to an animal of that species, whose body did not exceed twelve feet in length.—Their calcareous nature is not altered; but the bones of the Mastodon (found on the south side of the island) contain iron.

NEW DISCOVERY IN OPTICS.

A very interesting and important discovery has lately been made on the increase and projection of light, by Mr. Lester, engineer. As this discovery will form a new æra in optics, a record of its history must prove interesting to the scientific world, and, as such, we shall briefly lay before our readers the following account of it by a correspondent.

Mr. Lester being engaged at the West India Docks for the purpose of applying his new mechanical power, *The Convertor*, to to cranes, by which the labour of winches is performed by rowing, &c.; on taking a view of the immense spirit vaults, he was forcibly struck by the inefficient mode adopted to light those very extensive and wonderful depôts*, which is by a cast-iron cylinder of about two feet in diameter, and two feet deep, placed in lieu of a key-stone in the centre of each arch;—these cylinders are closed at their tops, and each furnished with five plano-convex lenses (bull's eyes) of Messrs. Pellatt and Green's patent, which are admirably adapted to the conveying of light in all situations, except down a deep tube or cylinder, where the refraction they produce (in consequence of their convex form) betwixt the angles of incidence and reflection, prevents the rays from being projected into the place intended to be lighted. This refraction throws the light upon the concave sides of the cylinder, where it is principally absorbed, instead of keeping the angles of incidence and reflection equal.

From these observations, Mr. Lester concluded, that a lens might be so constructed as to prevent this refraction, and commenced a course of experiments for that purpose. He succeeded by obtaining the proper angle of the incidental rays with a mirror, and finding the scope of the cylinder sufficiently copious to admit the reflected rays into the vault, provided the refraction of the lens did not intervene. The same angle produced by the mirror he endeavoured to retain upon the sides of the lens, by giving it a different form, a peculiar part of which he intended to foliate. But having met with insurmountable difficulties in this process, he concluded, from the striking appearance of silvery light upon the interior surface of that part he intended to silver, that metal would represent the light by retaining that form, and, brought down below the edges of the lens, might produce the desired effect. In his attempt to accomplish this purpose, by holding the body in a vertical position between the eye and a candle, a flash of light was instantly produced, by representing the flame of the candle magnified to the size of the whole of the inner surface of this piece of metal, and gave an increased light upon the wall opposite to him. After this discovery, he had several pieces of metal formed, retaining the same angle, but of various diameters, and found to his great surprise, that, although their area were greatly increased, the representation of the flame still filled them without the least diminution in the quality of the light, but with an increased light against the wall, in proportion to the increased area of the surface of the

* One of which is nearly an acre and an half in area, and is supported by 207 groined arches and 207 stone pillars.

 metal.

metal*. How far this power and effect may extend, is not at present ascertained; but it is believed that a zone of light of the same quality and effect may be produced to an inconceivable extent. Some idea may be formed of the powerful and important results that may be derived from this discovery, by reasoning philosophically on its principles:—Let a candle or any other light be represented in a mirror at a given distance from the flame; and the eye of the spectator be placed so as to view its reflection nearly in the cathetus of incidence. Let him mark the quantity of light represented in the mirror, and such will be its true quality when forming a zone of represented flame of double the diameter of the distance betwixt the real flame and the mirror.

If a candle be placed before a mirror, its flame will be represented; and if a thousand mirrors are placed in a given circle round a candle, the candle will be represented a thousand times, and each representation equal in brilliancy, if the mirrors are at equal distances from the flame. Suppose that the thousand mirrors were united in such a form as to bring all the represented flames into one flame, of equal brilliancy with the real flame of the candle. For the same law of nature by which the flame is represented a thousand times in as many mirrors so united, it would be represented in one flame if the mirror be made of a proper form, and placed in a proper position to receive the rays of light that emanate from the candle in the direction of the angle of this peculiar formed mirror.

As the light of a small candle is visible at the distance of four miles in a dark night, what must the diameter or circumference of that zone of flame be that is produced by this discovery from one of the gas lights in the streets of London? Thus two lamps or stations would be sufficient to light the longest street, when its position approaches to a right line, as the diameter of the zone may be made of the same diameter as the street; and as the rays of light that are increased by this invention diverge from the luminous body, all parts of the street would be filled with light. Many are the minor advantages that will be derived from its application to domestic purposes, for writing, reading, and working by candle or lamp light. This, like Dr. Brewster's kaleidoscope, is another instance of the effects to be produced by mirrors.

* This invention is not confined solely to light, but the increase of heat keeps pace with the increase of light, and both in the ratio of the area of the surface.

The apparatus is so constructed as to be placed upon a candle, and sinks down with the flame, without either flooding or waste.

It

It appears that the great impediment to improvement and discovery in this branch of the science of optics has arisen from the difficulty of foiling glass to the various forms necessary, in lieu of which we have been compelled to use metallic substances. These difficulties once removed, a vast field of important discovery will be opened on the nature and effect of light. May not many of the phænomena that are observed in the air, such as *halos* round the sun, be produced by this principle, the rays falling upon a denser medium than air, and thus producing a zone of light? &c.

The further particulars of this important discovery we hope to lay before our readers in a future number.

POLAR EXPEDITION.

For the following interesting observations on the state of the ice in the Arctic Circles, and the probability of our Northern expedition accomplishing its objects, we are indebted to the intelligent captain of one of our Greenland ships recently arrived here:

"The probability of the ships which have sailed to the North attaining one or more of the ends sought, is a subject on which much argument has been used, and much difference of opinion expressed. On the morning of the 8th June, I was in latitude 79 deg. 40 min. N. about 40 miles distant from the land, reaching to the south, along the edge of the ice, and at that time saw both ships beating into Magdalen Bay. As early as the 2d May, I was in latitude 80 deg. 10 min. N. and had found the country remarkably open until I approached 78 deg. 40 min. N. Beyond this the ice was more considerable in quantity, and much more compact, so as to increase the difficulty of our progress further, but not sufficient to preclude our reaching what is called the fast ice. Here was presented an obstacle which nothing could overcome but a long succession of northerly winds, which if occurring, might be expected to separate and force it down. In the interval between the 2d May and 4th June, I was chiefly in low latitudes, but previous to the 10th was again as far as 79 deg. 45 min. N. The ice had indeed come down a considerable distance, but remained equally close and solid, so that I am positive no rational hope could at that time be entertained of reaching, or even coming near, the Pole.

"The other objects of the expedition I have no doubt will be fully realized, viz. the ascertaining whether Spitzbergen be an island, or joins some other land; and the investigating the situations of the bays, inlets, &c. The ice this season separated very early from the land, which circumstance was certainly favourable to the minor, if not particularly so to the major, objects of the undertaking. In giving my opinion decidedly against the probability of the great end to which we anxiously look being attained, it is but right to say, that I have witnessed the state of the country for more than sixteen years, and, comparing the present with some of the former, appearances are far less promising than I have before seen. In the year 1816 I reached the latitude of 82 deg. 15 min. N. and could see no obstacles but what might easily have been surmounted, had it been my wish to proceed further. This arises, as I have before observed, from the ice not separating, as in the season I have named, in its descent down to the south, but continuing close and unaltered."—(*Hull Papers.*)

PSEUDO-VOLCANO IN STAFFORDSHIRE.

Some interesting facts relative to what is called the Pseudo-volcano, near the Bradley iron-works in Staffordshire, have been published. The tract of ground is situated by the road side from Birmingham to Wolverhampton, about half way between Wednesbury and Bilston. It is mentioned by Plot, in his Natural History of Staffordshire, as being on fire in 1686, when he wrote: and he says it was not then known how long it had been on fire. It then occupied a space of eleven acres; but its ravages have since extended about one mile and a half in extreme length, and one mile in breadth. Whether the fire originated in accident, or from the sulphur contained in the coal and pyrites, is not known; but it probably arose from the latter cause; as, at other pits, the small coal has taken fire on being exposed to the air. As the combustible matter is exhausted, the hand of cultivation resumes its labour; and even in parts where the fire still exists, by carefully stopping the fissures, and preventing the access of air, different crops can be raised. A neglect of these precautions sometimes destroys half the produce, whilst the remainder continues flourishing. About two years ago it began to penetrate through the floors of some houses; it produced great alarm by appearing in the night; and four of the houses were taken down. It exhibits a red heat in this situation, and the smoke has forced its way through a bed of cinders forty feet in height. On the south it is arrested by beds of sand, which cover the coal formation in that part; and on the north-east it is impeded by cultivation. At first view a stranger might suppose himself in a volcanic region. The exterior view of the strata exposed by the falling in of the ground, presents a surface blackened by the action of fire, and presenting most of the porphyritic and trap colours in high perfection. The cinder dust on which you tread, the sulphureous vapours and smoke which arise from the various parts of the surface, and the feeling of insecurity which attends most of your footsteps, all combine to give a high

high degree of interest to the scene. In some places of this region coal is found only four feet from the surface.

TEST OF POISONING BY ARSENIC.

On the 22d of May, a Coroner's inquest was held before Thomas Stirling, Esq. at the King's Arms in Little Woodstock-street, on the body of Eliz. Danvers, who, from disappointment in love and a consequent state of despondency, had put an end to her existence by means of arsenic. Neither the family, nor the medical gentleman who was called in, at first suspected the cause of her death: but a coffee-cup, out of which the deceased, it was supposed, had drunk something improper, being sent to Mr. Hume, chemist, in Long Acre, sufficient evidence was obtained to induce a further inquiry. At this gentleman's request the body was opened, and the contents of the stomach, which amounted to about twelve ounces, were separated, and nearly a wine-glass full of these delivered to him for analysis. From these two sources the most satisfactory and convincing testimony was derived, that arsenic was the sole cause of her death; for even the coffee-cup, although in appearance quite empty and clean, yielded a sufficiency to substantiate above a thousand separate experiments. The contents of the stomach afforded a still more ample supply, for they impregnated at least a pint and a half of distilled water so completely, that every single drop indicated the presence of arsenic when exposed to the proper tests. It was remarked by Mr. Hume, and should serve as a warning to the experimentalist, that the *fluid* portion of what was abstracted from the stomach showed no indications of the poison; it was only from the sediment, and more gross and viscid matter, that he could form his solution. The medical practitioner, who had been called in to the deceased, when labouring under the agonizing symptoms, at first declared the case to be that of *cholera morbus*. It is but fair, however, to state, that this professional gentleman saw the young woman but a few minutes before she expired; for on witnessing some of the experiments, and attending to Mr. Hume's evidence, he most readily acquiesced in the verdict, that Miss Danvers's death was solely occasioned by a dose of arsenic. It cannot be too strenuously impressed upon the public mind, that no substance is more readily detected than arsenic, provided the proper means be employed: it is therefore highly incumbent on medical men in general to inform themselves of the nature and practical application of the tests, and most effectual modes of operating, which, we believe, are very simple, and require but a moderate share of chemical knowledge. There are many observations and comprehensive instructions upon this subject, especially

cially by Mr. Hume, which will be found in various elementary and periodical works of science, particularly in the Philosophical Magazine, and the London Medical and Physical Journal. To these we earnestly recommend the attention of our readers, particularly those of the Faculty of Medicine in all its branches, on whom the verdict of an English Jury, and consequent life or death of a fellow-creature, often solely depends for its direction.

IMPROVEMENT IN THE MANUFACTURE OF PAPER.

[*From the New York Evening Post.*]

We have lately visited the paper mills of Thomas Gilpin and Co. on the Brandywine, and witnessed the performance of their new machine for manufacturing paper on an extensive scale, which promises an important addition to the arts and manufactures of our country. This process of making paper delivers a sheet of greater breadth than any made in America, and of *any length*—in one continued unbroken succession, of fine or coarse materials, regulated at pleasure to a greater or lesser thickness.—The paper, when made, is collected from the machine on reels, in succession as they are filled; and these are removed to the further progress of the manufacture. The paper in its texture is perfectly smooth and even, and is not excelled by any made by hand in the usual manner of workmanship—as it possesses all the beauty, regularity and strength of what is called well closed and well shut sheets. The mills and engines now prepared, are calculated to do the daily work of *ten paper vats*, and will employ a water power equal to about 12 or 15 pair of millstones of the usual size.

The apparatus and the machine are on a principle and construction entirely new, and are patented by the inventors here. It has been very expensive, and has been brought to its present state of perfection with much labour and perseverance.

NEWLY DISCOVERED MEMBRANE IN THE EYE.

Doctor Jacob, demonstrator of anatomy in the university of Dublin, has discovered, and demonstrated in his lectures on the diseases of the eye, a membrane covering the external surface of the retina in man and other animals. Its extreme delicacy accounts for its not having been hitherto noticed. He arrived at the discovery by means of a new method of displaying and examining the minute structure of this and other delicate parts. He argues from analogy, the necessity of the existence of such a membrane, as parts so different in structure and functions as the retina and choroid coat, must otherwise be

in

in contact, in contradiction to the provisions of the animal œconomy in general. A detailed account of the discovery, with the method of displaying and examining the membrane, is in preparation.

NEW RIFLE GUN.

[*From the New-York Commercial Advertiser.*]

We have seen a new modelled rifle gun, which promises to be of some consequence, and is said to be the invention of Capt. Artimas Wheeler, of this town. We have not had sufficient opportunity to examine it so minutely as to give a description of it that would do it justice; we are inclined to think it a new and important invention, more particularly in case of an action with an enemy. It has one barrel through which the charges pass, that is of common length; also seven short ones not much longer than sufficient to contain a charge each;—these have a pan attached to them, to contain powder for priming, and are kept perfectly tight by a slide that covers them. These barrels are made to move circularly round near the lock, which is also of new construction. After firing the first charge, the half cocking moves by a spring one of these short barrels round, and confines it tight in the breech end of the long barrel, through which the charge must pass; the shutting the pan of the lock opens the slide which covers the priming. This gun is but little heavier than the common one; and when once loaded, which requires little more time than to load a common rifle, it can be fired as expeditiously as will be convenient to cock the piece and take sight, until the seven are discharged.

ATMOSPHERIC PHÆNOMENA.

Dr. Thomas Forster has of late noticed a phænomenon which ought further to engage the attention of philosophers; namely, that the moon appears on rising, particularly about the time of the full, to have the power of dispersing the clouds, and clearing the atmosphere. He was first admonished of this circumstance by some French sailors while crossing the channel from Calais; and it had likewise been cursorily noticed to him by Mr. Herschel of St. John's Cambridge. For some time past, whenever circumstances afforded an opportunity of observing clouds about the time of the moon's rising, they have shortly been much diminished in volume, or wholly evaporated. This fact is best observed in the neighbourhood of the sea, and seems to be less remarkable in very inclined situations. The circumstance is slightly hinted at by Aristotle, and the early writers on meteorology. It shows the power of light on the phænomena of the atmosphere.

LIST OF PATENTS FOR NEW INVENTIONS.

To John Neilson, of Linlithgow, glue manufacturer, for improvements in the tanning and tawing of hides and skins, and in the dyeing or colouring of leather and other articles.—22d June 1818.—6 months allowed to enrol specification.

To Robert Roux, of Yverdon, in the canton of Vaud, in Switzerland, Doctor in Divinity, in consequence of a communication made to him by a certain foreigner residing abroad, for improvement or improvements applicable to locks of different descriptions.—30th June.—2 months.

To John Baird, manager for the new Shotts Iron Company, residing in the parish of Shotts, county of Lanark, Scotland, for various improvements in the manufacturing and making of cast-iron boilers used for the purpose of evaporating the juice of the sugar-cane, or syrup derived from thence, by means of annealing them in a furnace or kiln of a peculiar construction.—11th July.—4 months.

To William Bailey, of High Holborn in the county of Middlesex, ironmonger, for improvements in sashes, sky-lights, and frames, generally used for the purpose ot receiving, holding, and containing glass for the admission of light, and the exclusion of rain and snow, and also for making roofs or coverings for houses and various other buildings.—11th July.—6 months.

To James Milton, late of Paisley, but now of Ashton-under-Line in the county of Lancaster, for a new species of loom-work, whereby figures or flowers can be produced, in a mode hitherto unknown, upon any fabric of cloth while in the process of weaving, whether such fabric be linen, cotton, woollen, silk, or any of them intermixed.—11th July.—2 months.

To John Richter, of Holloway, in the county of Middlesex, in consequence of a communication made to him by a certain foreigner residing abroad, for certain improvements in the apparatus or utensils used for distillation, evaporation and condensation, and which are new in this country.—14th July.—6 mon.

To Richard Ormrod, of Manchester, in the county palatine of Lancaster, ironfounder, for improvements in the manufacturing of copper or other metal cylinders or rollers for calico printing.—22d July.—4 months.

To Urbanus Sartoris, of Winchester-street in the city of London, merchant, for improvements in the method of producing ignition in fire-arms by the condensation of atmospheric air.—22d July.—6 months.

To Henry Creighton, of Glasgow, civil engineer, for a new method of regulating the admission of steam into pipes or other vessels used for the heating of buildings or other places.—22d July.—2 months.

Meteoro-

Meteorological Journal kept at Walthamstow, Essex, from June 15 *to July* 15, 1818.

[Usually between the Hours of Seven and Nine A.M. and the Thermometer (a second time) between Twelve and Two P.M.]

Date.	Therm.	Barom.	Wind / Remarks
June			
15	16 75	30·00	SE—SW—W.—Clear morn, but some *stratus* NW. and *cirrostratus*; fine day; red orange sunset; moon in a small *corona*, and clear, and *cirrus* in rays upwards.
16	66 74	30·00	SW.—Sun, and very hot; windy and hot sun; *cirrostratus*, and clear at night.
17	65 77	29·81	S—SE.—Clouds and windy; 8 A.M. a small shower; clouds and sun; small rain after 3 P.M.; still rain, but slight.
18	57 75	29·75	N.—Fine morn; sun and clouds; calm; some slight rain after 4 P.M.; windy; clear, and *cirrostratus*. Full moon.
19	53 70	29·75	S.—Fine morn; wind, clouds, and sun cooler; frequent small rain after 3 P.M.; *cirrostratus*, and windy evening.
20	55 67	29·70	W—NW.—Sun and *cumuli;* slight showers, and sun till about 6 P.M.; fine evening; clear, *cirrostratus*, and calm.
21	56 70	30·00	SW.—Sun and wind; *cumuli, cirrus*, and *cirrostratus;* fine day; some small drops of rain; cloudy and windy.
22	57 63	30·00	SW—W.—Showers; cloudy; fine day; a great shower about 4 P.M.; star-light at 11 P.M.
23	56 67	29·80	W.—*Cumuli*, sun and wind; clouds and sun; *cirrus*; cloudy night.
24	57 72	30·00	W—NW.—Fine morn; *cirrus, cumuli*, and *cirrostratus;* fine day; windy; clear; clouds and wind.
25	56 71	30·10	W.—Gray morn; fine sunny windy day; clear and *cirrostratus*. Moon last quarter.
26	62 73	30·00	W—SW—S.—Gray morn; clouds, wind, and sun; very clear night.
27	62 84	30·00	SE—E.—Hot sun, and *cirrocumuli*; clouds; sun very hot; black *cirrus* at 8½ P.M. hanging over *cirrostratus* like fringe; clear *cirrus* and *cirrostratus*.
28	63 70	29·70	SW—NW.—Showers, sun and wind; great shower between 12 and 1 P.M.; showers and sun; distant thunder; fine evening; *streaked cirrostratus* NW.
29	59 76	30·10	SW—W.—Sun, *cumuli,* and wind; fine day; clear night.

Date.	Therm.	Barom.	Wind.
30	60 80	30·20	N—SW—NW.—Hot sun; fine hot day; calm; clear night; a little wind arisen.
July			
1	62 79	30·20	NW—W.—Fine hot sun; calm; fine day; at 8 A.M. small thunder cloud; some drops of rain; black *nimbus* at 9½ P.M.; and afterwards *cirrostratus*.
2	61 72	30·10	Sun and wind; very fine day; cooler; clear and *cirrostratus*, and wind.
3	59	30·20	E by S—NW.—Sunshine, and clear at 7 A.M. at 9 A.M. hazy; fine day; clear and *cirrostratus*. New moon.
4	61 72	30·05	NW.—*Cirrostratus* and wind; fine day; clear star-light and wind.
5	62 76	30·10	NW.—Clear sunshine; fine very hot day; windy; clear night.
6	67 79	30·10	N—E—SW.—Fine and hot; fine hot day; windy.
7	63 80	30·10	SE—E.—Hot sun; calm; fine day; very hot; clear and *cirrostratus*.
8	61 69	29·90	N—NW.—Sunshine; fine day; stars, moon, and *cirrostratus*.
9	60 76	30·10	W.—Clear and *cirrostratus*; fine day; cloudy at night.
10	66 71	30·10	S—SW—S.—A slight shower before 7 A.M.; sun and wind; fine day, and hot; clear moon-light.
11	66 74	30·10	NE—N—W—SW—NW.—Sun and clouds; fine day; rain began soon after 9 P.M.—Moon first quarter.
12		29·85	SW—NW.—Rainy; rain till about 11 A.M.; afterwards fine day; very hot; *cirrostratus* and windy.
13		29·80	NW.—Clouds and wind, and some drops of rain; fine day; great wind and *cumuli*; moon-light.
14	66 77	30·20	W—N—E.—Fine morn; calm; fine hot day; Thermometer 80 at 3½ P.M.; black *nimbus*, and clear at 9 P.M.; at 10 P.M. fine mottled *cirrocumuli*, and stars.
15	63 80	30·30	N—SE.—Sun and wind; very hot; clear and *cumuli*; fine day; *very* hot and windy; moon- and star-light (clear).

During the very *dry* weather the birds came to be fed near the windows, *almost* as near as in frosty weather in winter.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

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

1818	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
June 16	13	75·	30·01	Fine
17	14	63·	29·79	Rain
18	full	66·	29·88	Fine
19	16	70·	29·86	Cloudy
20	17	56·	29·69	Rain
21	18	63·	30·01	Cloudy—rain P.M.
22	19	66·	29·77	Ditto—rain A.M.
23	20	64·5	29·94	Ditto
24	21	65·	30·08	Ditto
25	22	73·	30·02	Fine—shower A.M.
26	23	73·	30·17	Cloudy
27	24	78·	29·82	Fine
28	25	70·	29·89	Ditto
29	26	70·	30·29	Cloudy
30	27	79·5	30·32	Fine
July 1	28	65·	30·15	Rain
2	29	65·	30·24	Fine
3	new	68·	30·27	Cloudy
4	1	68·5	30·12	Ditto
5	2	71·	30·16	Ditto
6	3	73·5	30·20	Ditto
7	4	76·	30·03	Very fine
8	5	65·	29·94	Cloudy
9	6	70·	30·22	Very fine
10	7	74·	30·11	Ditto—rain P.M.
11	8	74·	30·01	Ditto
12	9	71·5	29·89	Cloudy—rain at night
13	10	69·	30·06	Ditto
14	11	75·	30·33	Very fine

On the few days marked RAIN there fell only showers, the effects of which were gone in three hours, except on Sunday night, when there was a heavy rain of three or four hours continuance.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For July 1818.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
June 27	68	84	68	29·75	78	Fair
28	66	74	62	·92	56	Showery
29	66	76	68	30.18	86	Fair
30	65	80	66	·19	105	Fair
July 1	66	80	65	·10	84	Fair
2	60	71	57	·03	72	Fair
3	60	75	60	·10	76	Fair
4	61	76	60	·04	70	Fair
5	66	77	66	·01	66	Fair
6	66	78	64	·03	70	Fair
7	68	75	64	29·90	80	Fair
8	64	76	59	·94	69	Fair
9	60	75	66	30·00	78	Fair
10	67	72	60	·01	66	Cloudy
11	67	76	66	29·85	78	Fair, rain at night.
12	60	74	64	·75	60	Fair
13	66	76	64	·96	56	Fair
14	67	78	71	30.17	66	Fair
15	67	79	66	·27	86	Fair
16	68	85	70	·16	102	Fair
17	68	76	66	·10	80	Fair
18	67	76	64	·04	76	Fair
19	69	80	64	29·90	85	Fair
20	66	76	64	·91	81	Fair
21	66	75	66	·91	66	Cloudy
22	67	76	68	30·00	74	Fair
23	68	82	76	·01	91	Fair
24	76	87	72	29·79	95	Fair*
25	70	80	67	·80	86	Fair
26	67	80	68	·80	79	Fair

* Very vivid lightning in the evening, with distant thunder; a small fall of rain during the night.

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

XII. *On the Fructification of Seeds.* By Mrs. Agnes Ibbetson.

To Mr. Tilloch.

Sir, — I showed in my last letter the formation of the hearts of seed in the roots of a plant. I showed also its progress up the alburnum vessels, and its entrance into the bag of the seed, the *contour* of the bag being really formed of that *line of life* which ties the hearts of the seeds together as they mount from the root. See fig. 1.

I have before given many specimens of the manner in which the seeds are half filled by the juices of the atmosphere; then completed by a powder flowing upwards from the root;—*there I stopped.* I shall now give the next *process*, which always follows *directly*; viz. "the fructification of the seeds;" that is, the conveying the powder of the pollen when dissolved in the juices of the pistil down to the seeds, allowing each vessel to enter each different seed, and bestow its quantum of matter on all by turns. This takes place as soon as the seed has received all that which may be called its nutriment; and is so different from the following process, that there is no fear of confounding them together. This last operation takes place as soon as the flower is full blown, and therefore arrived at its greatest perfection. The nectareous juice is then seen by the naked eye to mount the pistil, and settle in a large drop on the stigma: this it doth each sunny day; and even if the flower is turned downwards, (like a *campanula*,) still the drop appears to hang and *never* to *fall*, but returns within the style down the pistil, into the secret nectary, where it remains all night, and reappears next morning in the same situation on the stigma till the pollen is *ripe*; when, the dust falling on the stigma, the various apertures thus impressed receive and secure it, and it remains there visibly till it is completely dissolved.

That any one should deny the sexual system, who has *regularly dissected flowers* and plants (especially if done progressively), I can never believe. That the pistil is formed to receive and reduce the *flower of the pollen*, which clings to it, and that it then carries the mixt juice down to the little branch in the heart of the seed, vivifying and exciting its growth,—is certainly true: and let the different figures of the flower be ever so various in this respect, it will be exactly the same in its result. This constant progress take place, and the whole progressive movements succeed each other in constant routine. Nor can I conceive how botanists could reconcile the doubt they made with respect to the trough in the style and stigma, and therefore to its conveying down the joint juices of the pollen and pistil matters, when they saw and acknowledged the rising of the juice of the pistil to the stigma in that beautiful drop before impregnation. Would not the same trough serve the purpose for the falling liquid, that it did for the rising one? And yet all botanists acknowledge the first, but many deny the last, and believe that there is room for the pistil juice, and not for the same when the pollen powder is dissolved in it, though the style is then infinitely increased and inflated.

The flower, let it be ever so various in appearance, is invariable in having the summit and style (if it has any) above the seed-vessel (see fig. 2); and in these parts being in one connected pillar with the secret nectary; while the open nectary is *always* at the exterior of the pillar, but standing perfectly aloof. If the stamens appear united with the pistil, it is only so to appearance; they are never fastened but to a skin which is connected with the cylinder below, and carries on the vessels of the stamen to the wood part, and only lies against the pistil. Such is the stamen of theq *Auilegia*, (see fig. 3,) and the stamens of the *Malva* and *Viola*, &c. which I have not room to give. The pistil is then unconnected with every other part, but those just mentioned. The stamens when ripe throw their pollen by many different methods on the stigma. The various forms of the pistil proclaim that it was made to *receive*, *secure* and *dissolve* the powder so bestowed. The stigma is either covered with innumerable *short thick hairs*, which to the eye give it the appearance of velvet, but when greatly magnified, show that each vessel has several apertures to take in the powder, while the points secure the balls of the stamen, till they burst with the moisture of the pistil. The dust is then received into these innumerable apertures, which empty themselves into the interior in a gutter, which runs all down the style to the seeds.

But if the stigma, instead of resembling the pistil of the *Columbine*, is like the flower of some of the *Tetradynamia siliculosa* order, then the stigma swells *above*, so as to overtop the style, as in the *Iberis Erysimum*, (fig. 4.) &c. while the numerous corresponding apertures take in the powder at *d d d*, and send it when dissolved *down to the seeds*, (see fig. 4) In the *Chelidonium*, a gutter is carried round below the surface, which receives all the juice the hairs bring it, as at fig. 5. (*a a*) in regular rows; and the whole centre is one deep trench in the middle, by which, there being no style, the juice is at once carried in three regular cuts down to the seeds. In the *Cheiranthus*, the seed-vessel being a sort of flattened triangle (fig. 2); the summit at 2 XX has a bending in the middle; the large orifice is therefore in the centre only: while at fig. 2. *a a* is the summit;

b b

b b the seed-vessel; *c c* the hidden nectary; *d d* the rising up of the hearts of the seeds before they enter the seed-vessel, and place themselves at *e e* in the seed.

The next sort of pistil is that which opens the stigma into various divisions. This most simply declares its office, since the pistil never divides till the pollen balls are ready to explode. This is seen in the *Iris*, where the powder or dust is discovered all the way down the passage (fig. 5. *e e e*,) beginning to pass into it at *d d*. No pistil so puzzled the botanist at first as the *Iris*. It was long disputed which was the female: but nothing is so easy as fixing on that part, as it is always the middle pillar, (see fig. 5.) *b b* the seed-vessel; *c c* the secret nectary; *σ σ* the stigma; and *g g* the style which conveys the joint juices in three rows to the seeds. In many of the stigmas formed in this manner, there appears no opening till just before the whole is concluded and that the stigma is covered with balls, as at B C: then, if carefully watched, the stamens will be seen to burst one after the other, so as to surround the stigma with a sort of cloud; and this is almost always at noon. This accounted to me for a beautiful sight I never saw but twice, though I have often watched for it—the flowing of the pollen in a field of rye-grass. A cloud came on, which at first I could not understand, and fell on the female flower and leaves around. I had just come for the purpose of examining whether the seeds were fructified: I found they were not; but two days after when taken up, the line of life appeared along the heart and seed, and the seed-leaf had begun to grow. In many stigmas that divide, the stamens fall so completely into the aperture which the separation has made, as to be themselves dissolved and sent down the style to the seeds. Authors have said that it is strange that the dust of the pollen is never discovered in the passage, and that it never tints with its colour the interior of the style. But this is certainly a great mistake, for I have repeatedly seen it do so. I have just mentioned a proof of this; and the *Rhododendron* is another instance where the passage is so open that the balls enter and are soon absorbed, and the juice carried down to the seeds. But in general the cases of the pollen are taken for the powder *itself*; the inward dust is rarely of any other colour than a very light *green* or very pale *yellow*, though the cases are often highly painted: and this faint tint is so like the appearance of the juices, that it would produce no change on them if mixed. But when this proves different, the interior of the style gets often coloured, as is seen in the tulip; in which though the pollen is a very pale green, yet the stamens are almost black, and often mix their colour with it, and thus paint the interior of the style. This also happens to the yellow *Iris*, where the coloured stamens impress their tint

 within

within the passage of the three leaves, which I have repeatedly discovered deeply tinted, though affected principally by means of the outward skin of the stamen, which in the rubbing has got mixed with the pollen powder.

The first thing to be assured of, is the mark which proves that the seed is really perfected. This sign is most plain, and is the same in every seed, never varying. It is a line which passes through the heart and seed, and then out again, touching that little branch which the corculum brought from the root with a sort of loop; the vessel then runs to the next seed, and so on, (see fig. 6.) *g g* are the seeds before impregnation, *h h* after. It will be seen that at *i i i* the heart has not yet risen to the top of the seed, where it must get before the line can pass. There are often many stigmas and styles to one seed-vessel: the number of divisions is proportionate;—but this I shall leave till I come to show the pericarp, as the cause of this arrangement is exquisitely beautiful; formed and adapted by nature for the purpose of diminishing the space required, and secreting and confining in as little room as many seeds as possible. The art with which this is done (though the seeds change their form three or four times) *is most wonderful*, and the deal of space left to give them free liberty can never be enough admired; some requiring to lay a part of their time in liquid, when the seeds are left quite in a trough, which they only fill up with solid matter when their outward cases grow. The manner in which the middle pillar of the flower is left,—at such perfect liberty that it may have room for every motion, and for every increase, and also to admit the insect to seek the open nectary,—also merits particular observation. But the astonishing care that has been taken to prevent the possibility of its approaching the secret nectary cannot be sufficiently admired; as that would at once be robbing the flower of its future perfect seed, by preventing its impregnation. I have repeatedly placed the pollen on different stigmas, to show exactly the time the seeds will be in receiving this appearance, shown at fig. 6, *h h*. Few seeds require more than three days; the stone plants will, indeed, sometimes require a fortnight or three weeks, but rarely more: it is a great mistake to suppose that so much time is necessary. When the whole is performed, a total suspension of action takes place till the seed gets buried in the ground. The earth then forms a new stimulus. But seeds will rarely pass beyond a certain point till they fall; and the interior of the seed always begins to grow two or three days at furthest after the line has passed up the seed, because the oxygen they directly yield is absolutely necessary to the growth of the embryo, which is now increasing most wonderfully.

The idea of the seed-leaf yielding food instead of gas was the most

most strange and contradictory imaginable; since the nourishing vessels are visibly made for the latter purpose; but leaves from the first moment yield their oxygen, as may be known by their dissection. As to the manner of the impregnation of the seeds, I have tried them in almost every class and order, but never discovered any difference in this respect. But I am now trying in different sets of plants whether they vary in any way; such as fresh-water plants, sea-weeds, sand-weeds, &c.

In my next letter I shall give the progressive passage *after* the impregnation of a plant to its growth in the earth, with all the intervening steps, never before shown, and most truly curious.

It is always better, for dissection, to trust to the indigenous or flowers of the country; it is certain that, the spiral not being so perfect in *exotics*, its functions are not performed so well. I have now before me above forty different sorts of pistils, but they all proclaim the same law and mode of proceeding. The stigma of the *Pentandria digynia* plant is a very curious one, formed of bubbles, which visibly show the absorption of the powder and its reduction into the juices which tint the interior, through the trough of the style (see fig. 9); as is also the pistil of the *Glecthoma* (fig. 9 X), which has long hairs, to which the balls adhere till they explode, yielding their extremely fine dust by a sudden apparent electrical effect, when the absorption takes place, and it is quick and immediate.

It is quite a mistake of Gærtner to say, that when barren and fertile seeds are mixed together in the same seed-vessel, the whole becomes nugatory. There is no seed-vessel that has not both sorts in the same pericarpium; but they are imperfect from different causes, easily perceived: from the heart not reaching the seed,—from the seed not being impregnated; that is, the line of life not passing through the corculum,—from the nourishment dying away before it can reach the seed. Every seed-vessel shows at first many seeds, even those that never perfect but one or two. This is admirably seen in the chesnut, which almost always has six when the pericarp is under the bud; they go off by degrees, and at last one or two only are completed. As to the proof that the mixture of the pollen is the cause of the impregnation of the seeds, let the evidence be only fairly examined, and no one can disbelieve it. A bed of female plants of the dioicous tribe has been set, and kept from the approach of staminiferous flowers, and *perfect seeds* have notwithstanding been got from it. This has been brought forward as a proof of the *falsehood* of the *sexual system*,—without the female plants being examined, to see whether there were not males concealed

 among

among them, which is almost always the case. This at once renders that trial nugatory. Then it must be recollected that most flowers can be impregnated from pollen of the same species. This is another cause of uncertainty. The only true method, therefore, is trying those plants that never produce, or will bear this mode of management; among whose pistils male plants are never found, and that cannot be impregnated from other pollen than their own. There are a few of *these:* The Palms, the Pistachio nut, the Fig, and two or three more. These have been for years without producing fruit, on the failure of the male plant. I had a complete proof of this, which I shall give, as it is in vain to repeat Linnæus's excellent trials, which are in every botanical book. When I was at Lisbon, in passing to the Caulders, about half way I came to a village, where there were at that time many female palm-trees and no male, but in the middle of each tree was placed a branch of the male. I inquired the reason. The man told me he had planted many, and chose them wrong from ignorance, and for years they had given no fruit, as there were then no palms on this side of Lisbon; but his children going to Lisbon brought a branch of the male palm, and stuck it in the tree, and that tree gave fruit. The next year he had therefore performed the same to each, and had always a quantity of dates.

At Belle Vue I planted a female Juniper; and it never had fruit, though there were many male plants within two miles: but it was placed on a remarkably high hill, where the winds blew with violence. After many years experiment I wished to try a male plant, and placed it near; and I had fruit very soon after, within two years I think. As to the Fig, it is the only flower that appears to me *truly to be made for the insect to do the office,* since both parts are confined in a receptacle which is laid open at the proper time. When the insect *Cynips* enters the male Fig, it rolls itself in the pollen, then flies to the female, and deposits the powder all round the aperture which nature makes at this time in both fruits. Here at the entrance it inserts its own eggs, and leaves *the pollen* all round the orifice, which is soon conveyed to the stigma by means of the juice of the pistils, which almost overflow the receptacle with their liquid.

By what means will the pollen of the *Primula veris,* thrown on the *Primula vulgaris,* make a *Cowslip* of it the next year, though that flower is so rare in this county of Devon,—and that degenerate and return to its original species, if the *trial* is not *repeated?* Why will the pollen of the Sweet purple Pea, thrown on a number of white ones, give seed the next year that will bestow a pretty equal number of both colours, and some variegated?

gated? Why is the syngenesian flower capable of receiving the juice of the pistil, which *runs up* and often even covers the seeds below, without any effect?—but when it descends with the flower of the stamen, and runs into the seeds with the joint juices, why are the *seeds directly* impregnated? If the pollen had no effect, how could all this happen? But of all the seeds, none will perhaps show the whole process more completely than the *Fragaria* (fig. 10), especially at the seed fig. 11 X: *a a* is the stigma and style; *b b* the heart of the seed, which rises from the root of the plant; *c c* the ascent and descent of the juices from the corculum; *d d d* is the juice first ascending to the stigma; and *e e* is the liquid running from the style to the heart of the seed, and carrying the mixed juice: therefore in this seed there is a separate vessel; and one peculiarly adapted to convey the powder and juice through the heart, showing that the pistil juices could not do it. Fig. 10 is the seed-vessel, and *d d d* the part which conveys the corculums up to each seed. I have drawn it completely as it appears, and as a half-guinea glass can show it. As to the idea that the embryo is formed in the male or stamina, it is most ridiculous; for it is far smaller than the embryo of the seed. Besides, I have shown by a progressive picture, that the beginning of the seed proceeds directly from the root, and that it must be the right part—*the real embryo*—since it is never lost *sight of* till *fructified;* this part may therefore be as well called an *egg* till it has joined its seed.

When I said that I knew but of one flower that appeared really to want the assistance of insects, I only meant that nature had made them perfect, but the change of climate had altered them:—many exotics that would go on increasing their filaments, are stopped by the alteration of climate, and the weakness of the muscle. It is sometimes the case with our own flowers in bad seasons; and in watching, how continually have I seen innumerable flowers bend down their pistils with a jerk to procure the powder of the stamen! In many of the Lily tribe, I have caught them when bending with a sudden jerk, which had much the appearance of an electrical effect or shock,—such as the pistil of the Orange Lily, or the *Hemerocallis fulva*, the large white Lily. I have seen the single *Camilla Japonica* (the pistil of which is sometimes prolonged much beyond the stamen) bend down and sweep the pollen of each stigma in its turn, till it loaded itself with powder. I have also seen in like manner the *Amaryllis Jacobæa* bend down and make each stigma take its share of the powder: but this moves gently along after the reception of the powder, and not with a jerk. How curiously in the Winter Rose will the stamens (though they all lie around) raise themselves, and turn so many at a time in a contrary direction,

 while

while in the *Geranium*, the pistil is made to bend by the filaments twisting round her! How beautifully does the *Spartium junceum* throw off its keel, that the pistil may have room to *sweep round*, and turn its points to the spreading stamens, which pass between each! I have watched this, flower after flower, with admiration: for the manner in which it first throws back the banner is most curious; the wings then fly apart, and the inward motion of the keel becomes perceptible; when the sun striking full on it, it flies open, and the stamens disperse some of their powder. *Now at full liberty*, the boxes of the stamen open, and the pistil turns round, and one after the other presses between the two points of the stigma, and thus loads it with powder.

I must now stop: My letter is already too long. Fig. 7 is the interior of the flower of the *Aquilegia vulgaris*, to show how the hearts of the seeds pass up the stem to the seed-vessel and seeds at *e e e*, and how the juice passes up from the secret nectary *a a* to the stigma *b b b*, and the pollen up the vessels *g g* to the stamen *f f*. Fig. 8 is a single pericarpium and stigma at top. Fig. 5 is the interior pillar of the Iris, &c. The rest I believe I have already explained.

I am, sir, your obliged servant,

Agnes Ibbetson.

Description of Plate II. *No.* 8.

Fig. 9 is the stigma of the Ground Ivy; the pollen powder within the balls enters the diminutive holes, when dissolved. At *a a* in

Fig. 4 is one of those stigmas which are so much larger than the style, and have a gutter running round at *a a a*, and descending also at *b b* to the style.

Fig. 4 X is the stigma of the *Pentandria digynia* plant; and *c c c* the hairs at which the mixed juices enter the stigma: it has a gutter all round, and one through the middle.

Fig. 3 is the manner in which the stamens loosely surround the pistil when it is supposed to be fastened to it. B and C two more stigmas.

The seeds, fig. 7 X and fig. 2 *c c* are the shape of the seeds at that time, both impregnated.

XIII. *On the Comparative Powers of Algebra and Vulgar Arithmetic.* *By* William Gutteridge, *Esq.*

To Mr. Tilloch.

Sir,—As you will no doubt acknowledge that in a commercial country like ours, the true principles of calculation should be thoroughly

thoroughly understood and profoundly taught, I feel persuaded that you will permit me, through the medium of your Magazine, to express my disapprobation of the present method, which prevails in the public seminaries throughout the united kingdom, of solving such questions as relate to mixing of merchandises, as rums, wines, &c. commonly termed Alligation, which, save the medial case, is in all respects *very abstruse, frequently false, and generally detrimental to the merchant.* Nay more, indeterminate equations (of which questions in Alligation are a species) are in works of the greatest celebrity very frequently most erroneously solved, where the great power of algebra in the present mode of application is insufficient to demonstrate the truth; whilst, if rightly handled, vulgar arithmetic is abundantly sufficient to obtain all the integral answers to such questions.

That this is not on my part a mere assertion, but a well-grounded fact, I shall adduce, for the satisfaction of the public, a most glaring instance, which may be found in that well-known work, *Dodson's Mathematical Repository,* where, vol. ii. question XII. page 39, we read as follows:

"Let $2x+3y+5z+30u=100003$," the number of integral answers (see fol. 44) are stated to be "160,190,378,249."

Now, sir, you will give me leave to state, that the actual number of integral equivalent answers to that equation are just 185,090,752,407; therefore the error in Dodson's computation is 24,900,374,158 answers too few. And you will also give me leave to state, that such erroneous computation arises not from accident, but from the adopted method of solution.

Authors have uniformly avowed that vulgar arithmetic would not obtain all the possible answers to questions of this sort, and that algebraic reasoning was indispensable. I am ready to prove the contrary, and to show that arithmetic is not only equal, but infinitely superior, in all cases of alternation.

Errors equally glaring are to be found in the algebra of the celebrated *Bonnycastle,* who, so recently as his *7th edition of that great art,* fails in some of these equations. And, what is more extraordinary, the ingenious *Mr. Davis's Key* to that *celebrated work* corresponds with *the author's errors*;—a proof of the necessity of adopting a different system, whereby the truth may be discovered.

To enter into sufficient detail would engross too many pages of your Magazine; for I am well aware, that in instances where errors have become habitual, nothing short of an elaborate process can possibly convince. I shall therefore content myself, for the present, with a general disapproval of the inadequacy of that system universally prevailing; but should any gentleman think

think proper to dispute the accuracy of my foregoing computation, or the justice of my disapproval, (and that you will be pleased to afford me your pages as the medium,) I shall enter without hesitation into all the minutiæ of the question.

I am, sir,

Your most obedient servant,

St. Finbars, Cork, July 15, 1818. WILLIAM GUTTERIDGE.

XIV. *An Account of Experiments for determining the Length of the Pendulum vibrating Seconds in the Latitude of London. By Capt.* HENRY KATER, *F.R.S.**

To determine the distance between the point of suspension and centre of oscillation of a pendulum vibrating seconds in a given latitude, has long been a desideratum in science. Many experiments have been made for this purpose; but the attention of all who have hitherto engaged in the inquiry (excepting Whitehurst) appears to have been directed to the discovery of the centre of oscillation. The solution of this problem depending, however, on the uniform density and known figure of the body employed, (requisites difficult if not impossible to be ensured in practice,) it is not surprising that the experiments made by different persons should have been productive of various results.

When I had the honour of being appointed one of the committee of the Royal Society for the investigation of this interesting subject, I imagined that the least objectionable mode of proceeding would be to employ a rod drawn as a wire, in which, supposing it to be of equal density and diameter throughout, the centre of oscillation, as it is well known, would be very nearly at the distance of two-thirds of the length of the rod from the point of suspension; and I purposed by inverting the rod, and taking a mean of the results in each position, to obviate any error which might arise from a want of uniformity in density or figure. After numerous trials however, and as frequent disappointments, I was at length convinced of the impracticability of obtaining a rod sufficiently uniform; and I was besides aware, that under certain circumstances errors might arise from this cause which it would be impossible by any method to detect.

Not feeling at all satisfied with the prospect which the use of a rod presented, I endeavoured to discover some property of the pendulum of which I might avail myself with greater probability of success; and I was so fortunate as to perceive one, which

* From the Transactions of the Royal Society for 1818, part i.

promised

promised an unexceptionable result. It is known that the centres of suspension and oscillation are reciprocal; or, in other words, that if a body be suspended by its centre of oscillation, its former point of suspension becomes the centre of oscillation, and the vibrations in both positions will be performed in equal times. Now the distance of the centre of oscillation from the point of suspension, depending on the *figure* of the body employed, if the arrangement of its particles be changed, the place of the centre of oscillation will also suffer a change. Suppose then a body to be furnished with a point of suspension, and another point on which it may vibrate, to be fixed as nearly as can be estimated in the centre of oscillation, and in a line with the point of suspension and centre of gravity. If the vibrations in each position should not be equal in equal times, they may readily be made so, by shifting a moveable weight, with which the body is to be furnished, in a line between the centres of suspension and oscillation; when the distance between the two points about which the vibrations were performed being measured, the length of a simple pendulum and the time of its vibration will at once be known, uninfluenced by any irregularity of density or figure*.

An unexceptionable principle being thus adopted for the con-

* In the *Connoissance des Temps* for 1820, is an article by M. de Prony on a new method of regulating clocks. At the conclusion of this article is a short note, in which the author adds, "J'ai proposé en 1790 à l'Academie des Sciences un moyen de déterminer la longueur du pendule en faisant osciller un pendule composé sur *deux* ou trois axes attachés à ce corps. (voyez mes Leçons de Mécanique, art. 1107 et suivans.) Il paroit qu'on a fait ou qu'on va faire usage de *ce moyen* en Angleterre." On referring to the Leçons de Mécanique, as directed, I can perceive no hint whatever of the possibility of determining the length of the seconds pendulum by means of a compound pendulum vibrating on *two* axes; but it appears that the method of M. de Prony consists in employing a compound pendulum having *three* fixed axes of suspension, the distances between which, and the time of vibration upon each, being known, the length of three simple equivalent pendulums may thence be calculated by means of formulæ given for that purpose. M. de Prony indeed proposes employing the theorem of Huygens, of which I have availed myself, of the reciprocity of the axis of suspension and that of oscillation, as *one* amongst other means of simplifying his formulæ, and says, "J'ai indiqué les moyens de concilier avec la condition à laquelle se rapportent *ces formules,* celle de rendre l'axe moyen le réciproque de l'un des axes extrêmes; J'emploie pour les ajustemens qu'exigent *ces diverses conditions* un poids curseur dont j'ai exposé les propriétés dans un mémoire publié avec la Connoissance des Temps de 1817." Now it appears evident from this passage, that M. de Prony viewed the theorem of Huygens *solely* with reference to the simplification of his formulæ; for, had he perceived that he might thence have obtained *at once* the length of the pendulum without further calculation, the inevitable conclusion must instantly have followed, that his third axis and his formulæ were wholly unnecessary.

struction

struction of the pendulum, it became of considerable importance to select a mode of suspension equally free from objection. Diamond points, spheres, and the knife edge, were each considered; but as it was found difficult to procure diamond points sufficiently well executed, the knife edge was preferred, after many experiments had been made with spheres, the result of which it may not be useless for a moment to dwell upon.

It is known, that if two curved surfaces be ground together in every possible direction, they will become portions of spheres; and thus a perfect sphere may be formed by grinding a ball in a hemispherical cup. If a pendulum vibrate on such a sphere, working in a conical aperture, it is evident that the centre of the sphere will be accurately in the axis of vibration. In trying this method, however, it was found that the friction was so considerable, as to bring the pendulum to a state of rest after a few vibrations; and when the friction was sufficiently diminished, by a contrivance which it is unnecessary to describe, the *lateral* force of the pendulum in an arc of two degrees and a half, was sufficiently powerful to carry the ball entirely out of the socket; and it was consequently evident, that though the arc of vibration might not be large enough to effect this, it must necessarily cause the ball in some degree to ascend the inclined plane of the aperture; and this consideration induced me to abandon at once a mode of suspension which I should otherwise have esteemed the best that could have been employed.

The principal objections to the use of a knife edge, appeared to be, the difficulty of forming it perfectly straight, and the possibility that it might suffer a change of figure from wear, during the experiments, which might introduce an error not to be detected. The first of these objections I found to be perfectly groundless, as a knife edge can be made so as not to deviate sensibly from a right line. The second objection would indeed be of weight, were the usual method of determining the time of vibration resorted to, by comparing the pendulum with a clock, at the distant intervals of 24 hours; but it will hereafter appear, that should any alteration in the form of the knife edge take place, it must become perceptible every ninth minute; in addition to which, I proposed to measure the distance of the knife edges both before and after the experiments, when any change would of course be immediately detected.

Description of the Pendulum employed.

The pendulum constructed upon these principles is formed of a bar of plate brass, one inch and a half wide, and one eighth of an inch thick. Through this bar, two triangular holes are made, at the distance of 39·4 inches from each other, to admit the

the knife edges. Four strong knees of hammered brass of the same width as the bar, six inches long and three quarters of an inch thick, are firmly screwed by pairs to each end of the bar, in such a manner, that when the knife edges are passed through the triangular apertures, their backs may bear steadily against the perfectly plane surfaces of the brass knees, which are formed as nearly as possible at right angles to the bar. The bar is cut of such a length, that its ends may be short of the extremities of the knee pieces about two inches.

Two slips of deal 17 inches long, and of the same thickness as the bar, are inserted in the spaces thus left between the knee pieces, and are firmly secured there by pins and screws. These slips of deal are only half the width of the bar; they are stained black, and in the extremity of each a small whalebone point is inserted, for the purpose of indicating the extent of the arc of vibration.

A cylindrical weight of brass, three inches and a half diameter, one inch and a quarter thick, and weighing about two pounds seven ounces, has a rectangular opening in the direction of its diameter, to admit the knee pieces of one end of the pendulum. This weight being passed on the pendulum, is so thoroughly secured there by means of a conical pin fitting an opening made through the weight and knee pieces, as to render any change of position impossible. A second weight, of about seven ounces and a half, is made to slide on the bar near the knife edge at the opposite end; and this weight may be fixed at any distance on the bar by two screws with which it is furnished.

A third weight, or rather slider, of only four ounces, is moveable along the bar, and is capable of nice adjustment by means of a screw fixed to a clamp, which clamp is included in the weight. This slider is intended to move near the centre of the bar. It has an opening, through which may be seen divisions, each equal to one-twentieth of an inch, engraved on the bar; and a line is drawn on the edge of the opening to serve as an index to determine the distance of the slider from the middle of the bar.

We now come to the most important part, the knife edges. These are made of that kind of steel which is prepared in India, and known by the name of wootz. Their form is triangular, and their length one inch and three quarters. Mr. Stodard was so obliging as to forge them for me: they were made as hard as possible, and tempered by immersing them merely in boiling water.

The knife edges were ground on a plane tool, which necessarily ensured a perfectly straight edge. This was ascertained by bringing the edge of the one in contact with the plane of the other,

other, when, if no light was perceptible between them in any position, it was inferred that the edge was a right line. They were then carefully finished on a plane green hone, giving them such an inclination as to make the angle on which the vibrations are performed about 120 degrees.

Previously to the knife edges being hardened, each was tapped half way through, near the extremities, to receive two screws, which being passed through the knee pieces, drew the knife edges into close contact with them, the surfaces of both having been previously ground together to guard against any strain which might injure their figure.

The Support, and other Apparatus.

The support of the pendulum consists of a piece of bell metal six inches long, three inches wide, and three-eighths of an inch thick. An opening is made longitudinally through half the length of the piece, to admit the pendulum, and the bell metal is cast with a rectangular elevation on each side of the opening extending the whole length of the piece. Two plates of agate* were cemented to this elevated part, beds having been made to receive them, in order that their surfaces might be in the same plane with that of the bell metal. The whole was then ground perfectly flat. A frame of brass is attached by two opposite screws, which serve as centres, to the sides of the elevated part of the support; and one end of this frame being raised or depressed by means of a screw, the pendulum when placed with its knife edge resting in Ys, at the other end of the frame, could be elevated entirely above the surface of the agate, or be gently lowered until the knife edge rested wholly upon it; and thus the knife edge was sure to bear always precisely on the same part of the agate plane, by elevating the Ys above its surface, placing the knife edge in them, and then letting down the whole gently by means of the screw, till the Ys were completely clear of the knife edge. The support was firmly screwed to a plank which will hereafter be described.

To the kindness of Henry Browne, Esq. F.R.S., I am essentially indebted for the success of the experiments which form the subject of this paper. He most obligingly allowed me the use of his house, his excellent time-pieces, and transit instrument, assisting me with indefatigable zeal by his very accurate daily observations, and intermediate comparisons for determining the rate of the clock. The house is substantially built, and is situ-

* Plates of hard steel were first tried, but were found to have suffered penetration by the knife edge.

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ated in a part of Portland Place not liable to much disturbance from the passing of carriages. The room in which the experiments were made is the last of two on the ground floor, communicating with each other and facing the north. The temperature consequently is very steady, and, if necessary, may be raised to any given degree by a fire in the first room. The clock with which the pendulum was compared was made by Arnold; and in addition to the gridiron compensation for temperature, its pendulum is suspended by a spring, the strength of which is so adjusted, that the vibrations in different arcs are performed in equal times. This clock is firmly screwed to the wall, in a recess opposite to the window. Near to this, on the wall which is at right angles to the recess, is fixed another time-piece by Cumming, which was the property of the late General Roy, and is considered by Mr. Browne to be the best in his possession. Respecting this clock, it will be sufficient to remark, that three-tenths of a second was the greatest variation in its daily rate from the 22d February, when the observations commenced, to the 31st July; and consequently the deviation from its mean rate during that period, did not exceed 0·15 of a second per day. This clock has been used as the standard of comparison, the time having been taken from the transit instrument by a chronometer of Arnold's. With such advantages it will be confessed that there can be little chance of error arising from the rate of the clock.

A plank of well seasoned mahogany, two feet wide, and three inches thick, was forcibly driven between the walls forming the sides of the recess, until it was near the top of the clock case. To this the support of the pendulum before described was firmly screwed, and carefully levelled, in such a position as to allow the pendulum to vibrate as near as possible to the clock-case without touching it; and that, when at rest, it might appear to an observer in front of the clock, to pass over the centre of the dial-plate, its extremity reaching a little below the centre of the ball of the pendulum. Beneath, fixed to the clock-case, was an arc divided into degrees and tenths, to determine the extent of the vibrations. Such a portion of the plank was cut away as was necessary to admit of the pendulum being placed on its support. A circular white disk was pasted on a piece of black paper, which was attached to the ball of the pendulum of the clock; and this disk was of such a diameter, as, when both pendulums were at rest, to be just hid from an observer at the opposite side of the room, by one of the slips of deal which form the extremities of the brass pendulum.

Though there was little reason to imagine that the vibrations of the pendulum could communicate any motion to a support so firm

firm as that which has been described, it became a point of considerable importance to verify this by actual experiment. For this purpose I had recourse to a delicate and simple instrument invented by Mr. Hardy, clock-maker, the sensibility of which is such, that had the slightest motion taken place in the support, it must have been instantly detected. This little instrument consists of a steel wire, the lower part of which inserted in the piece of brass which serves as its support, is flattened so as to form a delicate spring. On the wire, a small weight slides, by means of which it may be made to vibrate in the same time as the pendulum to which it is to be applied as a test. When thus adjusted, it is placed on the material to which the pendulum is attached; and should this not be perfectly firm, its motion will be communicated to the wire, which in a little time will accompany the pendulum in its vibrations. This ingenious contrivance appeared fully adequate to the purpose for which it was employed, and afforded a satisfactory proof of the stability of the point of suspension.

A firm triangular wooden stand, as high as the ball of the pendulum, was screwed to the floor at the distance of nine feet in front of the clock. This served as a support, to which was attached a small telescope, magnifying about four times, which was capable of a horizontal motion on its axis, a vertical motion, and a motion at right angles to the line of sight. In the focus of the eye-glass was a diaphragm forming a perpendicular opening, the sides of which were parallel, and capable of being placed nearer, or further asunder. The edges of this diaphragm were adjusted so as to form tangents to the horizontal diameter of the white disk, and consequently to coincide with the edges of the slip of deal. When, therefore, both pendulums were at rest, nothing was visible through the telescope, excepting the divided arc for ascertaining the extent of the vibrations, and which was seen through a horizontal opening made for that purpose in the top of the diaphragm.

Method of determining the Number of Vibrations made by the Pendulum in twenty-four Hours.

If both pendulums be now set in motion, the brass pendulum a little preceding that of the clock, the following appearances may be remarked. The slip of deal will first pass through the field of view of the telescope at each vibration, and will be followed by the white disk. But the distance between the centres of suspension and oscillation in the brass pendulum being rather the longer, the pendulum of the clock will gain upon it, the white disk will gradually approach the slip of deal, and at length, at a certain vibration, will be wholly concealed by it. The minute and

and second at which this total disappearance is observed, must be noted. The pendulums will now be seen to separate, and after a time will again approach each other, when the same phænomenon will take place. The *interval* between the two coincidences in seconds, will give the number of vibrations made by the pendulum of the clock; and the number of oscillations of the brass pendulum, in the same interval, may be known by considering that it must have made two oscillations less than the pendulum of the clock. Hence by simple proportion, as the vibrations made by the pendulum of the clock are to the number of vibrations made by the brass pendulum, so are the vibrations made by the pendulum of the clock in 24 hours, to those of the brass pendulum in the same period*.

Many experiments were made in order to select such a distance of the knife edges as might give an interval which would allow of the determination of the time of coincidence without an error of a single second †, and yet afford a convenient number of intervals before it should become necessary to renew the motion of the pendulum. At the first coincidence, the velocity of the brass pendulum, at the lowest part of the arc, must not exceed that of the pendulum of the clock, otherwise the disk would disappear for an imperceptible time, and then re-appear; and this limits the extent of the arc of vibration.

Again; the observations must not be continued beyond a certain diminution of the arc of vibration; otherwise the *space*, which the pendulum of the clock has to gain on the brass pendulum in one vibration, becomes so small as to render the observation of the time of coincidence in some degree uncertain; and, should the *space* be so far diminished as to be less than the error or deviation from a right line, which would probably take place in the adjustment of the sides of the diaphragm, the end of the pendulum, and the disk, the results would be erroneous, as the interval would go on increasing till the pendulum came to a state of rest.

The interval which best fulfilled these conditions was found to be about 530 seconds. This admitted five coincidences (affording four intervals) to be taken before the arc became too small for the observations to be continued with safety. With this interval an error of one second in the time of coincidence

* In order to render the calculation more easy, the clock has always been supposed to keep mean time, or to make 86,400 vibrations in 24 hours, and the variation from this number, or the rate of the clock (being a very small quantity) has been afterwards applied as a correction.

† The *principle* on which this method of coincidences is founded, was employed by Dr. Wollaston, in May 1808, in some experiments in which he was then engaged, the moment of coincidence being determined however by sound instead of sight.

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would occasion an error of only 0·63 in the number of vibrations in 24 hours.

Here it must be evident that no sensible alteration could take place in the knife edge during the experiments without its becoming perceptible at every coincidence, since the number of vibrations in 24 hours deduced from each interval, must vary with any change in the form of the knife edge.

The following was the method pursued in making the observations. The small weight or slider being placed with its index at a certain distance (say one inch and a half) from the middle of the pendulum towards the great weight, and the second weight about five inches from the knife edge, the Ys of the support were elevated, the knife edge of the pendulum was placed in them, with the great weight *above*, and the frame gently lowered till the knife edge was left on the surface of the agate. The requisite adjustments of the telescope having been made, the pendulum was set in motion in an arc not exceeding one degree and four-tenths, in order that its velocity might not be greater than that of the pendulum of the clock.

The minute and second, at which the disk ceased to be visible, was then carefully noted; and the arc of vibration seen through the telescope, the height of the barometer, and the temperature indicated by a thermometer suspended on the clock-case near the middle of the brass pendulum, were also observed and registered. Five successive coincidences were thus taken, and the number of vibrations in 24 hours was deduced from them in the manner before described; but the vibrations thus obtained being made in different arcs, it became necessary to apply a correction to determine what they would have been in an arc infinitely small. For this correction I might have used a formula depending on the decrease of the arcs in geometrical progression, whilst the times decrease in arithmetical; but as there is an uncertainty in observing the arc of vibration amounting to one or two hundredths of a degree, this method, though more perfect in theory, would have been an unnecessary refinement in practice.

The error arising from the greater length of the vibration in a circular arc, being nearly as the square of the arc, if the mean of the observed arcs at the commencement and end of each interval be taken, and its square multiplied by 1·635, (the difference between the number of vibrations made by the pendulum in 24 hours, in a cycloid and in an arc of one degree,) the required correction will be obtained, to be added to the number of vibrations before computed.

The mean of these last results being taken, and also the mean of the observed temperatures at the first and last coincidences, the number of vibrations in 24 hours was obtained at a certain temperature,

temperature, and altitude of the barometer, in an infinitely small arc, the great weight being *above.*

The frame of the support was now elevated, the pendulum was inverted, placed in the Ys, with the great weight *below;* and the knife edges being gently let down as before on the agate plane, the same process with respect to the observations was followed, which has just been described. And if the mean temperature differed from that in the former position of the pendulum, the mean number of vibrations was corrected for such difference of temperature, the expansion of the pendulum being known by experiments hereafter to be detailed, and consequently the gain or loss in 24 hours by a given change of temperature.

The mean number of vibrations thus found, differing from that given in the former position of the pendulum, the second weight was moved, the number of vibrations again determined; and the pendulum being inverted, the process was repeated until the vibrations in 24 hours, in either position of the pendulum, were brought as near to an equality as could readily be effected by means of this weight: it was then firmly secured in its place.

Whatever alteration may be made in the arrangement of the weights, the effect on the vibrations (except in one particular instance) will be the same in both positions of the pendulum, always increasing or diminishing their number in both cases, though in different degrees; and the vibrations will be least affected by such change when the great weight is below, and will consequently be nearest to the truth in this position. No doubt, therefore, can arise, as to the kind of correction required. The number of vibrations after the adjustment by the second weight has been completed, must be left *in defect,* for a reason which will be immediately apparent.

There is a point in the pendulum where the effect of the slider in increasing the number of vibrations is a maximum; and it appears from Dr. Young's investigations, that this point in one position of the pendulum is different from that in the other. *Very near* either of these points, the pendulum being in its corresponding position, the motion of the slider produces scarcely any change in the number of vibrations; but the slider being then more distant from the point of maximum belonging to the other position of the pendulum, the corresponding increase of the number of vibrations arising from such motion of the slider, will in that position be very perceptible.

In the present instance, the point of maximum, in either position of the pendulum, is about four-tenths of an inch below the middle, and consequently the distance of the two points from each other is about eight-tenths of an inch. The slider, which had remained stationary during the adjustment of the second

weight

weight at about one inch and a half from the middle of the pendulum towards the great weight, must now be shifted (say one inch) towards the middle of the pendulum, in order to increase the number of vibrations which it may be recollected were left in *defect*, so that they may be in *excess*. It is evident that the true number of vibrations will be found, when the slider is somewhere between its first and second position. Let the slider be now placed half-way between these two points. If the number of vibrations in this third position be still in excess, the truth will lie between the first and third positions of the slider. And thus, by continually bisecting with the slider, the distance of the two last found points, the number of vibrations when the great weight is *below*, will rapidly approach the truth, being alternately in defect and in excess; and when the approximation is such as that the difference in either position of the pendulum becomes inconsiderable, the vibrations, when the great weight is below, may be taken for the truth; and thus the number of vibrations in 24 hours, of a pendulum equal in length to the distance between the knife edges, will be known at a certain temperature, and at an observed height of the barometer.

[To be continued.]

XV. *Account of Experiments made by the Assay Master of the King of the Netherlands, at the Mint of Utrecht, on the Native Copper existing in Blocks on the South Side of Lake Superior, communicated by a Letter from Mr.* EUSTIS, *Minister Plenipotentiary and Envoy Extraordinary from the United States, &c. to Dr.* SAMUEL L. MITCHILL, *dated Hague, Oct.* 12, 1817.

DEAR SIR,—PERCEIVING by the public newspapers, that my friend Dr. Le Barron had presented you a piece of copper, I inclose the analysis of a piece which he gave me at the mint of Utrecht, a portion of which, in its crude state, I presented to the minister of foreign affairs, to be deposited in the university of Leyden. My object in procuring an assay in a foreign country, was first to add to the diffusion of information respecting our country; and secondly, that it might be compared with experiments made in the United States. I had hoped to return this autumn, and to have taken it with me; but the state of our commercial relations with this country has necessarily deferred that hope until the spring. I am, &c.

The Hon. Samuel L. Mitchill. W. EUSTIS.

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The Report from the mint is in these words:

From every appearance, the piece of copper seems to have been taken from a mass that has undergone fusion. The melting was, however, not an operation of art, but a natural effect, caused by a volcanic eruption.

The stream of lava probably carried along, in its course, the aforesaid body of copper that had formed into one collection, as fast as it was heated enough to run, from all parts of the mine. The united mass was probably borne in this manner to the place where it now rests in the soil.

The crystallized form observable every where on the original surface of the metal that has been left untouched or undisturbed, leads me to presume that the fusion it has sustained was by a process of nature; since this crystallized surface can only be supposed to have been produced by a slow and gradual cooling, whereby the copper assumed regular figures as its heat passed into other substances, and the metal itself lay exposed to the air.

As to the properties of the copper itself, it may be observed that its colour is a clear red; that it is peculiarly qualified for rolling and forging; and that its excellence is indicated by its resemblance to the copper usually employed by the English for plating.

The dealers in copper call this sort Peruvian copper, to distinguish it from that of Sweden, which is much less malleable. The specimen under consideration is incomparably better than Swedish copper, as well on account of its brilliant colour, as for the fineness of its pores, and its extreme ductility.

Notwithstanding, before it is used in manufactures, or for the coining of money, it ought to be melted anew, for the purpose of purifying it from such earthy particles as it may contain.

The examination of the North American copper, in the sample received from his excellency the minister, by the operations of the cupel and the test by fire, has proved that it does not contain the smallest particle of silver, gold, or any other metal.

XVI. *Experiments on the Relation between Muriatic Acid and Chlorine. By* ANDREW URE, *M.D. Professor of the Andersonian Institution, and Member of the Geological Society.**

THE Chloridic theory, though more limited in its application to chemical phænomena than the Antiphlogistic, may justly be

* From the Transactions of the Royal Society of Edinburgh.

regarded as of scarcely inferior importance. If established, it leads to the adoption of entirely new views concerning combustion and many of its products; it removes the muriates, a set of apparently well characterized saline bodies, from the class of salts altogether; and it has given birth, by analogy, to two new genera of compounds, in which iodine and fluorine, like chlorine, act a corresponding part with oxygen, in the system of Lavoisier.

This new era in chemical science unquestionably originated from the masterly researches of Sir Humphry Davy on oxymuriatic acid gas; a substance which, after resisting the most powerful means of decomposition which his sagacity could invent, or his ingenuity apply, he declared to be, according to the true logic of chemistry, an elementary body, and not a compound of muriatic acid and oxygen, as was previously imagined, and as its name seemed to denote. He accordingly assigned to it the term Chlorine, descriptive of its colour; a name now generally used.

Chlorine when combined with an equal volume of hydrogen forms muriatic acid gas, the hydrochloric of Gay-Lussac. This muriatic acid gas, hygrometrically dry, unites with its own bulk of dry ammoniacal gas, to constitute the dry pulverulent solid called sal ammoniac. Hence this saline body is ultimately composed of chlorine and hydrogen, for its acid; and of azote and hydrogen for its base. By comparing the weights of muriatic acid and ammoniacal gases, in equal volumes, we obtain the proportion of 67·8 muriatic acid gas to 32·2 ammonia, for the composition of 100 parts by weight of the solid salt. If we saturate liquid muriatic acid with gaseous ammonia, or with the base of the ammoniacal carbonate, and evaporate carefully to dryness, we find the resulting salt to have precisely the same constitution, namely, in 100 parts, 51 of dry muriatic acid, equivalent to 67·8 of the acid gas, and the remainder 32·2 ammonia. This concurrence of results, whatever way the salt may be obtained, is fully demonstrated in my researches on the ammoniacal salts, (*Annals of Philosophy for September* 1817,) and proves it to be a substance of very uniform and determinate composition.

Those chemists who consider chlorine to be oxymuriatic acid must suppose, when a volume of it weighing 44·13 unites with an equal volume of hydrogen weighing 1·32, that, in the resulting hydrochloric or muriatic acid gas =45·45, this hydrogen exists combined with 10·00 of oxygen, its saturating quantity, forming 11·32 of constituent water. In this view, muriatic acid gas, like gaseous, sulphuric, and nitric acids, contains water as an essential element. There seems to be no violation of chemi-

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cal analogy in this supposition. The quantity will be represented by the fraction $\frac{11 \cdot 32}{45 \cdot 45}$, being nearly one-fourth.

If chlorine, however, be a simple body, which forms with hydrogen muriatic acid gas, then sal ammoniac is rightly named Hydrochlorate of Ammonia. And since ammonia itself results from three volumes of hydrogen and one of azote, condensed into two volumes, that saline body can contain neither water, nor its indispensable element oxygen.

On the other hand, if chlorine be oxymuriatic acid, then the fourth part of water existing in the resulting muriatic acid gas must necessarily enter into the sal ammoniac as an essential constituent; for the whole ponderable matter of that gas, as well as of the ammonia, passes into the salt. This water being as indispensable an ingredient of sal ammoniac as it is of oil of vitriol; heat alone can no more separate it from the former, than it can from the latter compound.

Moreover, if we decompose sal ammoniac by the agency of any body containing oxygen, an evident source of fallacy exists relative to the watery product, which may be referred by the supporters of the chloridic theory, not to the salt itself, but to the hydrogen of the hydrochloric acid, united with the oxygen of the decomposing substance. This ambiguous interpretation is experimentally illustrated in my paper on the Ammoniacal Salts.

If, however, we shall decompose that equivocal salt, by means of a substance which certainly contains no oxygen; and if we still obtain water in nearly the above proportions; then this result is no longer equivocal, nor will it admit of two interpretations. We must thenceforth be compelled to recognise in muriatic acid gas, as in the other acid vapours, WATER as an ingredient essential to its constitution; and to acknowledge that chlorine consists of a base united to oxygen, or is in fact oxygenated muriatic acid, as Lavoisier and Berthollet taught, and as the whole chemical world believed, till their faith was lately shaken or subverted by the predominating genius of Sir Humphry Davy.

With the view of deciding the above important controversy, I performed the following experiments:

Of sal ammoniac, kept for some time in a platina capsule at a subliming heat, to remove every particle of adhering moisture, a known quantity was put into a glass tube, and made to slide down to the one end, which had been hermetically sealed. Over it a given weight of bright metallic laminæ, cut into slender segments, was slightly pressed. The salt occupied in general about one inch of the tube; the laminæ four or five inches. Silver, copper, and turnings of iron made with a dry tool, were employed in successive experiments. The open extremity of the tube was drawn out to a point, and recurved, so as to pass under a vessel inverted on the mercurial pneumatic trough. Between this and the portion containing the metal, there was a length of six or more inches of tube, which was kept cool. In one variation of the experiment, a tube of Reaumur's porcelain was used for containing the materials, to which was firmly luted, by a collar of caoutchouc, a glass tube, with a little globe blown in its middle, and its loose end plunged, as usual, into the mercurial trough.

When tubes of crystal glass were employed, the part containing the materials was lodged in a semicylindrical case of iron, which traversed a small charcoal furnace five or six inches in diameter. The metallic laminæ being raised to full ignition in day-light, the case and tube were slightly moved forward, in order to bring a little of the salt within the sphere of the heat. Great nicety was required in the advancement of the sealed extremity; for the glass tube being perfectly softened in its middle, too sudden volatilization of the salt never failed, by inflating and bursting it, to spoil the experiment. This accident frequently happened. On the other hand, if the central part of the tube was exposed to merely a dull red, the experiment would not succeed with silver and copper. At this temperature they did not decompose the sal ammoniac. When, however, the above-mentioned precautions were observed, dew could be perceived to settle speedily on the cool portion of the tube. This dew became more and more visible as the sublimation advanced, till, finally collecting into distinct drops, it trickled down the sides in striæ, and formed a filament along the bottom. To obtain good results of this kind, four or five hours must be devoted to one experiment, in which 20 grains of salt, and from 60 to 100 of metal, are employed. More rapid transmission of the salts effects mere sublimation. Bubbles of gas come over, which, with silver and copper laminæ, are found to be a mixture of ammonia and hydrogen. In this case, the liquid condensed is water of ammonia.

The metallic laminæ are evidently heavier than before their introduction; but the increase of their weight could not be exactly ascertained, because a portion of the silver or copper is impressed on the inner surface of the tube, giving it a very beautiful iridescent and metallic lustre, similar to the colours of the diamond beetle viewed in a microscope. The silver laminæ have for the most part exchanged their native brilliant white, for a dull brown or grayish hue; and, instead of being eminently tough and ductile, have become more brittle than any substance

with

with which I am acquainted. The slightest touch of the finger breaks them across. Digested in pure nitric acid somewhat dilute, the segments only partially dissolve, bits of muriate of silver, of their own shape, being left in the liquid.

The ignited copper turnings, after experiencing the action of sal ammoniac, are found to have lost also their original lustre, and have acquired a dull brown colour. Digested in water, a liquid muriate is obtained, which gives the characteristic brown precipitate with prussiate of potash.

The most considerable of my experiments with turnings was made with the tube of Reaumur's porcelain, which, as it contains no oxide of lead, is not liable to any ambiguity on this score, and being capable of sustaining a very high heat without fusion, permitted me to obtain very satisfactory results indeed.

Thirty grains of recently heated sal ammoniac being put down to the sealed end, 200 grains of bright turnings of very pure soft iron were introduced over it, so as to occupy six inches of the tube. The glass tube above described, was attached by the elastic gum collar. The part holding the iron being brought to bright ignition, the sealed end of the tube was advanced by degrees almost imperceptible. As soon as the salt began to exhale, moisture began to condense in the glass tube, though none ever appeared prior to heating the sal ammoniac. The evolution of gas was much more copious than in any of the experiments with the other metals. When allowed to escape through the quicksilver into the air, it exhibited the dense cloud, and had the odour, of muriatic acid. Received into a tube over mercury, and then exposed to the action of water, $\frac{5}{100}$ parts of the volume were absorbed, which on trial were found to be pure muriatic acid. The remainder was a mixture of azote and hydrogen, in the proportions very nearly that are known to constitute ammonia. I analysed this mixed gas, by explosion with half its volume of pure oxygen, in a peculiar apparatus which I shall describe in the sequel. On firing 100 measures with the electric spark, 76·2 disappeared, two-thirds of which, $=50·8$, are hydrogen. Before explosion, the hundred volumes consisted of $66\frac{2}{3}$ ammoniacal gaseous matter, $+33\frac{1}{3}$ oxygen. Of these $66\frac{2}{3}$ parts, 50·8 are hydrogen, and 15·86 azote; or, in the 100, $76·2 + 23·8$. But, by Gay-Lussac, one volume of azote unites with three volumes of hydrogen to form ammonia Hence 23·8 measures of azote should have been accompanied with only 71·4 of hydrogen, instead of 76·2 actually obtained. This excess of hydrogen is due to the decomposition of a little of the watery product, in the formation of the muriate of iron. That muriate of iron is formed, is proved by many circumstances. First, the disappearance of the acid in the gaseous products. Sal ammoniac

moniac being decomposed into its ultimate gases, will consist of two measures of those constituting the alkali + one measure of the muriatic. Hence 100 volumes should contain $33\frac{1}{3}$ of this acid gas; but they actually contained only about 5. Therefore about 28 measures, which form the difference, were condensed with the iron. Secondly, the iron turnings had increased in weight; they deliquesced speedily on exposure to the atmosphere; and, digested in water, they yielded an acerb-tasted solution of muriate of iron, giving with prussiate of potash a copious blue precipitate.

The quantity of muriate produced in the experiment will depend on the proportion of turnings which have been but moderately heated; for the ammonia, in its passage over the strongly ignited iron, may be conceived to separate the oxygen, and thus prevent the formation of muriate.

Water impregnated with muriatic acid equal in weight to nearly one-sixth of the sal ammoniac decomposed, is uniformly obtained by the above process. Scarcely a particle of ammonia seems to escape entire decomposition. The evolved muriatic acid, amounting to $\frac{5}{100}$ of the whole gaseous products, must carry off with it a portion of its constituent water. Hence we ought to find a little less water here condensed, than, by my experiments on the ammoniacal salts above referred to, sal ammoniac, viewed as a muriate, is shown to contain.

It seems evidently to follow, from this experimental detail, that chlorine is oxygenated muriatic acid. Since dry sal ammoniac consists of ammonia and muriatic acid gases, both hygrometrically dry; and since water is obtained in its decomposition by pure metals; this water must have existed in the gaseous acid; for all experiments concur in proving ammonia itself to contain nothing but azote and hydrogen. And, finally, since muriatic acid gas is a compound of chlorine and hydrogen, the water derived from the resulting muriatic acid, demonstrates the presence of oxygen in the chlorine, or, in other words, that it is really oxymuriatic acid *.

All the experimental phænomena hitherto adduced in the chloridic controversy, were susceptible of explanation on both the old and new doctrine. Thus, the hydrogen which remains after tin is subjected at a high temperature to muriatic acid gas, could be regarded, with Davy, as resulting from a metallic ana-

* If the Chloridic theory be still retained, then the production of water in the above circumstances can be ascribed only to the decomposition of azote into oxygen and hydrogen, as has been already indicated in my paper on the Ammoniacal Salts. It is possible that this alternative may eventually be found the true one; yet, in the present state of our knowledge, such an inference would be illogical.

lysis

lysis of hydrochloric acid; or it might be derived from the combined water of muriatic acid, of which the oxygen became fixed in the muriate of tin. When chlorine also at high heats was made to act on earths or common metallic oxides, the evolved oxygen could be referred with equal probability either to the solid or to the gas.

And though we ignite by the strongest Voltaic power, charcoal or other combustibles in chlorine, still we shall not be able to convert it into muriatic acid gas, for want of the essential constituent water; no more than we can, without the same water, obtain oil of vitriol. Present water to chlorine, then light alone will separate its oxygen, and leave muriatic acid. Such, indeed, is the affinity existing between the muriatic acid basis and water, that those muriates which of themselves resist decomposition at a red heat, when exposed at that temperature to the vapour of water, are speedily resolved into gaseous muriatic acid, and their peculiar bases.

By restoring the theory of Lavoisier and Berthollet, we get rid of those mysterious and almost incomprehensible transformations which a drop of water has been lately conceived to produce on some of the muriates. Dried sea-salt, for example, when viewed as a compound of chlorine and sodium, is no sooner moistened, than a portion of water resolves itself into oxygen and hydrogen; whence result soda and hydrochloric acid, and a solution of muriate of soda. Expel the drop of water, we have a chloride of sodium once more; and we may repeat this invisible change for an indefinite number of times by the addition or subtraction of a little moisture. Thus we must consider dry salt and moist salt to be bodies widely and essentially different, the former containing neither alkali nor acid, while the latter contains both. This supposition, which the chloridic theory compels us to make, must surely be reckoned somewhat violent.

XVII. *Experiments on Muriatic Acid Gas, with Observations on its Chemical Constitution, and on some other Subjects of Chemical Theory.* By John Murray, *M.D. F.R.S. E. Fellow of the Royal College of Physicians of Edinburgh.**

Some years ago I proposed, as decisive of the question which has been the subject of controversy on the nature of oxymuriatic and muriatic acids, the experiment of procuring water from muriate of ammonia, formed by the combination of dry ammoniacal

* From the Transactions of the Royal Society of Edinburgh.

niacal and muriatic acid gases. Muriatic acid gas being the sole product of the mutual action of oxymuriatic gas and hydrogen, it follows, that if oxymuriatic gas contain oxygen, muriatic acid gas must contain combined water; while, if the former be a simple body, the latter must be the real acid, free from water. When muriatic acid gas is submitted to the action of substances which combine with acids, water is obtained; but though the most simple and direct conclusion from this is, that the water is deposited from the muriatic acid gas, the result may be accounted for, on the opposite doctrine, by the supposition that it is water formed by the combination of the hydrogen of the acid with the oxygen of the base. Ammonia, however, containing no oxygen, if water is obtained from its combination with muriatic acid gas, we obtain a result which cannot be accounted for on this hypothesis, but must be regarded as a proof of the presence of water in the acid gas. And this, again, affords a proof equally conclusive of the existence of oxygen in oxymuriatic gas.

The results of the experiment which I had brought forward were involved in much controversial discussion: and a brief recapitulation of the objections that were urged to it is necessary, as an introduction to the experiments I have now to submit, and to the consideration of the present state of the question.

The original experiment was performed by combining thirty cubic inches of muriatic acid gas with the same volume of ammoniacal gas carefully dried. The salt formed was exposed in a small retort with a receiver adapted to it, to a moderate heat gradually raised. Moisture speedily condensed in the neck of the retort, which increased and collected into small globules *.

This result was admitted by those who defended the new doctrine, when the experiment was performed in the manner I have described,—water being obtained, it was allowed, "in no inconsiderable quantity." But, to obviate the conclusion, it was asserted, that this is water which has been absorbed by the salt from the atmosphere. This was affirmed by Sir Humphry Davy, who stated that the salt absorbs water in this manner to a very considerable extent; that it is only from the salt in this state that water can be procured; and that when it is formed from the combination of the gases in a close vessel, and heated without exposure to the air, not the slightest trace of water appears, even when the experiment is performed on a large scale.

The reverse of this I was able to demonstrate by further experimental investigations. It was shown, that the salt absorbs no moisture from the air in the common state of dryness and

* Nicholson's Journal, vol. xxxi. p. 126.

temperature

temperature in which the experiment is performed: when weighed immediately on its formation, in an exhausted vessel, it gains no weight from exposure, but remains the same after a number of hours; and when exposed to the air in the freest manner, it remains, after many days, perfectly dry. It was further shown, that when the other circumstances of the experiment are the same, it yields no larger portion of water when it has been exposed to the air, than it does without this previous exposure. And, lastly, it was proved, that when the salt has been formed, and is heated without the air having been admitted, water is obtained from it. This last result was even at length admitted by those who had advanced the opposite assertion, in an experiment performed with a view to determine the fact. The quantity of water was indeed less than what is procured in the other mode; but this was obviously owing to the circumstances of the experiment being unfavourable to its expulsion,—more particularly to the difficulty of applying a regulated temperature to a thin crust of salt, so as to separate the water without volatilizing the salt itself,—and to the effect arising from the whole internal surface of a large vessel being encrusted with the salt; so that, if the heat is locally applied, the aqueous vapour expelled from one part is in a great measure condensed and absorbed at another; or, if the heat is applied equally, is retained in the elastic form, and, as it is cooled, is equally condensed. Accordingly, when the experiment was repeated, obviating these sources of error as far as possible, the water obtained was in larger quantity. And as no fallacy belongs to the conducting the experiment in the more favourable mode in which it was first performed, (the assertion of the absorption of water from the air being altogether unfounded,) the quantity procured in that mode is to be regarded as the real result*.

The argument was maintained, that the water might be derived from hygrometric vapour in the gases submitted to experiment. This it was easy to refute. Dr. Henry had shown that ammonia after exposure to potash, and muriatic acid after exposure to muriate of lime, retain no trace of vapour whatever. And these precautions had been very carefully observed. The assertion was brought forward, too, only to account for the minute quantity of water obtained in that mode of conducting the experiment which affords the least favourable result; and, were it even admitted to all the extent to which it can be supposed to exist, is inadequate to account for the larger quantity obtained in the other.

* Nicholson's Journal, vol. xxxii. p. 186 &c.; vol. xxxiv. p. 271.

That

That the entire quantity of water contained in the muriatic acid gas is not to be looked for, is evident from the nature of the ammoniacal salt, particularly its volatility, whence the due degree of heat to effect the separation of the water cannot be applied. If the other muriates yield the greater part of their water, only when raised nearly to a red heat, (which is the case,) it is not to be supposed that muriate of ammonia shall do so at a temperature so much lower, as that which it can sustain without volatilization. What is to be expected, is a certain portion of water, greater as the arrangements employed are better adapted to obviate the peculiar difficulty attending the experiment. There *is* a production of water in every form of it; and there exists no just argument whence it can be inferred, that the quantity is less than what ought to be obtained. On the opposite doctrine, none whatever should appear.

To effect the more perfect separation of the water from the muriate of ammonia, I had performed the additional experiment of passing the salt formed from the combination of the two gases in vapour through ignited charcoal, on the principle that by the interposition of the charcoal the transmission of the vapour would be impeded, and it would be exposed to a more extensive surface, at which a high temperature would operate, while some effect might also be obtained from the affinities exerted by the carbonaceous matter. To remove any ambiguity from the effect of the charcoal, it was previously exposed in an iron tube to a very intense heat, until all production of elastic fluid had ceased; and removed, while still warm, into a tube of Wedgwood's porcelain, containing the muriate of ammonia, which was then placed across a furnace so as to be raised to a red heat. As soon as the vapour of the salt passed through the ignited charcoal, gas was disengaged, which was conveyed by a curved glass tube adapted to the porcelain one, and received in a jar over quicksilver. Moisture was at the same time pretty copiously deposited, condensing both in the glass tube in globules, and being brought in vapour with the gas, which it rendered opaque, and condensing on the surface of the quicksilver within the jars. The elastic fluid consisted of carburetted hydrogen and carbonic acid, products evidently of the decomposition by the ignited charcoal of a portion of the liberated water. In this experiment, then, the result was still more satisfactory than in the other. That no ambiguity arose from any effect of the charcoal in affording water, is evident from this,—that the water appeared at the moment the salt began to pass in vapour, and at a temperature far below that at which the charcoal had ceased to afford any gas. In another variation of the experiment,

ment, muriate of ammonia was passed in vapour through an ignited porcelain tube alone. Water was obtained in larger quantity than when the salt had been exposed to a heat short of its volatilization; and even the salt which had yielded water by that operation, afforded an additional quantity in this mode,—a proof of the more perfect separation of the water by the effect of a higher temperature*.

By all these results, then, I consider the existence of water in muriate of ammonia, and of course in muriatic acid gas, as demonstrated.

Dr. Ure has lately laid before the Society the result of another mode of conducting the experiment,—that of subliming the muriate of ammonia over some of the metals at the temperature of ignition. Water is thus stated to be obtained in considerable quantity, with a production of hydrogen gas.

No objection appeared to Dr. Ure's experiment, except, perhaps, that the salt operated on was not that formed by the direct combination of its constituent gases, but the common sal ammoniac, in which water might be supposed to exist, either as an essential or an adventitious ingredient, as it is abundantly supplied to it in the processes by which it is formed. I had found, indeed, in some of my former experiments †, that sal ammoniac yields no water when exposed to a heat sufficient to sublime it, but affords it only when exposed to a red heat by transmission of its vapour through an ignited tube;—that, therefore, (owing no doubt to its previous sublimation,) it contains apparently even less water than the salt formed by the combination of the two gases. Still, objections entitled to less consideration than this one, had been maintained in the course of this controversy. I therefore thought it right to repeat the experiment, with the necessary precaution to obviate it, and to observe the actual result.

Thirty grains of muriate of ammonia, formed from the combination of muriatic acid and ammoniacal gases, were put into a glass tube with a slight curvature. Two hundred grains of clean and dry iron filings were placed over it. The tube was put in a case of iron with sand, and placed across a small furnace, so that the middle part, where the iron filings were, was at a red heat, the extremity terminating in the mercurial trough. The salt, from the heat reaching the closed extremity of the tube, soon passed in vapour through the ignited iron. Gas issued from the extremity, and moisture appeared in the cold part of the tube. A large quantity of gas was collected, which had the odour quite strong of muriatic acid, and was in part con-

* Nicholson's Journal, vol. xxxi. p. 128. † Id. vol. xxxiv. p. 274.

densed

densed by water; the residue burned with the flame of hydrogen. The tube, for several inches, was studded with globules of water, and was bedimmed with vapour further. I did not prosecute the experiment, so as to ascertain the weight of water produced, as I had other experiments in view, which I conceived might afford more conclusive results. But it proves the point it was designed to establish, that water is obtained from the salt formed by the combination of the gases, as well as from the common sal ammoniac.

My attention having been thus recalled to the subject, I have again executed the experiment in its original and simplest form, —that of obtaining water from the salt by heat alone; and to this I was led more particularly, as it had occurred to me, that a more perfect abstraction of its water might be effected, by conducting the experiment in an apparatus somewhat on the principle of the instrument invented by Dr. Wollaston, which he named the *Cryophorus*. In a retort of the capacity of seven cubic inches, fitted with a stop-cock, and exhausted, sixty cubic inches of ammoniacal gas were combined with the requisite quantity of muriatic acid gas, each previously carefully dried,—the former by exposure to potash, the latter by exposure to muriate of lime. The stop-cock was then detached from the retort; the excess of ammoniacal gas was removed by a caoutchouc bottle, and replaced by atmospheric air; the salt was pushed down from the neck; and it was connected with another similar retort, the joining of the two being secured by cement. This last retort was also fitted with a stop-cock adapted to a tubulature at its curvature; and heat being applied to it, a little of the included air was allowed to escape. It was then placed in a mixture of muriate of lime and ice, while the other, containing the muriate of ammonia, was placed in warm oil. The heat of this was raised to 420° of Fahrenheit: moisture condensed at the upper part of the neck, when the heat had been raised to 220°, and continued for some time to increase. It then diminished, from the continued application of the heat, carrying it forward into the cold retort; and at the end of the experiment a considerable part of the body of this was encrusted with a thin film of ice. This result, therefore, coincides entirely with what had been before obtained *.

* The other papers in this controversy by Dr. Ure and Dr. Murray, will be given in a subsequent number.

XVIII. *On*

XVIII. *On Chemical Philosophy.* By Mr. MATTHEW ALLAN, *Lecturer.*

[Continued from p. 58.]

Essay VII.

THE GALVANIC APPARATUS consists of alternate arrangements of copper and zinc plates, the sides of which are placed in contact with an acid solution; the acid has a stronger attraction for the zinc than for the copper; the oxygen too of the water is aided in its attraction for the zinc by that contained in the acid; in this way the water and acid are decomposed: the oxygen of both is abstracted; part of it combines with the zinc, but the greater part assumes the gaseous form. In consequence of assuming this gaseous form, there must, it is evident, be a prodigious demand for this caloric or ethereal power, to give and support this new state of existence which it assumes; and at the same time a still larger quantity of hydrogen, the other constituent of water, is set at liberty, and of course there is here demanded a still larger quantity of this power to give it also the gaseous form*. It is this demand which explains the effects produced by the galvanic arrangement, and the explanation is this:—The demand is made through the nearest and best conductor, which in this arrangement must be the copper; the copper is thus robbed of its natural quantity (as is the negative conductor by the revolutions of the cylinder of the electric machine, to be explained presently), and, of course, instantly demands "*its due and relative share*" from the earth and surrounding medium. This supply from the earth and surrounding medium is no sooner received, than it is instantly absorbed by the oxygen and hydrogen assuming the aëriform state; and this current during its passage exhibits the correctness of the law already briefly hinted at,—that bodies are, relatively to others, positive when they are relatively worse conductors. The copper, the zinc, and the solution, are relatively to each other in positive and negative states of existence. But though the galvanic action might, and does in some measure, accumulate in the solution, on the principle of its being the worse conductor; yet this accumulation is in part prevented, by the current demanded to support the changes going on, which stream or current is carried by the conducting power of the metals: so that in this way, as I have already pointed out, there is produced by chemical means a current of this power, as there is by mechanical means in

* It has been frequently repeated, that in every change of existence caloric is given out or absorbed, in the form either of electricity, of galvanism, of caloric, or of light.

electric contrivances: and it is evident that on these principles the galvanic action will continue as long as these gaseous results require and demand this power; and this must continue as long as the surface remains susceptible of oxidation, or capable by the means described of producing these effects of decomposition. The cause, also, why the metal which has the strongest attraction for oxygen is always *positive*, while the other, having less, is *negative*, is explained on the same principles. The oxygen after being separated from its combination with hydrogen in the state of water, and when so separated and having demanded this power to hold it in solution—is again *attracted to, and deposited on*, the metal, *so that this solvent is here again set at liberty*; whereas the hydrogen having no such attraction for the metal, the energies of this power *are here not at liberty, but are taken up with holding this hydrogen in solution or in the gaseous state: and hence at this end the current of gas is seen to arise, while on the other no such current is perceived, though this is the positive point,—the power* IS THERE; *but being unoccupied, it is in its pure and attenuated state, and of course invisible.* When wires are employed which are not oxydizable, then oxygen is given off at one end and hydrogen at the other; or rather they *appear* to be given off at distinct and separate ends; for wherever oxygen is separated, there must hydrogen also, each portion of water being alike composed of both: but oxygen having a greater affinity for all metals than hydrogen, and this affinity being greater in some than others, it is detained by the point where this decomposition is going on, while the hydrogen is carried to the next metal—all which is beautifully proved by the arrangement of the cups, and by many facts of galvanism, which we must leave to be explained when we come to treat of all the facts classed under the head of Galvanism, and to which all this must still be considered as preparatory. The oxygen and hydrogen are given off at these points, and so far occupy the energies of this power. Here consequently it may be said, that as the oxygen is not deposited on the metal, it does not give up this power in the way just described, and therefore cannot be positive. It is not so indeed in so high a degree;—positive and negative are mere relative states of existence. The hydrogen occupying this power more than the oxygen, they are *still* relatively to each other positive and negative, only not in so high a degree: the hydrogen requiring about thirteen times more to give it the gaseous form, than the same weight of oxygen requires, they still remain to each other positive and negative, and the hydrogen of course negative.

I am aware, as I have already stated in the former Essays, that this is not the common statement of the difference in their capacity;

capacity; but I have also already hinted, that I conceive the methods hitherto used to ascertain capacity to be fallacious, and of course that the tables in some instances are erroneous; that it is not alone the transference of heat from one body to another, or the quantity of ice which bodies will melt in cooling, which can determine it; but how far this power is separated in its pure and unconfinable form, and of course makes its escape without having time to produce any of these effects.—But of this more afterwards.

This explanation of galvanism will beautifully apply to the evolution of gas in coal pits. It is confirmed by the fact lately ascertained,—that a heated atmosphere increases the power of galvanism. It is confirmed too by a review of those circumstances which modify the actions of this power in galvanism, and render them so different in their effects and appearances to those which it produces in the form of electricity. Let us then examine these differences. Every fact connected with the discovery of galvanism, and the history of its progress, proves the explanation I have given to be the true one. They prove that positive and negative are mere relative states of existence, produced by that arrangement of conductors and of substances which, by the changes they induce, calls forth a current or stream of this power; which depends on the same principles, though varied by circumstances, as that which is produced in the form of electricity.

GALVANISM I shall therefore define "*as that object of science which treats of some of the* CHEMICAL AND NATURAL *means of* PARTIALLY *separating the* GRAND AGENT *from some of its combinations, and of ascertaining its actions in this state.*"

Electricity I have considered as the most pure and separate form of fire, or of the power which produces the phænomena of heat and flame; and consequently more attenuated than any other, more rapid in its movements, and less resistible in its passage through substances. Galvanism I consider to be the same power, only *partially* separated from its combinations, and differing widely in all these respects. Hence we perceive the solution of that most interesting question, stated, but not answered, in that valuable work the Edinburgh Encyclopædia, "How do Galvanism and Electricity differ from each other?" If we attend to the solvent, attractive and energetic properties of this power, as already pointed out, and the different methods peculiar to its production in electricity and galvanism, it is evident that they must differ from each other. In electricity we contrive by mechanical means to collect the loose and "*uncombined quantity*" from the earth and surrounding medium: and this we do in circumstances in which it has nothing to act upon, as free from moisture of any kind as possible; in fact, from every thing readily soluble in heat or in this power. I would therefore define Electricity to be "*the object of science which treats of the mechanical and natural means of separating this* GRAND AGENT *from some of its combinations, and of ascertaining its actions in this state.*" In galvanism, on the other hand, this solvent power, this electric fire, is produced in circumstances in which it has substances to act upon; substances which are most readily dissolved in it; substances, in fact, which seem to form the grand medium of communication between this POWER and PASSIVE SUBSTANCES; and which are partially dissolved in it. And hence I have defined Galvanism as the electric fire or the GRAND AGENT, "only *partially* separated from its combinations;" by which I refer principally to oxygen and to hydrogen.

With this in view, we may answer such questions as the following, which have often been stated but never answered: "Why does galvanism exist in a lower state of intensity than electricity in producing shocks?" Because its active energies are less, being in part occupied by holding other bodies in solution; from which cause it is also less attenuated, consequently less rapid in its movements, or passes through substances with greater difficulty. But "Why again is its power in producing chemical effects greater than electricity?" First, because its quantity produced in a given time is so much greater; but chiefly, because it is combined with substances which have a powerful tendency to direct and fix its actions, and which are as it were the grand medium of communication between this POWER and PASSIVE SUBSTANCES; whence has arisen the proverbial fact,—that when such substances are employed in the galvanic apparatus as least produce this decomposition and solution of oxygen and hydrogen, the electrical effects are then greatest, and the chemical effects slightest, and not perceptible at all when there is no fluid or moisture present. In this way we would explain why De Luc's column, which is excluded from air and moisture, produces no chemical effect; and why the electric machine produces so much less than that produced by the galvanic means. Hence too galvanism burns charcoal with such intense brilliancy; and yet the charcoal is scarcely consumed, because the oxygen and hydrogen held in solution produce, in part, this effect: hence a wire heated by galvanism continues so longer than when heated by electricity; and hence we perceive the explanation of a very singular fact,—that the chemical effects of galvanism are increased by increasing the surface or size of the plates to a certain extent, beyond which it ceases to have these effects. The explanation of this last circumstance I conceive is this: By increasing the size of the plates we increase the chemical

action

action in each distinct division of the pile, by which the movement or current is proportionally retarded and broken: but this retardation is of course accumulated, and hence its power to decompose and dissolve so much more oxygen and hydrogen. This quantity of oxygen and hydrogen again, to a certain extent, increases its chemical action; and at the same time, from the motion being slower, increases the chemical power of that quantity. In this manner I conceive the *partial* solution of oxygen and hydrogen assists and modifies the chemical agencies of galvanism. If, however, this retardation is too much extended, by a large galvanic apparatus and by having a considerable volume of fluid intervening between the plates, then the galvanic fire or fluid becomes saturated, or its solvent and attractive powers become occupied and suspended, with this oxygen and hydrogen held in solution. And to prove that this is the correct view, the series may be very much extended, if the volume of interposed fluid is in any way diminished. Hence also it is that the galvanic shock is greatest on a person with a dry and tense, and least on a person of a moist and lax, fibre; and that it is perceptibly milder where fear does not render appearances deceitful.

Thus we perceive that when this GRAND AGENT OF NATURE is *more perfectly* separated from its combinations, it is ELECTRICITY—when *partially* separated, GALVANISM. When no means are used to retain it in either of these states, but when in its actions it passes from one substance into another, it is CALORIC, or fire in its common acceptation. To confirm this view, every fact and experiment under their respective heads are seen to be mutually convertible into each other. If caloric abounds in an uncombined state, artificially or naturally, we easily collect it by the electric machine in its purest form;—if chemical or natural actions of this power call forth a current faster than it can dissolve the substances on which it acts, we obtain it *partially separated*, as in galvanism, &c. If the current either acts with greater intensity on decomposable and soluble substances, as in common combustion; or is accumulated in quantity, but more impeded in its progress, as in a galvanic apparatus of immense size,—we have it with substances dissolved in it, which dissolution is proved by the varied colours which are imparted to flame, and by the oxygenating and hydrogenating effects in all these, as well as in every other instance where the same causes operate, as will appear in our consideration of light.

If then electricity and galvanism depend on the same power, which pervades the universe and circulates through matter, we perceive that this explanation or theory accords with the facts and phænomena of nature. It is there evident that this POWER is more or less impeded in its passage through matter, as bodies differ in their conducting power and capacity, and according to their greater or less degree of solubility in it: and hence it follows, that all things dissimilar and in contact become relatively to each other in excess or defective—negative or positive; and of course exhibit proofs of these disturbed and deranged states. From the energetic reactions of this power, arising from these disturbances, they further either destroy each other, or assimilate into one, or give rise to new forms and existences. Thus iron nails in sheets of copper, and every arrangement of dissimilar substances, either rapidly corrode or produce some decomposition. Thus gases are produced by the strata in the earth, rocks of dissimilar composition moulder into soil, &c. It is on these principles of derangement, and on the effort of nature to effect a proper distribution, that I shall attempt in these Essays to explain all the movements and changes of the universe,—an explanation which I think we shall find confirmed, the more closely we attend to the operations and phænomena of nature and art.

The explanation now given (which as far as I know is different from every other) is, I conceive, the true one, of the different phænomena and effects produced by electric and galvanic contrivances. The Electric contrivances to be hereafter explained, I propose to call MECHANICAL, and the Galvanic CHEMICAL. In both instances the same *grand*, attractive and solvent power is called into action. In electricity, this GRAND AGENT of nature is, from its attraction for substances, disturbed in its due and relative diffusion, by motion and friction; and when these mechanical actions are made in a given direction, a current of this power is attracted and carried in the same course; the point from which it is abstracted becomes negative, and in its turn demands a new supply. It is thus that the action of the electric machine, and all the facts and experiments connected with electric science, on these principles receive a ready explanation. In the production of this power by the electric machine, as fast as that part of the machine from which the revolutions of the cylinder recede, is robbed of its natural quantity, it demands it from the earth and surrounding media; (hence the necessity of a conducting and communicating chain;) while, on the other hand, that side to which the motion proceeds, receives this current by means of metallic points fixed to the prime conductor, where by its insulation it is retained and accumulated. This power is called into action in galvanism in a different way, but still depending on the same principles; with this exception, as we have already

already seen, that here its SOLVENT as well as attractive properties are exerted and employed. In galvanism, the excitation of this power, I repeat, depends on the alternate arrangement of dissimilar metals having a fluid interposed between them, for which the one metal has a greater affinity than the other, so that chemical changes are the consequence: the fluid is decomposed, and the products assume the gaseous form; a demand is made on this grand agent, in order to dissolve and support these new forms of existence, which are thus produced*. In this way the metal in contact becomes robbed of its natural quantity, and demands a fresh supply, which is no sooner received than it is imparted to the metal having the stronger affinity for the fluid, and where these changes and gaseous results require and demand it. Thus a current is produced alternately positive and negative; but which differs from electricity not only in the retardation these actions occasion, but in having to traverse a different medium—an imperfect conducting fluid, by which the current has its velocity not only further retarded and broken, but its qualities modified.

All that has been now laid down will be further confirmed by the views we shall develop when we come to Chemical Affinity, Light, and Electricity; when I trust I shall satisfy the reader that I have not in the commencement held out more than I shall be able to establish, as I proceed in executing the task. Still, I trust every allowance will be made for any imperfections which must necessarily be connected with the novelty of the views, on so extensive and so difficult a subject as that of not merely marking effects and phænomena, but the mode of operation and nature of that power which produces them.

Colliergate, York, July 15, 1818.

[To be continued.]

XIX. *Observations relating to the Operations undertaken to determine the Figure of the Earth. By M.* BIOT, *of the French Academy of Sciences* †.

WHEN about two centuries ago Galileo, on one of the towers of Florence, explained to a few individuals, in language almost mysterious, his new discoveries respecting the laws of gravitation, the motion of the earth, and the figure of the planets; how little did he imagine that these truths, at that time so miscon-

* It has been often explained how this power in different quantities, differing in every different kind of matter, produces and sustains bodies in all their various forms, states, and stages of existence.

† From the French of M. Biot.

 ceived

ceived and so persecuted, would, after so short an interval, be deemed of such importance and so generally admired, as to induce the governments of Europe to undertake great operations and distant voyages for the sole purpose of explaining them, and of verifying all their relations; and that, by the effect of an unexpected propagation of knowledge, the results of these labours should be presented to the attention of the public in numerous assemblies composed of the most eminent classes of society! Yet such is the great change which has been effected in the fate of the sciences since that period.

When Galileo and Bacon appeared, they found the sciences only on the brink of being—for it would be inaccurate to give the name of science to that mass of useless hypothetical speculation of which all natural philosophy previously consisted. The aim of the ancients was rather to divine than to investigate natural causes. The art of examining nature in order to constrain her to reveal her secrets was unknown;—it remained for Galileo and Bacon to make this discovery. They evinced that the human mind is too feeble, and too evanescent in its efforts, to advance by its own strength through the labyrinth of natural facts; that it is necessary at every new step of its progress to rest upon and to classify those phænomena which approximate to one another; and that in the multiplied opportunities which nature offers for inquiry, experiments industriously imagined are requisite to conduct to a course of new phænomena which shall neither entangle nor mislead.

Such has been the felicity of this mode of investigation, that in less than two centuries, discoveries without number, and certain and durable, have illustrated every department of science;—the arts have rapidly participated in their beneficial effects; industry has been enriched with many wonderful applications, and a sum of knowledge has been accumulated a thousand times greater than that of which antiquity could ever boast. As the sciences, however, have thus been enlarged, they have grown beyond the reach of any single individual to attain. So large a sphere could no longer be embraced but by a numerous literary body, which in its aggregate capacity similar to one mind should unite all conceptions, views and thoughts; and which, interrupted neither by human infirmities nor the decline of reason, or age, but always young and always vigorous, should incessantly scrutinize the peculiar properties of natural objects, discover the powers concealed in them, and at last present them to society prepared and ready for application. In a central body such as this, where opinions have the freest operation, no authority can prevail, if it be not that of reason and of nature. The voice even of a Plato himself could no longer gain attention in such an an assembly to the brilliant reveries of his imagination; and the genius of a Descartes would be constrained faithfully to adhere to that mode of observation and of doubt which he himself had promulgated, and not pretend to exhibit truth unmixed with error: nor with all their glory could Plato and Descartes be now regarded as more than mere elementary branches of this great organ of the sciences:—its force would outlive their genius, and carry into futurity the gradual development of their thoughts. —Such is now the noble, destiny of learned societies. The simultaneousness and the durability which their institution gives to the efforts of mortals, complete the power of the experimental method. They alone can in future give continuity to the progress of knowledge; they alone can develop grand theories, and produce results which, by their difficulty, by their diversity, by the perseverance and the extent of exertion which they require, could never be attained by individuals.

To determine the size and figure of the earth; to measure the gravity at the surface, to ascertain its connexion with the interior construction, with the disposition of the strata, and the laws of their densities, are of the number of those important questions which learned societies alone could resolve. During a century and a half they have formed a chief object of the Academy of Sciences. The first exact measurement of a degree of the terrestrial meridian was made in France by Picard in 1670. Newton availed himself of it, in order to establish the law of universal gravity. Richer, who was sent by the Academy to Cayenne, two years after, to make astronomical researches, discovered that his clock, which at Paris beat the seconds, went gradually more slowly as he approached the equator; and that it again went quicker, by the same degrees, in returning towards the north, so as to resume exactly its original motion at the point of his departure. Again, according to the discoveries of Huygens, the quickness of the oscillations of a pendulum augments or diminishes with the intensity of the gravity which causes its motion. His observation proved that this intensity was different in different latitudes, and that it increased in going from the equator to the pole. Newton in his Principles of Natural Philosophy connected all these results with the law of attraction. He showed that the variation observed in gravity disclosed a flattening of the earth at the poles; a circumstance which is also observable in the form of Jupiter, Saturn, and the other planets which turn upon an axis. He conceived that this flattened form was a consequence of the even attraction of the portions of every planet, combined by the centrifugal force of its rotatory motion. But in order that the arrangement determined by these two kinds of forces should thus have been able to make itself effectual,

fectual, it behoved these great bodies to have been originally fluid. He took them then as in that state, and showed how to calculate the flattening of a planet, according to the intensity of the gravity at its surface and the quickness of its rotation, supposing its mass to be homogeneous. This theory, applied to the earth, gave a variation of gravity but little different from that observed by Richer, though somewhat slighter, indicating that the earth is composed of strata of which the density goes on increasing from the surface to the centre, as Clairault has since demonstrated.

For some time the calculations of Newton were the only inductions for believing the earth to be flattened at the poles. The arc of the meridian measured by Picard was sufficient to give the length of the semidiameter of the earth at the place where it was observed; but that arc was much too small even for showing imperfectly the effect of the flattening. More accurate knowledge was expected to be procured from the measurement of the complete arc which traverses France from Perpignan to Dunkirk, which was intended to serve as the axis of a general map of France, with the execution of which Colbert had intrusted the Academy. But in the imperfect state of the instruments and astronomical methods of that period, this arc itself was too short to make the influence of the flattening distinctly perceptible; and the small variations which thence result in the length of the consecutive degrees, might very easily be lost in the errors of the observations, as was indeed actually the case. The differences which the degrees presented, were found from the effects of these errors, in such a direction as would have led to the result of elongation at the poles, in place of flattening. The Academy was not intimidated. It perceived that the question could not be accurately decided without measuring two arcs of the meridian, the one near the equator and the other near the pole. In the year 1735, Bouguer, Godin, and La Condamine were sent to America, where they joined the Spanish commissioners. Clairault, Maupertuis and Le Monnier departed for the North. The results of these expeditions completely ascertained the flattening of the earth, but its absolute amount still remained uncertain. The degree of Peru compared with that of France, gave a slighter flattening than if the earth was homogeneous; the sphere of Lapland indicated a greater. In this uncertainty the lengths of the pendulum which they were careful to measure, agreed with the flattening deduced from the operation at the equator; but the exactness of these measurements, especially in the sphere of Lapland, was not such as could enable them to solve the difficulty.

Matters remained in this state for fifty years. Meanwhile Bouguer,

guer, La Condamine, Clairault, and Maupertuis died; and it was only when astronomical instruments became more perfect, that the fact of the flattening at the poles could be accurately ascertained. The Academy gave still more importance to these researches by proposing to take the measurement of the earth as the fundamental element of a system of general and uniform measures, of which all the parts would be connected by simple relations, and in accordance with our mode of numeration. The Academy hopes that such a system, founded upon natural elements, invariable and independent of the prejudices of the people, will ultimately become as common to all, as are now the Arabian ciphers, the division of time, and the calendar. This wish was long ago expressed by the best and the most enlightened of our kings. The proposal for carrying it into effect was one of the last sighs of the Academy, and the act which decided its execution was one of the last which preceded the fatal epoch of our great political convulsions. All the institutions tending to maintain civilization and knowledge perished, and the Academy perished with them. But men of science prosecute without authority what they esteem useful. In the midst of the disorders of popular anarchy, MM. Delambre and Mechain, furnished with the new instruments of Borda, commenced and prosecuted, often at the risk of their lives, the most extensive and exact measurement of the earth. They concluded it with the same perfection, though not with the same ease, as if it had been in times of the most profound tranquillity. Nor was the measurement of the pendulum neglected. Borda, who had so far advanced every other mode of observation, invented for this experiment a method, the exactness of which surpassed every thing previously known, and which has never been surpassed.

After these operations, it was thought that the arc might be continued many degrees south across Catalonia, and that it might be possible to prolong it to the Balearic isles by means of an immense triangle, of which the sides extending over the sea should join these islands to the coast of Valentia. Mechain devoted himself to this operation: but after having surveyed all the chain and measured the first triangles, he died of a fever in Valentia. M. Arago and myself were intrusted with the completion of the work, along with the commissioners of the king of Spain. We had the good fortune to succeed; but, as is well known, M. Arago did not return to France without encountering great danger, and after a distressing captivity. Our results confirmed those of the arc of France, and gave them a new proof of accuracy. After the method of Borda, we also measured at our remote station the lengths of the seconds pendulum. M. Matthieu and myself repeated

repeated the same operations upon different points of the arc comprised between Perpignan and Dunkirk. These experiments gave for the flattening of the earth a value almost equal to that which M. Delambre had already obtained, by comparing the arc of France and Spain with the degrees of the equator, but calculated with more accuracy, and corrected by the degree of Lapland, which Mr. Svanberg, an able Swedish astronomer, had certified by new observations; and finally, with an arc of many degrees which Major Lambton had measured with great accuracy in the British possessions of India.

Confirmed by so many combinations, our arc of France and Spain had a good title to become a fundamental model for measures. An occasion occurred of rendering it still more important. Since the rebellion of 1745, the English government had perceived the utility of constructing a detailed map of the three kingdoms, which should equally serve to direct the amelioration of the country in peace and its defence in war. I may state, in passing, that it is the war for twenty years back which has given to geodesiacal operations the great extension and the extreme perfection which they have acquired in all the states of Europe. However this may be, the English *triangulation* begun by General Roy, and continued after him by Colonel Mudge, was prolonged from the south of England to the north of Scotland, and presented in that extent many degrees of the terrestrial meridian measured with excellent instruments. It was desirous that this arc should be joined to that of France. But as from the geographical position of England she is placed a little to the westward of ours, there was reason to fear, lest, all the terrestrial meridians not being exactly alike, the difference of longitude would affect the results which might be obtained from that junction. Nevertheless, there could be no dread of this, so far as concerned the measurements of the pendulum, which are much less disturbed than the degrees by the slight irregularities of the figure of the earth. The Board of Longitude was desirous that the same apparatus which had served for these measurements in France and Spain, should be employed through the whole extent of the English arc. The consent of government and the approbation of men of science in England were necessary. Neither the one nor the other was wanting. The respectable Sir Joseph Banks and his worthy friend Sir Charles Blagden assured us of all imaginable facilities. M. Lainé the minister of the interior, with whom every thing useful or honourable has only possibility for its limits, was able to furnish means for this enterprise, and the Board of Longitude had the goodness to intrust me with its execution.

I left

I left Paris in the beginning of May last year, carrying with me the apparatus made use of in other points of the meridian: a repeating circle by M. Fortin, an astronomical clock and chronometers by M. Breguet;—in fine, every thing necessary for the observations. Orders from the English government, obtained through the vigilant intervention of Sir Joseph Banks, awaited our arrival at Dover. The whole was sent to me quite entire, under the seal of the Customs, without fees, without inspection, as if I had not passed from one country into another. Every thing was protected with the same care in the carriage to London, and was at last deposited in the house of Sir Joseph Banks. How can I describe what I felt for the first time on seeing the venerable companion of Cook, rendered illustrious by his long voyages, remarkable for a stretch of mind and an elevation of feeling which interest him in the progress of every species of human knowledge! Possessing high rank, an independent fortune, and universal respect, Sir Joseph has rendered all these advantages the patrimony of the learned of all nations. So simple, so easy in his kindness, it almost seems as if he felt the obligation were on his part; and at the same time he is so good that he leaves us all the pleasure of gratitude. What a noble example of a protection, whose sole authority is founded in esteem, respect, free and voluntary confidence; whose titles consist only in an inexhaustible good will, and in the recollection of services rendered; and the long and uncontested possession of which necessarily supposes rare virtues and an exquisite delicacy, more especially when we recollect that all this power is formed, maintained and exercised among equals!

Favoured by these honourable auspices, every thing became easy. Colonel Mudge, who had shown himself most favourably disposed towards our enterprise, seconded it by every means in his power. We left Edinburgh together, and fixed our first station in the Fort of Leith, where Colonel Elphinston, the commandant, afforded us all the accommodation in his power. I required a situation where the view was open, and also sheltered, to erect my circle. I constructed a portable observatory which could be taken down at pleasure, so as to allow me to make observations on all the sides of the horizon. It was necessary, however, that the apparatus of the pendulum should be fixed with solidity; and for this purpose stones of the weight of sixty quintals were fixed in thick walls with iron chains. Every thing that could be useful was lavished upon me, and if my observations were incorrect it was my own fault. Unfortunately the health of Colonel Mudge did not permit him to accompany me; but his place was supplied by one of his sons, Captain Richard Mudge, with whom I completed my labours. After they were finished

finished at the Fort it was necessary to go and repeat them in the Orkneys, the uttermost limit of the English arc. But Col. Mudge perceived that it was possible to connect the Orkneys with the Shetland isles, by triangles whose *apices* should rest upon the isles, or rather upon the intermediate rocks, of Faro and Foula. This plan extended the new arc two degrees to the north, and this was sufficient to decide the matter. The arrangement had still another advantage, of very different importance, which consisted in carrying the English line of operation two degrees towards the east, almost upon the meridian of Formentera. By this fortunate change the English operation became the prolongation of ours, and the two together form an arc almost equal to the fourth part of the distance from the pole to the equator. If one might hope that the different nations of Europe would agree to choose the base of a common system of measures in nature, is there not here an element the most beautiful and the most certain that could be adopted? And this arc, which leaving the Balearic isles, traverses Spain, France, England, Scotland, and stops at the rocks of the ancient Thule, being taken in combination with the flattening of the earth, which is deduced from the measurement of the pendulum, or from the theory of the moon,—will it not give for fundamental unity, a measure the most complete, and I dare say the most European that can ever be expected?

When the possibility of this great project was conceived, it absorbed all our thoughts: but the delicate health of Colonel Mudge did not permit him to realize these hopes in person, and he intrusted the execution to one of his officers. He gave me his son, whose assistance had been of such service, and which might still be of much more. My apparatus, the portable observatory, the large stones and the iron chains were all embarked, with the instruments of the English operation, in the Investigator brig-of-war, commanded by Captain George Thomas, whose activity and skill do not certainly stand in need of any praise of mine, but whose politeness demands all my gratitude. This officer was so good as to take me on board his ship to Aberdeen, where, during a short stay, I experienced the most distinguished hospitality. On the 9th of July we sailed for Shetland. We were long at sea, and bitterly regretted the loss of so many fine nights for observations. Leaving the Orkney mountains upon our left, on the sixth day we discovered the Isle of Faro, which saw the vessel of the Invincible Armada broken to pieces upon her rocks. The peaks of Shetland appeared, and on the 18th of July we landed near the southern point of the isles, where the Atlantic billows uniting with those which come from the sea of Norway, cause a continual swell and a perpetual tempest.

The

The desolate aspect of the island corresponded with the soil and climate. It was no longer those fortunate isles of Spain, those smiling countries, Valentia, that garden where the orange and the lemon trees in flower shed their perfumes around the tomb of a Scipio, or over the majestic ruins of the ancient Saguntum. Here, on landing upon the rocks fissured by the waves, the eye sees nothing but a soil wet, desert, and covered with stones and moss, and cragged mountains scarred by the inclemency of the heavens; not a tree nor shrub nor bush to soften the savage aspect: here and there some scattered huts, whose roofs covered with thatch allowed the thick smoke with which they are filled to escape into the fog. Reflecting on the sadness of this abode, where we were about to remain in exile during many months, we took a direction across pathless plains and hills towards the small assemblage of small stone houses forming the capital called Lerwick. There we felt that the social virtues of a country were not to be estimated by the appearance of poverty or riches. It is impossible to conceive hospitality more free, more cordial, than that with which we were received. People who but a little before were ignorant of our names, were eager to conduct us every where. Informed of the design of our visit, they collected and communicated every sort of useful information. In particular Dr. Edmonston, a well-informed physician, who has published a Description of the Shetland Islands, gave us a letter to his brother, who resides in the isle of Unst, which afforded us a station about half a degree north of Lerwick, where we resolved to make our experiments. But arrived at Unst, we were constrained, from the local situation of the island, to transfer ourselves to a small island called Balla, at the entrance of the principal bay of Unst, where we disembarked our instruments. But upon a more close examination of this station, its exposure to the winds, the moisture which prevailed in abundance, the remoteness from every habitation, and the many difficulties which presented themselves to an establishment suited to the pendulum—made us resolve to return to Unst, and to ask a reception in the only house which was in sight, which happened very luckily to be that of Dr. Edmonston's brother. A large sheepfold (empty because it was now summer) whose walls were capable of resisting every storm, received the apparatus of the pendulum. The portable observatory together with the repeating circle were placed in the garden. With great labour we dragged the stones to the place of their destination. It required all the efforts of the brig's crew, guided and animated by their officers. On the 2d of August we commenced our observations, and on the 10th made the first experiment with the pendulum. On the 17th we had made eight of these experiments, and 270 observations

observations of the latitude. I was now certain of success, and perseverance was only necessary. It was no small disadvantage that Captain Mudge became greatly indisposed; and a whaler having touched at the island, I with difficulty prevailed upon him to return to a more genial climate. He invested me with all the powers of his father, and afforded all necessary assistance. When left alone, I found the advantage of residing with Mr. Edmonston. His kindness increased with my difficulties. The operation of the repeating circle required two persons, one to follow the star and another to mark the indications of the level. A young carpenter, who by his fitting up the observatory had given proof of his intelligence, (and who, similar to the generality of the peasants in Scotland, could read, write, and cipher,) was by the advice of Mr. Edmonston employed for the latter part of the observation. He acquitted himself better than a more learned assistant; for he observed and marked my level with the fidelity and the accuracy of a mechanic, and even to satisfy my impatience would not admit my results until the bubble of the level was in a state of perfect immobility. With this assistant, in two months I collected 38 series of the pendulum, each of five or six hours, 1400 observations of the latitude in 55 series made equally on the south and the north of the zenith, and, to regulate my clock, about 1200 observations of the absolute heights of the sun and stars. After this I was chiefly employed in observing, and only made three or four calculations; but the remainder since my return home I have found accurate. The results which are deduced from them, being combined with those of Formentera of the arc of France, give for the flattening of the earth exactly the same value which is deduced from the theory of the moon, and the measurement of the degrees compared at great distances. This perfect agreement between determinations so different, shows at once the certainty of the result, and the sure method which science employs to obtain it. Nor was this point of precision reached without great difficulty, as is obvious from the fact—that the variation of the length of the pendulum by which the flattening is measured, is in all, from the equator to the pole, but four "millimetres," less than two lines, and from Formentera to Unst, one "millimetre and a half," or less than three-fourths of a line. This small portion, however, exhibits and measures even with great accuracy the flattening of the whole terrestrial spheroid, and proves that in spite of slight accidents of composition and arrangement, which this exterior and slender surface presents, the interior of the mass is composed of strata perfectly regular, and subjected to the laws of superposition, density and form, which a primitive state of fluidity had assigned to them.

But

But however great the pleasure of having completed my operations, if I had immediately departed from the rocks of Balta to my own country, my sentiments would have been specifically different concerning these isles. The dreariness of their situation, the poverty of their soil, and the inclemency of their sky, would have accompanied me, and I should have remained ignorant that they contain sensible, kind, virtuous, and enlightened inhabitants. Nor should I have been able to discover the charm which these pathless barren regions—the region of rain, of tempest, and of sterility—have to reconcile them to such hardships.

Peace and not plenty constitutes this charm. The sound of a drum has not been heard in Unst for twenty-five years, while Europe was wasting her best blood; and during all that period the door of the house where I resided had not been shut day or night. Neither conscription nor press-gang had afflicted the inhabitants of these peaceful isles. Their rough seas protect them from the incursions of privateers, and their poverty is still a stronger defence. These people receive the intelligence of the transactions of the continent, as they would read the history of other times. Their calm and contracted situation gives to their mode of life a charm unknown in other climes. They live in one great family. But the strength of affection produces the extreme of grief upon death or separation. When death enters the dwellings of those whose affections are so concentrated, it comes in all its bitterness. Nor are the grief and sorrow much less when a son, or a brother, or a friend takes his journey to another country, for seldom does their own little isle contain the children with the fathers. A small portion around their huts is all the soil that is cultivated; and horses and sheep almost in a wild state pasture the remainder. A principal part of their wealth and support is procured from the tremendous waves and billows of the ocean, which with unexampled boldness they combat in quest of fish. When the weather is good the toil becomes a pleasure; but when the sea becomes tempestuous, the struggle in their uncovered boats is violent. Under their guidance I have found myself calm when contemplating those lofty cliffs of primitive rocks—that ancient structure of the globe, whose strata lie inclined towards the sea, and, undermined at their base by the fury of the waves, seem threatening to bury under their ruins the frail bark which bounds at their feet.

Carrying with me the most agreeable recollections, I took leave of these isles after a residence of two months. An equinoctial gale conveyed us in fifty hours to Edinburgh. Returned to Colonel Elphinston, I experienced that hospitality had not retired to the Shetland isles. Having finished my particular labours,

labours, an opportunity was afforded to consider the situation of the country; the character, the manners, the institutions, and the pursuits of the inhabitants. The review was both consoling and sorrowful, to one whose days had been spent amid wars and commotions. Here dwelt a people poor, but laborious; free, but submissive to the laws; moral and religious, without sternness or indifference. The peasants were seen reading the Essays of Addison, Pope, Johnson, Chesterfield, and the most approved of the English moralists. Even in the passage-boats cards and dice were exhibited. Village farmers were seen in clubs discussing the topics of politics and of agriculture; and these formed into societies to purchase the most entertaining and instructive works, the *Encyclopædia Britannica* not excepted. I saw, in fine, the higher classes adorning in an eminent degree their stations, by exciting and directing all the enterprises of public utility, mingling with the vulgar, but preserving a noble superiority, procuring respect without exciting envy, and enjoying as the reward of their exertions, peace, union, reciprocal esteem, mutual confidence, and a lively affection.

I next visited the most industrious counties of industrious England. I saw there the powers of nature employed in the service of man under every supposable shape, and man himself reserved for those operations which mind alone can direct and perform; but I rather admired that immense display of manufactures, than wished to see them established in my own country. After visiting Oxford and Cambridge, those ancient abodes of learning, I went to rejoin M. Arago in London, to measure the seconds of the pendulum no longer in a desert, but in the magnificent Observatory at Greenwich. M. Humboldt attended him, assisted in the operation, and meanwhile seemed to forget the multitude of his other talents in his labours as an excellent astronomer. The Astronomer Royal manifested that generous ardour which men devoted to the progress of science, alone can feel.

After such success, and brought under so many pleasing obligations, I returned to my native country. The pleasure of observing the heavens, of studying one of the greatest phænomena of nature with fine instruments, by so many observations, and in a place renowned for so many astronomical discoveries, enabled me to confer a lasting tribute of gratitude upon the place of my birth. In a voyage undertaken for the advancement of science, the stranger learns what to honour and what to cherish. Without the circle of political passions, without rank, without ambition, his principal aim is to do good to mankind. He is ennobled by the numerous services which he has rendered to the civilization of the world, by the universal admiration which he has excited, and by those intellectual stores with which he has enriched

enriched the arts and sciences. Similar to Minerva, that country accompanies him into a foreign land; she speaks for him, introduces him, protects him, and claims in his favour an hospitality which she has often nobly conferred. Having reached the end of his labours, and while relating to his countrymen the reception, the assistance, the kindness, the friendship received from a celebrated nation, he experiences in the expression of his gratitude a pleasure so much the more pure, that he feels sensible that all these favours were less conferred on himself, than through him on his country.

XX. *On the Means of curing the Dry-Rot.* *By* A Correspondent.

To Mr. Tilloch.

Sir, — Observing in the public papers and periodical works the havoc the dry-rot has made in our shipping and public and private buildings, and having, I presume, found out substances for the preservation of wood from dry-rot; I take the liberty of stating the composition and mode of applying them.

First. Make a strong caustic solution in water of barilla, kelp, or potash, and when boiling hot, wash the parts of the wood affected with the rot. The effect of this caustic ley will be the destruction of the vegetating fibres of the fungus.

Secondly. Dissolve oxide of lead or iron in pyrolignous acid; and twelve hours after the first application of the leys soak the wood well with this solution. A decomposition of the metallic liquor takes place; the acid and alkali unite, and the oxide of the lead or iron is precipitated in the pores of the wood, and, prevents the fungus from spreading.

Another way of preventing the rot is: first, to wash the wood with the pyrolignous solution of lead, and ten or twelve hours after to wash it with a strong solution of alum (in the proportion of one pound and a half of alum to one gallon of water).

Since writing the above, I have seen in your Philosophical Magazine an Essay by Mr. Gavin Inglis, recommending sulphate of iron to prevent the dry-rot. I think you will find the iron liquor in my process preferable, as the alkaline solution precipitates the oxide of the metal in the pores of the wood. I have paling now in good preservation, that was put up fourteen years ago, with staves of old iron liquor pipes and puncheons, and never painted. The wood is hard, and can scarcely be cut with a knife; the liquor has penetrated into the pores of the wood, and contracted or filled them up.

Mr. Inglis recommends that all kind of timber should be cut after the fall of the leaf, &c. This every judicious man will approve of; but the loss of the bark will be a great obstacle to its being put in practice. If the trees were peeled at the proper season for such work, and cut down in the fall of the year, or left standing two years to season, the loss of bark would be prevented, and the timber well seasoned for ship-building or other purposes.

Chester, July 24, 1818. I. I.

XXI. *Notices respecting New Books.*

Mémoires sur la Marine et les Ponts et Chaussées de France et d'Angleterre; contenant deux Relations de Voyages faits par l'Auteur dans les Ports d'Angleterre, d'Ecosse et d'Irlande, en 1816, 1817 *et* 1818; *la Description de la Jettée de Plymouth, et du Canal Calédonien, etc.*—"Memoirs on the Maritime Works and Civil Engineering of France and England, by M. Ch. Dupin, Engineer of the French Navy."

[For the following interesting notice of this work of M. Dupin, we are indebted to the pen of another able French engineer, M. Bosquillon de Jenlie.]

During the fourth part of a century, war, and still more a suspicious policy, had kept France in total ignorance respecting the internal condition of Great Britain. Thus far, at least, the pretended blockade of the United Kingdoms had been realized. But in the very time (especially from 1812 to 1814) that it was attempted by documents and accounts, *ex officio*, to represent England as in the last stage of exhaustion, the nation was, in truth, rising to an unparalleled state of splendour and wealth. This wonderful effect was more particularly exemplified in the sea-port towns; some of which, as Liverpool, were doubling in a few years a population of 50,000 souls. But not only in such towns as were eminently aided by their local position, but every where in Great Britain immense establishments and magnificent constructions displayed the national wealth and the improvement of the arts. One might there observe stupendous maritime works, superior to all the constructions which a government with the disposal of all the treasures of Europe had raised on the banks of the Seine, achieved in three years on the banks of the Thames by a single society of merchants.

Since peace has reestablished an intercourse between two nations worthy of contending in other arts than war, various French travellers have presented their countrymen with pictures of the manners of Great Britain, with descriptions more or less witty,

witty, but often tainted by illiberal reflections, the consequences of long feuds. It was quite a new, and by far more interesting point of view, to consider all the changes wrought, the wonders performed during the course of twenty-five years, by the efforts of industry, the improvement of the arts, the concurrence and activity of a whole nation animated by the same spirit. Such is the true aspect under which the author of the book before us has viewed Great Britain in his journeys during the years 1816, 1817 and 1818. In this extensive display of the results of English industry, the author was of course obliged to limit his views; and he particularly confined himself to exhibiting the industry of the nation in the applications to three branches of public service; viz. hydraulic works, and military and naval constructions.

In the field though thus limited there was still a rich harvest to gather; but the task was not without its difficulties. It required a varied knowledge in the arts of construction and other public services; an eagerness and perseverance that should overcome all the obstacles which, in a foreign country, incessantly impede the traveller; a great talent of observation; and finally, such introductions and connexions as might shield him from the consequences of national jealousy. Such were the qualifications requisite to bring the undertaking to a successful completion: and the book before us shows that in none of them has the author been deficient. Bred in a school* out of which no sound mind can come without being imbued with valuable information respecting the various public services; a fellow of several learned societies, which he has enriched with his memoirs; and favoured by a happy combination of circumstances,—the laboratory of the artist, the port-folio of the engineer, and the closet of the learned, have all laid open their treasures to his inspection. Such advantages lead us to place much confidence upon the results which he has offered to the learned world.

The memoirs now published comprehend but a succinct account, divested of all scientific and abstracted particulars, of one of the three subjects of his journey; that which treats of the public works in England. The one which relates to artillery and military engineering has been appreciated in a way highly honourable for the author, in a Report made to the Academy of Sciences, by a judge whose opinion in all that regards military science must always command respect—the Marshal Duke de Ragusa.

In the memoirs which are a compendium of both his journeys in Great Britain, the author first treats of that city which the national partiality has distinguished (as Rome anciently was) by the emphatic appellation of *the town*. He takes a view of Lon-

* The Polytechnick school.

don under three different aspects:—as the largest trading port of England; as the chief focus of the industry in the mechanical arts; lastly, as the centre of the operations of the navy. After having described the extensive bed of the Thames covered with innumerable ships, which scarcely leave room for sailing; after having described those numerous and magnificent basins for trade, newly constructed and distinguished by the name of *docks*,—the author enters into some interesting particulars about systems of construction which essentially differ from ours, the internal sections and external figure of their wharfs, as well as of their large sluice-gates, their cast-iron swing bridges; also respecting the use of the iron rail-roads for all sorts of conveyances by means of hand or horse-carts. He describes the process of dredging, which is advantageously employed in cleansing docks and deepening rivers. It consists in boats or vessels equipped with buckets put in motion by the machine generally used in Great Britain, and become in that country, for the mechanical arts, what the plough is for husbandry—the *steam-engine*.

Another not less ingenious and remarkable process is that of the diving-bell, which enables the workmen to work as on dry land at a great depth under water. This apparatus, in which nothing conducive to the safety and accommodation of the workman has been omitted, is used with the utmost success by the engineer who superintends the most part of the maritime works in England, and the building of the finest bridges in London, Mr. Rennie, whose name is so often mentioned in these Memoirs. From him, as well as Mr. Telford*, the author has particularly received much valuable information, and the most kind reception.

After describing a great and curious shed 1300 feet long, entirely of iron, from the pillars that prop it, to the very roof, and built by the same gentleman, the author shows us several engines not less eminent for ingenuity than for their extensive application. One feels a satisfaction mixed with regret in remarking, in a foreign country, as the inventor of many ingenious machines, the name of a Frenchman, M. Brunel.

As appendages to London, looked upon as the centre of the great operations of the navy, one may consider the fine docks and establishments of Deptford, Greenwich, Woolwich, which the traveller describes, in going down the river to Sheerness; a port created anew, the works of which give occasion to some most interesting remarks. One of them deserves a parti-

* M. Dupin in his Memoirs pays every where with the greatest pleasure his debt of gratitude for the benevolence and liberality of these two gentlemen, as well as of their friends and brother-engineers MM. Nimmo, Jardine, &c. &c.

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cular notice, as offering one of the finest conquests of art over nature.

This military port, founded on a marshy island at the confluence of the Channel and Medway, was, notwithstanding all the advantages of its position, deprived of one of the chief requisites in a naval establishment,—it had no sweet water, and it was necessary to carry it at a great expense from a neighbouring port. The bold idea was conceived of seeking for some spring far below the bottom of the channel and sea. They were enabled by art to dig and sink to a depth of 350 feet: there they found a spring of sweet water, which spouting with impetuosity filled up the well to within two yards of the top, and then sunk again to forty. Ever since it has afforded a plentiful supply of good water.

We cannot follow the author through all the places which he has successively visited in both his journeys, and which comprehend nearly all the ports of the United Kingdoms. From the most powerful recommendations he got admittance into both the great arsenals of the English navy, Portsmouth and Plymouth. He also visited Bristol and Liverpool, the two chief trading ports next to London; Birmingham, noted for its beautiful manufactures; Newcastle, justly famed for extensive and valuable coal-pits; Sunderland, distinguished by her magnificent iron bridge, under which ships of 4 to 500 tons are daily sailing; Edinburgh, become by the culture of the sciences, the Athens of the north; Glasgow, Dublin, &c.—all of which places are by turns the subject of the most valuable descriptions and interesting remarks. Everywhere in the inland country, as well as in the sea-port towns, new constructions and numerous establishments evince a recent prosperity, and the greatest improvements in all the arts. On observing these, we are naturally led to inquire into the cause which has produced all these wonders. It is the same which in 1792 gave to France such a superiority in the arts of war—necessity. Great Britain, in her turn, attacked by the whole continent, could only oppose the efforts of her trade and industry; and in this struggle, which appeared so unequal, the friend of the arts forgets all national rivalry, to attend only to operations and works which attest the power of the human mind, the benefit of equitable laws, and the energy of national character.

To the compendium of both his journeys the author has subjoined two memoirs, intended to describe two magnificent works which are now in execution in Great Britain—the Caledonian canal, and the jetty of Plymouth.

The former, which has been planned by Mr. Telford, a very skilful engineer, is intended to open, through a very singular valley in the Highlands of Scotland, a communication between the North and Atlantic seas by a canal, which from its large scale, and the extensive lakes through which it passes, should rather be looked upon as an artificial arm of the sea, on which ships of 4 and 500 tons and 20 feet draught of water can sail.

The other work,—the jetty at Plymouth,—reminds us of the grand works at Cherbourg. The bold conception achieved at Cherbourg, of founding in the open sea a huge mole, an artificial island, intended to secure a space of water, forming a road, against the winds and waves, has been likewise applied to Plymouth. But the English had not, like us, large and expensive experiments to try, in order to construct that mole which they call by a term denoting its destination, *Break-water*. The mole, erected at three miles from the bottom of the road (the Sound), stretches to an extent of 4200 feet in a straight chief line, terminated by two short ones slightly directed inwards, between which and the shore there are two passes, the one westerly, the other easterly. It is built in the way termed by us *à pierres perdues*, with enormous blocks of stone, of more than 20,000 pounds weight, which form the nucleus. The hollow and uneven parts of this enormous heap are filled up by smaller blocks let down according to fixed lines; but confusedly, and as it were given up to the water and waves to be enchased and sloped. This huge wall rises, or rather sinks, to a depth of 57 feet, and is 300 feet wide at the basis and 30 at the top, which is raised 3 feet only above the level of spring-tides. This stupendous work, planned and directed by Mr. Rennie, has been going on these five years; it will require as many years more to be finished, and an expense of 1,000,000*l.* sterling. The particular description of all the means used for the extracting, conveying, and launching of the enormous blocks of stone, is executed by M. Dupin with the utmost care, and makes in some degree the reader present at the execution of this great work, which reminds us of the ancient and celebrated monuments known by the appellation of *Cyclopean constructions*.

It is chiefly in considering (as the author has done in both these descriptions) a great work as a whole, and then in all its details, that we are struck with the perfection which the English have been enabled to give to most of their machines, and to the application of inventions which often produced in French soil, could not thrive there. To this the essays of every kind which the extension of English industry and their numerous establishments allowed them to multiply, must undoubtedly have contributed. But there is still another cause to be looked for in the difference of national characters: An Englishman is satisfied if he has added any improvement, how little soever, to a machine, to an invention; without aspiring to make it his own, by changes

changes which alter it. But French vivacity, or another too common disposition needless to be insisted on here, suits better with another course.

To the extracts of his journeys in England M. Dupin has joined several memoirs which have a natural connexion—that of improvement of the arts in public works. We may chiefly remark a description of the machines for the use of the navy, executed at Rochefort upon the plannings of Mr. Hubert; an account of the experiments on the strength of timber, made by the author, the results of which he had the satisfaction of seeing confirmed in England; lastly, some valuable memoirs on the application of geometry to the stability of floating bodies, to the tracing of roads, the lowering of summits and filling of hollows, &c.—which memoirs have been approved by the Royal Institute of France.

Such a collection of memoirs, by a skilful engineer and a distinguished writer, intended as the description of the ports and great hydraulic works in a country where the extension of industry, the improvement of the arts, the immensity of capitals, have enabled the inhabitants to accomplish the most magnificent undertakings, cannot fail to excite, in the highest degree, the attention of every class of readers. The interest of the subject will possibly cause them to regret sometimes not to find in a rapid narration more detailed particulars. It is not the lot of every work to be reproached for its brevity: besides, the reader must remember that these Memoirs are only the introduction to the complete relation of his journey in England, which the author means for a future publication. What he has already imparted to us about it, ought to give the most favourable idea of the manner in which he has considered and treated such a valuable subject, and make us wish that he may soon publish his principal work.

M. Dupin's Memoirs are dedicated to a learned engineer, celebrated for the great and useful applications which he has made of theory to the works of his art—the celebrated M. Prony.

Du Grisoux, et des Moyens de preserver les Mines de Houille de son Inflammation. "Of Fire-damp, and the Means of preserving Coal-mines from its Explosion." Mons, 1818. 8vo. pp. 26.

The work before us is a brief compendium of information respecting the safety-lamp, which has been published under the direction of the Chamber of Commerce of Mons, for the use of the proprietors and workmen of the collieries of the province of Hainault, well known as one of the richest coal districts in Europe. It consists of a succinct account of the course of observation which led Sir Humphry Davy to his immortal discovery of the safety-lamp, and a series of directions for its practical use; illustrated

illustrated by notes from the pen of M. Gossart, the president of the Chamber; under whose superintendence a variety of experiments were made, for the purpose of verifying the admirable properties of the lamp. The body of the work cannot of course be expected to contain any thing on the subject which can be new to the English reader; but we subjoin from the notes of M. Gossart some observations which possess a considerable share of interest.

Alluding to the properties of a metallic tissue in intercepting heat and flame, M. Gossart observes:

"I have repeated the experiments on this subject with a metallic tissue, whose apertures were 1-40th of an inch in size; and the results have uniformily verified what has been published of its surprising effects. I got a mask made for myself of this gauze, and put my face close to a well-lighted coal fire, without feeling any other sensation than that of a slight heat. I next held my head over the flame of spirit of wine, and of sulphuric ether, so near that the flame and gauze were in contact—without any greater effect than in the first instance; but I observed that the heat of the flame was felt more sensibly at its summit than in the centre*.

"I afterwards placed the same cloth with some gunpowder, some vegetable tinder (*pollen de lycopode*), and some cotton moistened with sulphuric ether, above a candle; and the following were the results:

"1. The powder being presented ten or twelve times in succession to the centre of the flame, and of a current of carburetted hydrogen gas, was kept there each time for seven or eight seconds, and only withdrawn when the metallic wires began to exhibit a reddish appearance. It was found necessary, in order to inflammation taking place, that the metallic wires should attain a redness approaching to incandescence, which at the centre of the flame requires about ten seconds, but at the summit only three.

"2. The vegetable tinder, exposed to a similar trial, burned the shavings of wood when the gauze became red.

* Three persons who were present with me at this experiment, also repeated it, with the same effects. A mask made of a metallic gauze with apertures of from 1-60th or 1-64th of an inch would be extremely useful to glass-blowers, to metal-founders, &c. and especially to such as are engaged in the extinction of fires. It would be necessary, in the case of the latter, that the metallic gauze should envelop the head at the distance of an inch, or an inch and an half, and that it should never be allowed to redden. It would be well, besides, that they moistened their clothes with a solution of alum; and also that a quantity of this solution should be kept in the troughs of all fire-engines, that it might accompany them wherever they went. Thus provided, the firemen could more easily snatch the burning brands, cut off the communication of the fire, and save those in danger, by wrapping them up in a covering of linen dipped in this solution of alum, &c. &c.

"The

"The cotton moistened with sulphuric ether (the most inflammable liquid known), subjected to the like experiment, was not ignited: but as the ether oozed a little through the gauze, both in a liquid state and in the state of vapour, it became feebly inflamed at the under part of the cloth: the cotton, however, did not take fire till the gauze was quite red, which did not take place till after the lapse of some minutes, and often after the evaporation of the ether.

"I have exposed successively a wire gauze of iron, a gauze of brass, a piece of tinned iron pierced with holes of the size of $\frac{1}{60}$th of an inch, and an iron plate pierced with similar holes, between a lighted candle and a current of gas issuing from the cock of a reservoir (gasometer) full of carburetted hydrogen gas: the gas was soon inflamed by the light of the candle, on the other side of these diaphragms, without any communication of the flame taking place between the cock and the diaphragms of iron wire-gauze and pierced iron-plate; but after a certain time, the gauze of copper and the pierced white-iron could no longer prevent the communication of the flame between the two sides, and that because the heat was become sufficient to burn the zinc *, which is united to the red copper in the brass, and the tin which covers the iron in the white-iron-plate.

"The gauze of iron-wire placed between a current of inflamed carburetted hydrogen gas, and a current of the same gas not inflamed, both directed to the same point, has never permitted the communication of the inflammation to the cold gas, however long the experiment may have been continued. This experiment is the same as that which takes place when the safety-lamp is set in the midst of an atmosphere of detonating mixture; with this slight difference, that in the latter, every current of gas presses much stronger upon the gauze than when the gas is attracted into the interior of the lamp to replace that which has been absorbed by the combustion.

"I have only reported these experiments in order to demonstrate that it is indispensable that the gauze be of iron-wire †, and

* "The zinc at the moment of being inflamed became volatilized to the state of white oxide. The same thing happened at the sitting of the Chamber of Commerce of Mons, on the 28th of January 1818, in presence of a number of coal-proprietors. A current of inflammable gas, distilled from coal, having been directed for a long time upon one point of a safety-lamp, the gauze of which was of brass wire, it heated that part of the gauze to such a degree, that the zinc took fire, and communicated the inflammation to the gas between the lamp and the orifice of the cock of the vessel which contained the gas—an inconvenience which there is no fear of encountering when the gauze is of iron wire.

† *Pure* copper wire will answer equally well, as it is only the presence of zinc in brass wire which renders this improper.—T.

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of a close tissue, in order to have the safety-lamp of Davy in its full perfection."

"All my views," adds M. Gossart in concluding, "and those of the Chamber over which I have the honour to preside, in collecting and publishing these facts, are to make the proprietors of mines which are affected with fire-damp, fully sensible of the importance of the discovery of the celebrated Davy, and how much humanity owes to his genius. We would earnestly press upon them to permit no other means of lighting to be employed in the working of their mines, but the safety-lamp. They will find the use of it œconomical. It will enable them to recover, and work anew, veins of coal which the abundance of inflammable gas may have forced them to abandon. It will, above all, enable them to preserve their works from those calamities which explosion from fire-damp has till now been constantly occasioning—calamities which have too often been the ruin of many of them; and to save from the torments of burning, and all the infirmities following in its train, a multitude of workmen on whose labours the subsistence of a great number of families depends.

"Happy shall we be, if we can only succeed in making our countrymen as strongly impressed as we are ourselves with all the advantages of this inestimable discovery!"

A practical Treatise on the Use and Application of Chemical Tests; with concise Directions for analysing Metallic Ores, Metals, Soils, Manures, and Mineral Waters. Illustrated by Experiments. By FREDRICK ACCUM, Operative Chemist, Lecturer on Practical Chemistry and on Mineralogy, F.L.S. M.R. A.S. R.S. of Berlin, &c. 3d Edition, 8vo. pp. 606.

We are much gratified to find that the success of this valuable little work has been so great, as already to give us an opportunity of noticing a third edition of it; and to recognise in the many elaborate improvements by which it is successively distinguished, a pleasing proof that the author is not insensible of the due return which he owes for the high share of favour which his labours have received from the public. Mr. Accum has in the present edition greatly enlarged the scale of his experiments, which are not confined to the illustration of the practical operations in the analysis of such metallic ores, metals, mineral waters, &c. as are commonly to be met with, but extend to minerals which occur but rarely, and the proper mode of analysing which, it is only therefore of so much the greater consequence to know distinctly. Two new plates have also been added, descriptive of the instruments most necessary for the analysis of bodies by means of re-agents or tests. The work has upon the whole

been

been much improved, and it is with confirmed satisfaction that we repeat our recommendation of it, as a most useful manual to every student of chemistry.

Mr. Accum has in the press, a third edition of Chemical Amusement; comprehending a Series of instructive and striking Experiments in Chemistry, which are easily performed, and unattended by danger. With plates by Lowry.

Essays on the Proximate Mechanical Causes of the general Phenomena of the Universe. By Sir RICHARD PHILLIPS. 12mo. pp. 96.

The present is a republication, in a connected form, of a series of essays, which appeared in the course of last year in the Philosophical Magazine and other periodical works, from the pen of Sir Richard Phillips, on his New Theory of the Universe. The ingenious author, in a brief preface which he has annexed, complains, with some acerbity, of the tardiness of the scientific world in acknowledging the verity of a system, in which he has himself all "the confidence of a martyr." We would beg to refer him for consolation to the excellent maxim of Tacitus—*Veritas visu et mora, falsa festinatione et incertis valescunt.*

Memoirs, Biographical, Literary, and Critical, of the most eminent Physicians and Surgeons of the present Time in Great Britain; with a choice Collection of their Prescriptions, and Specification of the Diseases for which they were given, forming a complete modern extemporaneous Pharmacopœia: to which is added, an Appendix, containing an Account of all the Medical Institutions of the Metropolis, both scientific and charitable.

XXII. *Proceedings of Learned Societies.*

ASIATIC SOCIETY.

Calcutta, Feb. 25.—A Meeting of the Asiatic Society was held on Wednesday, the 11th, at which the Lord Bishop presided.

A letter was read from M. Cuvier, perpetual secretary to the Royal Academy of Sciences at Paris, introducing, in the name of the Academy, M. Diard to the favourable attention of the Asiatic Society. M. Diard is one of the correspondents of the Royal Museum of Natural History. M. Cuvier at the same time presented several works of his own composition. *Mémoires pour servir à l'Histoire de l'Anatomie des Molusques* have been received.

A let-

A letter was also read from M. du Trachet, transmitting to the Society his Researches on the Membranes of the Fœtus and on the Rotiferes.

A communication was received from Dr. N. Wallich, superintendant of the botanical gardens, submitting to the Society descriptions and drawings of some interesting Asiatic plants, viz. the *Daphne involucrata*, *Daphne cannabina*, and *Menispermum Cocculus*, with remarks. Dr. Wallich also favoured the Society with some samples of paper made of the bark of the paper-shrub, a species of *Daphne*, and probably the same that is described by Father Louriero in his Flora of Cochin-China. The paper manufactured from this substance is extremely cheap and durable. It is said to be particularly calculated for cartridges, being strong, tough, not liable to crack or break, however much bent or folded, proof against being moth-eaten, and not in the least subject to dampness from any change in the weather. If kept in water forany considerable time, it will not rot, and is invariably used all over Kemaoon, and in great request in many parts of the plains, for the purpose of writing genealogical records, deeds, &c. The method of preparing the paper is extremely simple. The external surface of the bark being scraped off, that which remains is boiled in clean water, with a small quantity of the ashes of the oak, which whitens the material. It is then washed, beat to a pulp, and, after being mixed up with the fairest water, is spread on moulds of frames made of common bamboo mats. Besides these, Dr. Wallich presented to the Museum a specimen of the *Bhojputtra* of the natives, being the outer rind of a new species of birch. It is much used in the mountainous countries to the north for writing upon, particularly by the religious. On one of the pieces was a letter written by the Rawal (head-priest) of Kiddernath, a temple on one of the mountains of the Himulayah, and a great place of Hindoo pilgrimage. For these specimens Dr. Wallich was indebted to the liberality and kindness of the Hon. E. Gardner, Resident at Katmandoo, who has already enriched the botanic garden with many valuable vegetable productions of Nepaul.

In presenting a *Mémoire sur l'Elévation des Montagnes des Indes*, by M. de Humboldt, Dr. Wallich laid before the Society some observations on several passages in that work by Capt. W. S. Webb, from which it appears that an incomplete manuscript copy of Capt. Webb's survey of the Himulayah mountains, or partial extracts from it only, had been seen by M. Humboldt, which has led that writer into a mistake respecting the height of the highest peak of that range.

Two Javanese works, one entitled *Jaya Alancara*, or Annals of of Victory, and the other *Aeshara Sandhi*, on Orthography, were presented in the name of A. Seton, Esq. by Capt. Lockett.

The Pentateuch complete, printed with metallic moveable characters, 1815–17, was presented by the Rev. Mr. Marshman. This is another valuable proof of the useful and meritorious exertions of those indefatigable individuals who compose the Baptist Mission at Serampore.

A letter was read from a new institution, called the Société Polytechnique of the Island Bourbon, desiring to establish a correspondence with the Asiatic Society.

A mathematical paper on the Cardioille was received from Capt. Grove, of the royal Danish engineers.

A letter was read from Mr. Thomson, late private secretary to the Marquis of Hastings, dated Calicut, Nov. 3, 1817, transmitting to the Society drawings of the *Cobra Manilla*, and two sorts of sea snake. It is said that the *Cobra Manilla* is known on the Malabar coast as the bangle snake, and this same is a translation of *Wala Caripan*, which in the Malabar language signifies the deadly bangle, or bracelet; it has two fang teeth, exactly like those of the *Cobra Capello*, and its bite is reckoned equally dangerous. The length varies from six to twelve or fourteen inches; but the female, although rather larger, has less brilliant colours than the male. Mr. Thomson during his residence in Bengal and the Upper Provinces had tried without success to obtain the snake called *Cobra Manilla*. He observes that the late Gen. Gillespie received the bite of this serpent when he was plucking a peach, and in two or three minutes afterwards lost all sensation. The last thing he recollected was some persons calling out for *eau de luce*, which applied very copiously, both internally and externally, he believed, saved his life, but he added that his constitution was not fully restored in two or three years. Mr. Thomson during his stay at Calicut accidentally discovered a species of silk-worm which feeds on the leaves of the wild mango-tree. Among the caterpillars he collected, for the purpose of obtaining butterflies, were some about the size of a man's little finger, with heads and tails of the colour of bright coral, and bodies covered with silvery hairs rising from a black skin. They soon left off feeding and became restless, endeavouring to crawl up the sides of the glass shade under which they were placed. The motion of their heads from side to side was constant and regular, and Mr. Thomson at length found that they had constructed ladders of most imperceptible threads, and when furnished with dry twigs they began to form their pods. The quality of the silk is coarser than that of Bengal, which may proceed from the nature of their food, as mulberry-trees are not found

found in the neighbourhood of Calicut. Drawings of the male and female silk-moth accompanied this communication.

M. Cuvier was proposed as an honorary member of the Society by the Lord Bishop, and duly elected.

SOCIETY FOR THE ENCOURAGEMENT OF INDUSTRY IN FRANCE.

Prizes proposed for competition during 1819:

Mechanic Arts.

For the application of the steam-engine to printing-presses.

The Society proposes a prize of two thousand francs to the person who shall put in action, by means of the steam-engine, one or more typographic presses, constructed either according to the old method, or according to any other method. The press thus worked must produce in a given time a greater number of impressions than in the ordinary way, and the clear advantage gained by it must be much greater than what is commonly obtained. The competitors to transmit descriptive memoirs accompanied with designs of the presses which they have employed, and certificates from the local authorities of their having been in active use for three consecutive months.

For the fabrication of a new species of œconomical carpet.

The Society, persuaded that the proper furnishing of houses contributes essentially to the comfort and health of individuals, proposes a prize of two hundred francs to the person who shall before the 1st of May 1819 have fabricated and sent to market a sort of carpet, the price of which shall be one half cheaper than that of the cheapest carpeting at present known in Paris.

Chemical Arts.

For the fabrication of an indelible green colour preferable to the green of Scheele.

The Society proposes a prize of two thousand francs to the inventor of the best means of preparing one or many solid and brilliant greens, fit for being employed in dyeing, in oil painting, and in paper staining. The green must be superior to the green of Scheele, and to those which are now in use.

For the discovery of the best process of pounding colours in oil and water to the degree of consistency required by artists.

A prize of 500 francs.

For the fabrication of animal charcoal from other substances than bones, and by a process different from that employed for preparing Prussian blue.

The certainty that other animal matters besides bones, can be brought to yield charcoal of a good quality, is deduced from the knowledge of a very important fact furnished by the employment of of the charcoal left in the fabrication of Prussian blue. This charcoal, when it has been prepared, possesses qualities infinitely superior to those of charcoal produced from bones, and it is known that it is furnished by other animal matters than bones, and is prepared by potash.

In the charcoal residue of Prussian blue, the whole, or nearly the whole, is charcoal; while in the charcoal from bones, there is scarcely more than a fifth of pure charcoal; the other four-fifths are formed of phosphate and carbonate of lime, matters altogether foreign to the action of charcoal.

From these considerations, the Society proposes a prize of two thousand francs to the person who shall communicate a certain and œconomical process for converting animal matters, other than bones, into a charcoal possessing all the qualities of charcoal from bones. The price of the charcoal thus obtained must not be greater than the present price of charcoal from bones (ten centimes the pound).

For the fabrication of isinglass.

The Society offers a prize of two thousand francs to the person who will establish in France a manufacture of isinglass, of a quality which may stand competition with the isinglass of the north. To be awarded in July 1819.

Œconomical Arts.

For the discovery of a vegetable substance, either natural or prepared, which will serve as a complete substitute for the leaves of the mulberry in the rearing of silk-worms.

A prize of two thousand francs.

ROYAL ACADEMY OF SCIENCES AT PARIS.

At a meeting of the Academy on the 30th of March last, M. Desfontaines made a report on a memoir by M. Dellile, on that long sought for tree of ancient times, the *Persea*. It was formerly, as we learn from the descriptions of Pliny, Theophrastus, and Dioscorides, much cultivated in Egypt on account of an excellent fruit which it yielded; but for ages past it has wholly disappeared from the banks of the Nile. M. Dellile thinks, however, that he has now recognised it in the *Xymenia Egyptiaca* of Linnæus, one specimen of which he saw in a garden at Cairo, and two others in Upper Egypt; and it appears also from his researches, that it abounds in Nubia and Abyssinia, under the name of the *glig*. The tree as described by Theophrastus "resembles the pear-tree; but differs from it in being evergreen. It produces fruits in abundance, which ripen about the time of the Etesian winds. When the fruit is intended to be kept, it is gathered

thered before it is quite ripe. In this state it is of a geeenish colour, and in form like an almond or elongated pear; the pulp, which is soft, agreeable to the taste, and of easy digestion, incloses a stone like that of a plum, but smaller and harder. The wood of the *Persea* is dense, and of a fine black colour, and is used for making tables and statues."

Prize Questions proposed by the Academy for 1820:

To form by the theory of universal gravitation alone, and without taking from observations any thing but arbitrary elements, tables of the movement of the moon as exact as the best tables in existence.

The following theorem of Fermat:—"Beyond the second degree there exists no power which may be divided into two other powers of the same degree."

The prize for each question is a gold medal of 3000 francs value, and the 1st of January 1820 is the latest time allowed for the reception of memoirs.

ROYAL ACADEMY OF SCIENCES AND BELLES LETTRES OF BRUSSELS.

The following questions have been proposed by this Society for competition in the Class of Sciences during the year 1819:

1. If to each of the angles of a plane perfectly square, from the centre of which a certain weight P (say of 100 lb.) is suspended, a cord be attached, which passes vertically on a pulley; and if each of these cords be charged with such a weight, as that *first*, the sum of the four shall be equal to 100 lb.; and *second*, that the weights attached to each of the two angles diagonally opposite, shall be equal to each other; as for example, two of them 49 lb., and the two others one pound each, and so through an infinite series of numbers—it is known by the ordinary rules of statics, that this plane will remain horizontally in equilibrio. On the other hand, if these four cords, instead of being thus loaded with weights, and passing on a pulley, are fixed to an immoveable board, it is obvious, but only on the metaphysical principle, *that wherever there is a perfect equality of efficient causes, the effects are also necessarily equal;* that the portions of the weight P, borne by each of these four points, will also be exactly equal to each other.

The point then is, to assign a principle truly physical—that is to say, founded on the properties of matter alone, from which there may result, among the infinite series mentioned above, relations among the four weights all equally proper to establish the equilibrium in the first hypothesis, and the preference which the relation of equality bears in the second—that is to say, when the distribution of the force to sustain depends actively and entirely

tirely on the weight P attached to the centre of the square plane.

In the 18th vol. of the Memoirs of the Academy of Petersburgh, Euler has treated this subject in all its generality with admirable skill and profoundness (*De pressione ponderis in planum cui incumbit*); but in the judgement of d'Alembert (*Opusc. Mathem.* tome viii. p. 40, § 13) the solution is still uncertain and hypothetical; and, in fact, the principle upon which it is founded seems rather to be a mathematical hypothesis than a physical principle.

It is therefore required,

First. That this principle be discussed fundamentally, and that it be demonstrated in a positive manner, whether it is or is not admissible as a physical principle.

Second. In the event of the demonstration being in the negative, that it be examined, whether by presenting this principle in any other point of view it cannot be confirmed, and the beautiful theory which flows from it be thereby preserved.

Third. If neither of these trials are satisfactory, that there be assigned for the particular case which has been specified above, a principle which shall be free from all objection.

2. Assuming that there is an identity between the forces which produce electrical, and those which produce galvanic phænomena; whence comes it that we do not find a perfect accordance between the first and the last?

3. Many modern authors believe in the identity of chemical and galvanic forces:—Can the truth or falsity of this opinion be proved?

The prize offered for each of these questions is a gold medal of the weight of twenty-five ducats. The memoirs may be written in Latin, French, Dutch, or Flemish, and transmitted previous to the 1st of February 1819 to the Secretary of the Academy M. Van Hulthem.

SOCIETY OF SCIENCES OF HAARLEM.

The following questions in the Physical Sciences have been proposed by this Society for competition previous to the 1st of January 1819:

1. What is the origin of carbon in plants? Is it produced by the vegetation itself either entirely or in part, as the experiments of M. Von Crell appear to have established, and as many philosophers suppose? If it is so, in what manner is this production effected? If not, in what manner does the absorption of carbon by plants take place? Is it effected after it is combined with oxygen and transformed into carbonic acid, or in what other mode?

 2. To

2. To what source are we to ascribe the iron which we find in the analysis of some plants? Is it to be ascribed in every case to particles of iron which the plants have taken up with their natural aliment, or can it be evidently proved by observations, that it is produced at least in some cases by the vegetation itself? And what light do these observations throw upon other branches of physics?

ROYAL INSTITUTION.

Mr. Brande will commence an extended and practical Course of Lectures and Demonstrations on Chemistry in the Laboratory of this Institution, on the first Tuesday in October, at nine in the morning, to be continued every Tuesday, Thursday, and Saturday. Two Courses are given during the season, which begins in October and terminates in June, and the subjects which they comprehend are treated of in the following order:

I. Of the powers and properties of matter, and the general laws of chemical changes. II. Of undecompounded substances, and their mutual combinations. III. Vegetable chemistry. IV. Chemistry of the animal kingdom. V. Geology.

COMPLETE SCHOOL OF PHYSIC IN IRELAND.

The following particulars respecting this school for the instruction of Students in Medicine, Surgery, and Pharmacy, will prove acceptable to many of our readers.

Foundation.

In 1704, Sir Patrick Dunn instituted, in his life-time, two Professorships in Dublin, viz. "one of Osteology, Bandages, and Operations of Surgery, and one of ancient and modern Materia Medica, and Pharmacy." Sir P. Dunn further directed by his will in 1711, that, if his funds were sufficient, "there should be Lectures publicly read on the Anatomy of the Bodies of Men, or the Bodies of Animals—on Chirurgery and Midwifery—on Botany and the Dissections of Plants." He also ordered, that the Professorships on these several subjects should be bestowed according to the merits of the candidates, to be ascertained by an examination on three several days, two hours each day. A King's Professor of the Theory and Practice of Physic, with corporate powers of holding and letting lands, was instituted by George the First.

An act was passed in the 21st year of George the Second, by which the King's Professorship of Physic, and the Professorship of Surgery and Midwifery, and that of Materia Medica and Pharmacy, instituted by Sir Patrick Dunn, were incorporated and established by law. Before this period also, Lectureships existed

existed in Trinity College, on Anatomy and Surgery, Chemistry and Botany.

Throughout the 21st of George the Second, and the will of Sir Patrick Dunn, recited in it, the different Lectures are always mentioned as being intended for the "instruction of Students of Medicine, Surgery, and Pharmacy;" and from thence arose the title of The Complete School of Physic, adopted in the subsequent acts, viz. the 25th, 31st, and 40th of His present Majesty, in which the 21st George the Second is constantly recognised as the foundation of the school, and as being still in force, except "as relates to the number of Professors, the Electors and the mode of election, the tenure and salaries of the said Professors, and the times and manner of lecturing."

Students.

The several Students in Physic are matriculated in the University, for which they pay five shillings; but such Students, unless they shall think proper, are not obliged to attend to the academical duties of the University. The several Lecturers, when they have delivered on half of their Courses, return to the senior Lecturer of Trinity College a list of such pupils as shall have attended them during such part of their Courses.

Professors.

There are six Professorships. Those of Anatomy and Surgery, of Chemistry, and of Botany, are on the foundation of Trinity College, and are called the University Professorships; those of the Institutes of Medicine, of the Practice of Medicine, and of Materia Medica, and Pharmacy, are on Sir Patrick Dunn's foundation, and are named King's Professorships. Provision is also made for the addition of a King's Professor of Midwifery, as soon as Sir Patrick Dunn's funds shall permit.

"The King's Professorships are open to persons of all nations professing their faith in Christ, and the Professorships of the University to Protestants of all nations;" and for both, it is required either to have taken medical degrees in some University, or to have obtained a license to practice from the College of Physicians, in consequence of a *testimonium* under the seal of Trinity College. Immediately before the election of any Professor, the electors are sworn to vote without "favour, partiality, or prejudice;" and immediately on being declared elected, the Professor is sworn to perform "his duties to the best of his skill and judgement." The electors of the King's Professors are the Provost and the Professor of Physic of the University, with three physicians chosen by ballot from their own body by the College of Physicians. The University Professors are elected by the Provost

 and

and senior Fellows of Trinity College. Each Professor is chosen for seven years, but may be continued, or may be re-elected.

In addition to the fees derived from the medical Students, the King's Professors receive a salary from Sir P. Dunn's estate, and the University Professors are paid by the Students of Arts in Trinity College, for the public or collegiate Course of Lectures.

Lectures and other Means of Instruction.

The University Professors deliver annually a public Course of twelve Lectures on their respective subjects.

Lectures on the following subjects are delivered from the 1st Monday in November until the end of the succeeding April, viz. on Anatomy and Surgery, and on Chemistry, in Trinity College. On the Institutes of Medicine, on the Practice of Medicine, and on Materia Medica, and Pharmacy, in Sir P. Dunn's Hospital. The Lectures on Botany commence on the 1st Monday in May in Trinity College, and continue until the end of July. Terms for each of these Courses of Lectures, four guineas.

Clinical Lectures are given on the cases of the patients in the Hospital, at least two days in each week of every session. This duty is taken for three months by the Professors, alternately, or in such other order as shall be agreed upon amongst them. Terms of each Course, three guineas.

Lectures on Comparative Anatomy, Physiology, and Pathology, are given by the Professor of Anatomy and Surgery, twice a week during the session, without additional expense to those who pay for the Lectures on Anatomy and Surgery. To other pupils, the terms for these Lectures are two guineas.

Anatomical Demonstrations are given daily, from the beginning of the session until April, by the Demonstrator of Anatomy in Trinity College. The Students are superintended in their dissections, and subjects are provided for the muscles, blood-vessels, and nerves. A private room is allotted to the use of practitioners who may wish to improve their knowledge of anatomy. Terms for dissections, subjects, and demonstrations, six guineas; for the demonstrations alone, four guineas.

Students who wish to be instructed in the performance of surgical operations on the dead body, may be superintended, and have the necessary number of subjects provided them. Terms for which, five guineas.

Towards the end of the session, a Course of Lectures is given on the diseases of the skin, by the Professor of Anatomy and Surgery; and one on the diseases of the eye, by the Demonstrator of Anatomy. Terms for each of these Courses, one guinea.

At the Chemical Laboratory, operating pupils are received and instructed in the details of chemical and pharmaceutical processes. Terms for such instruction are six guineas.

Students in botany have access to the botanic garden, which is in the immediate vicinity of Dublin, and have the opportunity of taking frequent excursions with the Professor of Botany and his assistant, to the mountains and sea-coast adjacent to the city.

Botanical Demonstrations are daily given by the Professor's assistant in the garden during the season. Terms of which, one guinea.

A Course of Lectures on Mineralogy is delivered by the Professor of Natural History, in Trinity College, to which those who have their names on the books of the University are admitted gratis.

The Museum of Trinity College, to which Students have admission two days in the week, contains a collection of minerals, systematically arranged, with references to a printed catalogue.

Pupils are taken by the Apothecary of Sir Patrick Dunn's Hospital, and instructed in the Practice of Pharmacy. Terms for which, during three months, two guineas.

A Medical Society holds weekly meetings in Trinity College, for the purpose of discussing subjects connected with Medicine, Surgery, or Pharmacy. A medical circulating library belongs to the members. Terms of admission to the Society, with the use of the library, one pound.

Medical Officers of the Army and Navy are permitted to attend the Lectures on Anatomy and Surgery, in Trinity College, without fee.

Hospital.

This is chiefly supported by the rents of Sir P. Dunn's estates, and partly by private contribution. The Board of Governors consist of the Visitors of the College of Physicians, the President, Vice-president, and Censors of the same, the Provost of Trinity College, and twelve Subscribers; but "no physician or surgeon" of the hospital is eligible to be a governor. The house is intended to hold one hundred and thirty patients, of whom thirty are selected for instruction and lectures, by the Clinical Professor for the time: the rest are placed under the care of a physician appointed by the Governors.

The cases of the clinical patients in the hospital are recorded. Every opportunity is also taken to examine the bodies of patients that die; the morbid appearances are explained to the Students, and preserved in the pathological collection of the School.

At present, all pupils are permitted to attend the entire practice of the Hospital during a year, for three guineas. Formerly, this privilege was extended to those only who had studied at least two years in Arts in the University of Dublin, Oxford, or Cambridge. All other pupils paid twenty guineas.

Library.

A large collection of medical books, bequeathed by Sir P. Dunn, is appropriated to the use of the Students, and provision is made for purchasing books in proportion as the funds increase. A Librarian is appointed annually, by the College of Physicians, with a salary of seventy pounds per annum. He furnishes the necessary fuel for the library and medical lecture-room, and discharges such duties as shall be prescribed to him by the College of Physicians.

Degrees.

The Students who do not graduate in Arts are permitted at the end of three years, from the date of their matriculation, to undergo an examination before the six Professors of the School, in their respective departments, on producing to the Board of Trinity College, certificates of diligent and regular attendance on Anatomy, Surgery, Chemistry, Botany, Institutes of Medicine, Practice of Medicine, Materia Medica, and Pharmacy, the Clinical Lectures and Practice of Sir Patrick Dunn's Hospital. They likewise write a Thesis in Latin. If found qualified by the examination, they publish the Thesis, perform the academical exercises for the degree of Doctor of Medicine, and receive the following *testimonium* from the Board of Trinity College:

"Omnibus ad quos præsentes literæ pervenerint salutem. Nos Præpositus et Socii seniores Collegii Sacro-Sanctæ et individuæ Trinitatis, juxta Dublin, testamur *A. B.* quamdiu apud nos commoratus est, sedulam operam Medicinæ navasse, examinationes solitas coram sex Medicinæ Professoribus feliciter sustinuisse, cæteraque exercitia necessaria præstitisse, his adducti judicamus eum habilem ac idoneum esse, qui exerceat artem Medicinæ quatenus leges statutaque regni permittunt; in cujus rei testimonium, manus et sigillum quo in his utimur, apposuimus. Anno Domini, &c. &c."

The Students who go through a collegiate Course, on producing certificates of their strict attendance on the Lectures of the Professors in the School of Physic, on the Clinical Lectures and the Hospital, are, three years after having graduated as Bachelor of Arts, admitted to an examination before the Regius Professor of Physic and the Professors of Anatomy and Surgery, Chemistry, and Botany, in Trinity College. On being approved, and performing the usual academical exercises, they take the degree of Bachelor of Medicine. Upon sufficient standing, publishing a Thesis, passing a second examination before the University Professors, and performing the necessary acts, the full degree of Doctor in Medicine is conferred. These rank with the degrees of

Bachelor and Doctor of Medicine obtained in the Universities of Oxford and Cambridge.

As qualifications previous to examination for the *testimonium*, the certificates of the Professors in Edinburgh are admitted for any three of the Courses required, with the exception of the Clinical Lectures, which must have been attended in the School of Physic in Ireland.

Certificates of attendance on the Professors in the School of Physic in Ireland are received, as giving standing in other Universities, and as qualifications for medical officers in the army, navy, and East India service. And certificates of attendance on the Anatomical and Surgical Lectures in Trinity College, are also admitted in the different Colleges of Surgeons.

XXIII. *Intelligence and Miscellaneous Articles.*

IMPROVEMENT AND EXTENSION OF IRON RAIL-WAYS.

The Highland Society of Scotland have recently announced the following premium; viz.

A piece of plate, of fifty guineas value, will be given for the best and approved essay on the construction of rail-roads, for the conveyanee of ordinary commodities. In this essay it will be essential to keep in view, how far rail-roads can be adapted for common use in a country;—the means of laden carriages surmounting the elevations occurring in their course; and whether rail-roads, or the wheels of carriages, may be so constructed as to be applicable to ordinary roads, as well as to rail-roads, so that no inconvenience shall be experienced on leaving either to travel on the other: the essay to be accompanied with such models or drwings as shall be sufficient to illustrate the statements it contains.

It is desirable that some account should be given of the principal rail-roads in Britain, together with a brief history of their introduction. The premium not to be decided until the 10th November 1819.

And with the same view, the following circular letter has been addressed to the various iron-masters in Scotland and England; viz.

"Sir,—Although the rail-way that is now in contemplation in the vicinity of Edinburgh be entirely a matter of local concern, the peculiar plan of it is certainly to be viewed in a different light, as an object that well deserves the attention of the various classes of the community throughout the kingdom. Instead of insulated patches of rail-way, here and there, for particular

ticular purposes, and for the conveniency of private individuals, as is now the case, it is here proposed, through the medium of rail-ways, to open extensive communications—to branch them out from the metropolis of Scotland in various directions, and to distant points—and thus to facilitate conveyance in general by an improved system of roads for heavy carriages.

"The Highland Society of Scotland have, in a very patriotic manner, offered a premium of fifty guineas for the best essay on the means of attaining so desirable an object as the introduction of rail-ways for the purposes of general carriage.

"With a view to the establishment of the rail-way in question, for the conveyance of commodities to and from Edinburgh, and thereby to give a commencement to the system generally, a subscription for a *survey* has been opened, and plans by Mr. Stevenson, engineer, are in considerable forwardness.

"It seems to be desirable, that rail-ways, for alternate carriage and general use, should proceed on a continued level, or upon successive levels: and a simple system of *lockage* (if it may be so called), by which loaded waggons may easily be elevated or depressed, from one level to another, would appear to be a desirable attainment. The edge rail-way is generally used and preferred in Scotland, as causing less friction, and less expense of horse power; and it would tend to facilitate the general use of rail-ways, if, by some simple change, the wheel usually employed for the road or street could be made also to suit the rail-way, or the rail-way wheel be made to suit the road or street, so that the cart or waggon which brings the commodity from the colliery or stone quarry, the farm-yard, or the manufactory, to the rail-way, might travel along it to the termination of the rail-way, and proceed from thence through the streets of the town to the dwelling of the consumer, without unloading, or change of carriage.

"The general use of rail-ways by iron-manufacturers, for their own peculiar objects, qualifies them in an eminent degree to afford valuable suggestions on the best means of perfecting the rail-way system; and from a desire to collect the general sense of enlightened and scientific men, we take the liberty of submitting the annexed queries to your consideration, and to request, if agreeable to you, that you will be pleased to favour us with any suggestions which may occur to you upon the subject.

"Nothing could give a stronger impulse to the iron-manufacture than the complete success of this scheme. It seems to claim the attention of the iron-manufacturers of Great Britain as a body, and to merit their individual and collective support.

"Edinburgh, March 25, 1818."

Queries.

Queries.

1. What is the best breadth of rail-way, and the best form of a waggon or carriage for the conveyance of commodities in general?

2. Supposing the trade *alternate*, it will be desirable that the rail-way should proceed on a continued level, or upon successive levels. What are deemed the best means, with reference to œconomy and dispatch, for elevating or depressing the laden carriages from one level to another?

3. Supposing the edge rail-way, which is generally preferred in Scotland, to be adopted, can a wheel be so constructed as to be applicable to streets or ordinary roads, as well as to rail-roads, so that no inconvenience shall be experienced on leaving either to travel on the other?

St. George's Medical, Chemical, and Chirurgical Lectures.

These will, as usual, commence the first week of October, in the following departments:

1. On the Practice of Physic, with the Laws of the Animal Œconomy, and occasional Pathological Demonstrations, at No. 9, George-street, Hanover-square, by George Pearson, M.D. F.R.S., Senior Physician to St. George's Hospital, of the College of Physicians, &c.

2. On the Science of Chemistry, in which Medical Jurisprudence will form a part of the Course, at the Royal Institution, Albemarle-street, by W. T. Brande, F.R.S. Prof. Chem. Royal Institution, &c.

3. On Surgery, at Mr. Brodie's Theatre, Great Windmill-street, by B. C. Brodie, F.R.S. Assistant Surgeon at St. George's Hospital, &c.

4. On Therapeutics, with Materia Medica, by George Pearson, M.D. F.R.S. &c.

5. Sir Everard Home will continue his Surgical Lectures gratuitously to the Pupils at St. George's Hospital.

LIST OF PATENTS FOR NEW INVENTIONS.

To Samuel Clegg, of Westminster, engineer, for an improved gasometer or gasholder.—24th July, 1818.—6 months.

To Richard Blakemore, of Milingriffith Work, county of Glamorgan, and John James, of Lower Redbrook, in the county of Gloucester, iron-masters and tin-plate manufacturers, for a new kind of plate, which they denominate 'amorphous metal plates,' and likewise a certain improved and more perfect method or methods of crystallizing, or rendering crystallizable, the surface of of tin-plates, or iron or copper-plates tinned, which plates they call 'amorphous metal plates.'—24th July.—6 months.

To Joseph Manton, of Davies-street, Berkeley-square, gunmaker, for certain primers for fire-arms, and also certain improvements in the construction of certain of the parts of fire-arms.—3d August.—6 months.

To John Malam, of Marsham-street, Westminster, engineer, for certain improvements on steam-engines.—5th August.—6 months.

To James Hollingrake, of Manchester, mechanic, for his improved method of making or manufacturing copper or other metal rollers for calico-printing.—7th August.—6 months.

METEOROLOGY.

Harvest Moon.

The present year is the third of a series of ten years in which the moon will prove the most beneficial to farmers for reaping and gathering in the produce of their fields; viz. from 1816 to 1825 inclusive. The nine years preceding, namely, from 1807 to 1815 inclusive, were of the class of those in which, from general physical causes, the harvest moon was least beneficial.

Meteorological Journal kept at Walthamstow, Essex, from July 15 *to August* 15, 1818.

[Usually between the Hours of Seven and Nine A.M. and the Thermometer (a second time) between Twelve and Two P.M.]

Date.	Therm.	Barom.	Wind.
July			
15	63 80	30·30	N—NE.—Clear and some wind; very hot fine day; clear moon-light.
16	64 82	30·25	SW—W—NW—SE.—Hot sun, and hazy; fine, hot and windy day; clear night.
17	70 76	30·20	SE.—*Cirrostratus;* showers between 8 A.M. and noon; fine day, very hot; mottled *cirrostratus* at 7 P.M.; clear, and *cirrostratus* at night. Full moon.
18	61 79	30·05	NE—SE.—Fine and sunny; fine day, and some wind; very hot; clear, and *cirrostratus* mottled.
19	68 80	30·00	NE—SE—NW.—Sun and *cirrostratus;* fine, very hot day; windy; clear and clouds.
20	63 78	29·95	NW.—Wind, and gleams of sun; very hot fine day; clear night.

July

July			
21	63 77	29·91	W—S—SW—W.—Some sun and *cirrostratus;* fine day; stars and *cumulostratus,* and wind.
22	64 78	30·05	SW.—Sun, wind, and *cumuli;* fine day; very hot; clear and star-light.
23	66 87	30·15	SE—E.—Hot sun, and wind; fine day; very hot clear night, and windy. Barom. 72 at 10 P.M.
24	72 91	29·90	SE—S—SW.—Very hot, clear and calm at 7 A.M.; at 9 A.M. some *cirrus;* fine day; some rain before 8 P.M. Thunder and lightning; very red sunset; *nimbus* and lightning at night*.
25	71 81	29·85	SW.—Clear, *cumuli,* and sun; fine day, sun and wind; clear at night, but some *cirrostratus* NW. Moon last quarter.
26	74 77	29·90	SE.—Sun and wind; *cirrostratus;* and very hot, fine, clear, and windy; some rain after 5 P.M.; night clear, clouds, and wind.
27	70 73	29·85	SE—W—NW.—Sun and *cirrostratus;* clouds and sun; dark *nimbus* NW; showers and wind after 1 P.M.; night cloudy and windy.
28	57 72	30·20	NW—W.—Sun and wind; fine day; windy; clear star-light.
29	64 74	30·20	NE.—Hazy; sun and clouds; fine day; fine star-light.
30	64 79	30·55	NW.—Windy and gray; sunny day; fine hot star-light night.
31	67 74	30·55	W—NW.—Gleams of sun; fine day; windy; after 3 P.M. some rain; cloudy evening; star-light at 11 P.M.
August			
1	60 66	30·00	NW.—Gray and windy; the dust scarcely laid by the rain; *cumuli* and windy; fine

* The Thermometer certainly at 1 P.M. the 24th of August was 91 by the Thermometer north-east side of the house; but probably it was caused by the great *heat* of the early sun not being gone off; for at the *same hour* another Thermometer which was hung on the north side of a large tree, and taken there from shade at 10 A.M., was only 88.

Errata.—15th June, *read* 60 Thermometer *for* 16, and the whole of that day seems wrong in many respects: the wind was SE—SW—S.

The Thermometer is omitted 12th and 13th July.

12th July 66·73.

13th July 63·75.

The leaves fall like October; the 5th August was the only morning in which a strong dew has been observed.

day;

Date.	Therm.	Barom.	Wind.
			day; very clear star-light; Milky-way *uncommonly* bright.
August			
2	62 71	30·20	NW—E.—Clear, and windy; very fine day; fine clear night. New moon.
3	55 71	30·10	SE.—Fine morn; fine day; star-light.
4	62 81	30·10	SE.—Clear and fine mottled *cirrostratus;* fine very hot day; clear night.
5	62 88	30·10	SE.—Fine sun; clear; *strong dew; very hot* fine day; *perfectly clear sky all day, and windy;* at 1 P.M. 88 under a large tree north side; clear night.
6	67 86	30·05	SW—W—NW—N.—Clear and *cumuli*; fine hot day; clouds appeared before 2 P.M.; cloudy night till near 11 P.M.; afterwards star-light.
7	62 73	30·05	N—NW.—Clear and *cirrostratus;* hot sun; fine day; sun and wind; star-light; fine and clear.
8	64 74	30·10	W.—Gray at 7 A.M.; at 10, sunny; at 11, hazy; at 1 P.M. fine sunny day; star-light.
9	64 77	30·10	NE—E.—Gray; some drops of rain before 11 A.M.; sunshine; fine hot day; moon-and star-light. Moon first quarter.
10	63 71	29·95	NW—N—E—NE.—Sun, wind, and *cirrostratus;* fine day, clear windy night.
11	59 69	30·30	E—NE.—Clear and windy; *cumuli* and clear; fine day; clear night.
12	59 71	30·20	E—NE.—Gray fine morn; fine day; sun and wind; clear night.
13	57 73	30·10	NE.—Fine sunny morn; very fine day, sun and wind; clear night.
14	58	30·10	N—NE.—Gray morn; fine day; some drops of rain about 7 P.M.; moon, stars, and light *cumuli.*
15	57 67	30·10	N.—Cloudy and windy; distant hills hazy; wind and clouds; and gleams of sun; cloudy night.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

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

1818	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
July 15	12	77·5	30·41	Very fine
16	13	83·	30·34	Ditto
17	full	72·	30·29	Cloudy—shower A.M.
18	15	75·5	30·16	Very fine—heavy rain with thunder in the evening
19	16	76·	30·06	Ditto
20	67	69·	30·08	Cloudy—rain P.M.
21	18	75·	30·01	Ditto
22	19	79·	30·15	Very fine
23	20	81·5	30·17	Ditto
24	21	91·	29·90	Ditto
	3 P.M.	92·5		with the bulk only—shaded and a cloud over the sun 101·5 notwithstanding a brisk breeze
25	22	85·5	29·89	Ditto
26	23	84·	29·90	Ditto—rain in the evening, with a particularly beautiful appearance of the rainbow
27	24	71·5	29·93	Cloudy
28	25	69·	30·30	Fine
29	26	75·	30·25	Ditto
30	27	76·	30·16	Ditto
31	28	76·5	30·08	Cloudy
Aug. 1	29	66·	30·07	Ditto
2	new	69·	30·24	Fine
3	1	70·	30·15	Ditto
4	2	79·	30·20	Very fine
5	3	84·5	30·12	Ditto
6	4	69·	30·15	Fine—rain A.M.
7	5	69·	30·14	Ditto
8	6	71·5	30·17	Cloudy
9	7	74·5	30·05	Fine—rain in the evening
10	8	66·	30·12	Ditto
11	9	65·	30·31	Ditto
12	10	70·5	30·24	Ditto
13	11	68·	30·28	Ditto
14	12	59·	30·23	Cloudy
15	13	63·	30·20	Ditto

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For August 1818.

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	69	77	60	29·80	56	Showery
28	60	74	64	30·20	58	Fair
29	64	76	66	·13	60	Fair
30	67	80	68	·10	66	Fair
31	67	76	60	·42	56	Fair
August 1	60	72	59	·02	60	Fair
2	60	71	56	·08	58	Fair
3	64	75	59	·06	75	Fair
4	64	82	68	·02	89	Fair
5	68	85	74	·02	102	Fair
6	74	83	63	29·99	83	Fair
7	64	74	66	30·01	91	Fair
8	67	78	68	29·97	72	Fair
9	66	74	60	·87	70	Fair
10	68	70	55	30.13	62	Fair
11	60	69	54	·07	60	Fair
12	60	69	57	·02	57	Fair
13	64	67	59	·06	62	Fair
14	60	64	58	·01	50	Cloudy
15	61	66	59	·01	55	Fair
16	58	70	59	·01	60	Fair
17	58	71	63	·01	61	Fair
18	63	71	56	29·91	75	Fair
19	56	66	55	30.02	60	Cloudy
20	56	63	56	·04	56	Cloudy
21	58	65	55	·06	56	Fair
22	55	66	54	·14	57	Fair
23	53	66	60	·22	58	Fair
24	61	60	61	·17	56	Fair
25	61	70	60	·11	57	Fair
26	60	67	60	29·97	58	Fair

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

XXIV. *On the Question "Whether Music is necessary to the Orator,—to what Extent, and how most readily attainable?"* *By* Henry Upington, *Esq.*

[Continued from vol. li. p. 461.]

To Mr. Tilloch.

Blair's Hill, Cork, July 28, 1818.

Sir, — Your late Magazine for June having contained my communication of the 10th of that month, without any further introduction I shall proceed with my subject.

Examination of The Speaker *continued.*

OF CERTAIN MODULATIONS, &c.

Observation 4.—Aided by my *associate* (to whose excellent ear I acknowledge myself indebted, and without whose concurrence I have never once ventured to decide, throughout this interesting inquiry), I now investigated, as minutely as possible, the most prominent species of sentences and rhetorical figures which presented themselves in animated conversation;—but in vain. They varied, it is true; yet the characteristic difference of any one species of either was absolutely undefinable; the alteration of a single word, or the slightest change of collocation, producing, in the very same sentence, a difference in the modulation. Even the interrogatory itself was equally uncertain—the *pitch* excepted, which towards the conclusion was generally, not always, higher than *that* pitch which an answer to such interrogatory would have produced. The exclamation, especially when consisting of several words, was equally if not more variable in its character than the Interrogatory; but, when constituted by one or two syllables, and indicating *surprise*, it uniformly ascended the scale. The parenthesis appeared more regular, in *one* respect, than either the Interrogatory or the Exclamation, *its* pitch being almost always lower than that of the preceding or succeeding passage. Lastly, with regard to the period—even *this* (I speak of the tolerably well executed period) apparently possessed but *one* undeviating distinction; namely, that its ultimate syllable was at all times lower than the preceding one; the *final cadence* (as I shall call it), or falling of the voice at the conclusion, hardly ever extending itself so far backward as the antepenult.

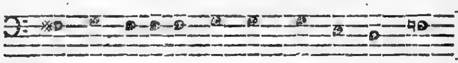
Remarks.—Our coincidence with ancient usage is equally conspicuous in the case of *interrogatory* as in that of the *diapente* which was particularly discussed in my paper of the 10th of June. Quintilian

Quintilian in a certain passage of his "*Institutes*" (the book and chapter of which I cannot immediately call to my recollection). assigns to *this* figure [interrogatory] the property of *acuteness:* his words I remember—"*Interrogantes acuto tenore concludunt.*" [Interrogations terminate in an acute manner.] This general practice among the Romans, so analogous to our own, could not however, any more than with ourselves, have affected all the individual syllables of which an interrogatory is composed; the general term *acuteness* being applicable to the ordinary genius, only, of such interrogatory on approaching its conclusion.

The true character of this figure does not appear, in my mind, to have been justly appreciated by any of our modern grammarians;—they have spoken at random, and were either incapable, or never took the trouble of analysing the question. In what then, independently of local usage, does the *essence of interrogatory* consist? Not, most assuredly, in the elevation or depression of any individual syllable or word,—nor yet in the comparative acuteness or gravity of its general tenor, or even of its conclusion; but—in opposition to the genius of the *period*—a suitable preparation having been made, it must terminate with a *non-finish.* Every musician will understand me: and, to prove the practicability, in different ways, of this operation, in speech, I shall set the following sentence (which is intentionally commenced with a *verb*, and terminated by an emphatical word,) both in an ascending and a descending form.

Experiment.

[The requisites for the execution of this passage will be found in the Philosophical Magazine for May. The *second* example, in particular, as deviating considerably from our ordinary habits, may require some little practice. The judicious management of *forte*, and the avoiding of *jerk*, especially on the two last syllables of the word "proportion," must be attentively regarded.]


Ascending.

Are *you* sa-tis-fied with your own *pro-por-tion?*


Descending.

From the most cursory survey of both these interrogatories, which are perhaps equally good; although the latter, notwithstanding its intentional termination with a flat fifth, has, when wel executed, been more generally preferred—is it not evident that our grammarians have been very idly employed in ushering

their

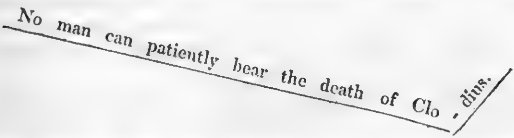
their local habits or fanciful *chimeras** into the oratorical world as the models of perfection? In modern Europe, where every desirable license compatible with the character of speech is given to the speaker, shall he not be permitted to play off his *voluntary* as he pleases? Good taste and decorum should be his only guides.

The *exclamation* and *parenthesis* require no comment. But not so with our PERIODS, or rather that portion of our periods which, in this letter, I have already called the "*final cadence;*" the discussion of its peculiarity being important. Our *music* possesses, in this instance, an unbounded latitude; and such latitude being very frequent in our ordinary tunes, must it not appear that to the habitude of *singing* which prevails throughout society, may be attributed that imperfection so generally complained of in our public readers, and even indeed in our extemporaneous orators, of falling immeasurably through the scale at the conclusion of their periods?

Our popular songs, our psalms and hymns, have in all probability contributed, particularly in this respect, to the injury of our elocution †; the ear, too often habituated to four or five successively *descending* syllables or notes, being intuitively led, in all cases of studied composition, to a *similar* modulation, of which the character of speech can never realise the execution ‡. Hence syllable is tumbled upon syllable; non-articulation is the result, and the die-away voice of the speaker becomes inaudible to all around him.

I know not any song whatever, notwithstanding its five falling

* At the head of *this* list I shall place the "Elements of Elocution," and "Rhetorical Grammar," by the late Mr. Walker. Take one of his own figures:



What! Eleven syllables, all descending in succession, and *slided* too—without any intermediate elevation, repetition, or even *sustentation!* Preposterous idea! Why did he not consult a musician? No human being, in any age or nation, not even Mr. Walker himself, could ever have uttered a sentence in such a manner.

† Almost all the good singers and good musicians whom I ever knew, were indifferent *speakers:* and I am acquainted with one person in particular, whose elocution has been materially injured by the cultivation of the violin.

‡ The successively *ascending* intervals with which our music abounds are almost equally injurious to oratory. Speech, without sing-song, rather rarely exhibits two, and is generally confined to *one*.

notes at the conclusion , whose character more nearly approaches the character of speech, than our national air of " God save the King:" divested of its TIME, it may, with the exception of these notes, be almost *spoken;* a quality peculiarly ascribable to certain passages of the *recitatives* of HANDEL; but in no degree to those of our fashionable composers.

This great original, in fulfilling the design of *recitative,* (which is, or rather should be, no more than highly-coloured *recitation,*) must, in my opinion, have closely attended to the modulation of our best speakers, particularly our theatrical performers; otherwise, it were impossible that such extraordinary compositions could have issued from his hand. In the oratorio of Athalia he has outdone himself; and therefore I shall confine myself, in the present observation on periods, to the close of that remarkable passage already quoted in your Magazine for May*.

Here is the true art of speaking literally made visible; and to this passage I would direct the particular attention of every orator; for without some reasonable approximation to this excellent method, I cannot see the possibility, in superior composition, of executing a graceful, and at the same time an *audible* period. This method is closely analogous to the ancient practice. The voice was let down some few words before the close†: a new *level* (if level it may be called, while admitting of certain variations) was the consequence of their *accentual* system; and the ultimate *falling* syllables, which never exceeded *two,* were governed by the character of the final word.

I shall endeavour to illustrate this level and these ultimate syllables, as far as they relate to the terminating word of an ancient period, by an appropriate example. Suppose that the

* In a more suitable place I shall gratify the reader with a transcript of the *entire* passage, of which the May Magazine contains but an extract.

† This letting down the voice may be called the *rhetorical cadence,* of which Quintilian, in the xith book of his Institutes, chap. 3d, has given us an example. Treating of the period which terminates the opening of the Æneid, he says in the plainest terms, "When I come to *atque altæ mœnia Romæ,* I will lower my voice [*depônam*] and then form a new beginning." Cicero in his "*de Oratore*" says that the last *three* words [I should suppose *independent* words, exclusive of particles,] are sufficient for the governance of a period.

word

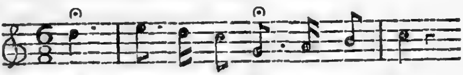
word *exemplification* is equally Greek and Latin with respect to its emphatic syllable *ca*, and that the only existing difference is dependent upon the *notes*, of which the highest or *accent* is seated in the Grecian language upon the unemphatic syllable *fi*; and in the Latin upon the emphatic *ca*: Now, for the conclusion of a period with this identical word, it would necessarily be expressed in the respective languages thus:

In the Greek, ...

In the Latin, ...

methods more lofty and less monotonous than ours; preserving, in the first place, the level or common tone throughout every syllable preceding the accent—then rising, *ad libitum* within the diapente, upon the *accented* syllable; and lastly, falling in like manner *progressively** through the scale, on every syllable which succeeds it.

Although the musical reader must thoroughly comprehend that *falling* is by no means necessary to a finish; yet Lord Kaimes, in his "Elements of Criticism;" and Mr. Sheridan in his "Art of Reading," having inculcated the contrary doctrine to the *classical* world, it becomes, in a certain degree, necessary to refute them. Had these gentlemen been at all acquainted with our musical compositions, they must have seen not only the practicability but the frequent introduction of the *ascending* finish—of the beauty of which the Greeks were so truly sensible, that, even in *speech*, it constituted a part of their *accentual* variety†.

With what excellent effect has our celebrated Braham introduced this species of close, in his well-known song "*On this cold flinty rock, &c.*"

Of the tonical Situation of the EMPHATIC SYLLABLE.

Observation 5.—The predominant, although by no means the

* Aulus Gellius in his *Noctes Atticæ*, book xiii. chap. 24. has shown us that the habit of *progressively* descending, after the accent, prevailed with the ancients. His original words are "*deinde gradatim descendunt.*"

† In the Grecian language, when the last syllable was acuted it *rose*. The Roman language being confined (in some degree, like our own,) to the *descending* period, was more monotonous.—*Quint. Instit.* book xii. cap. 10.

universal character of this syllable called by us the "*accent*," was that of elevation or acuteness.

Remarks.—Notwithstanding the closest investigation of this "accent," it was found impossible to analyse the causes of its deviation from its general character; such deviation depending to all appearance *rather* on the mere circumstance of modulation, or perhaps of habit, than on the nature of the existing sentiment or sentence. Its predominating genius is, for the present, sufficiently delineated in that passage which I have already set for the reader's perusal in your Magazine for May; and which passage shall be repeated in the subsequent part of this article. Other examples, in due time, may follow.

Were it now demanded, whether an accentual language like the Greek or Roman, or a non-accentual language like our own*, (I speak not of Euphony, in which we are infinitely excelled by both,) is better calculated for expression? I would not hesitate to declare in favour of the modern; and for this obvious reason—The fewer the shackles, the greater the latitude for the performer. Let it not however be understood that I would yield the preference to our own, or indeed to any other modern language as generally *spoken* by the respective people of this or of any other country: on the contrary, with all their accentual shackles, I would prefer the ancient Greek, and perhaps the Roman too, as certainly possessing much more of grace and dignity, with an equal if not superior susceptibility of expression.

To form an adequate estimation of the dignity and expression of the ancient languages, which were intentionally destined for *national characteristics*, it would be indispensably necessary to recite a certain number of *original* passages, agreeably to the acknowledged outlines of accent and quantity; or, in other words, of tone and of time;—and to compare them, so recited, with a certain number of our own: but this arduous task, which the powers of a Catalani or a Braham, combined with oratorical judgement, could with difficulty achieve, not being accomplishable by any ordinary musician, even if all the necessary *signs* were previously invented,—we must content ourselves, on the present occasion, with a comparative exhibition of some certain passage in our native tongue; and this rather as an object of literary curiosity than a decisive instance of Greek or Roman preeminence.

Let us suppose then, that in exemplification of our English

* The particles with which our language abounds must render all attempts at regular accentuation unavailing. Our general modulation is governed, in a great degree, by necessity, not choice.

usage,

usage, *that* passage which I have already given may be quoted in its original form; thus *,

English.

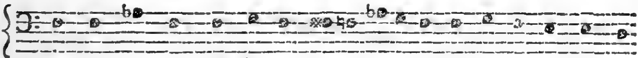
May the monarchs of England ever cultivate the happiness of man.

and that, agreeably to the usage of the Greek and Roman languages, the *accentual* rules of which are known to every grammarian †, this same passage were designated in the following manner, as if in those languages, for recital;

Greek.

Mày thè mónarchs òf Englànd éver cultívate thè háppiness òf mán

Roman.



Mày thè mónarchs òf Éngland éver cúltivate thè háppiness òf màn

—must we not instantly discern in these different forms (though all sufficiently good *English*) the respective genius of each individual country? and that, notwithstanding the theoretical jargon of our modern cavillers, the principles of delivery as well as of composition were thoroughly understood and cultivated by the orators of Greece and Rome?

Let us not therefore, in the petulance of self-conceit, decry those admirable masters whom posterity must everlastingly revere; but let us rather, in the consciousness of our own deficiency, search out their innumerable beauties, and adopt them as the models for our imitation.

* The reader should recollect all the previously suggested requisites for execution, and should beware, too, of the over-extension of the final consonant *n* in the word "*man*." The upward slide with which I have terminated this passage in the second example, must need the powers and the judgement of a *master*, ordinary execution being inadmissible; nay, ludicrous. This slide, though marked like a *slur*, is *continuous:* two distinct intervals must *not* be struck.

† From minutest inquiry and repeated trials, I am strongly of opinion that the *emphatic* syllable was at all times in the Roman language the "acute." [I consider the *circumflex* equivalent.] Not so with the Grecian; its variety, in this respect, was almost infinite. Hence the Roman language was more lofty and strong—the Greek more musical.

[To be continued.]

[That alterations, from time to time, had taken place in the accentual system both of the Greeks and Romans, is very probable. In the foregoing *Greek* imitation I followed the instructions of Dionysius of Halicarnassus, taking it for granted (though perhaps incorrectly) that the last syllable of every

every word of two or more syllables over which the *grave* accent is set (as in the present instance the last syllable of Englànd) should be delivered in an elevated *monotone*.

I have thought it unnecessary to introduce the *circumflex*. To this character the *slide* must always have been attached; but whether the *acute*, properly so termed, should also have claimed, at all times, this privilege, it is impossible to decide: with *us* it is sometimes almost impracticable; our *English short* syllables very seldom admitting, under any circumstance, such graceful execution.

These general observations on accentual properties, together with the examples which I have given, will sufficiently obviate the necessity of harassing my reader with *detail:* Volumes would not suffice for the answering of every objection with which some modern critics and grammarians have perplexed themselves and others.]

XXV. *On the Astronomy of the Orientals.* *By* A Correspondent.

To Mr. Tilloch.

Sir, — It is much to be wished that we had more accurate information respecting the modern astronomy of the Orientals; particularly the Arabians, Persians, and the Hindoos; especially when it is considered how this noble science flourished among them in former ages, and still continues in practice, as sufficiently appears in their compositions on the subject, and from the collections of manuscripts imported into this and other countries of Europe. The superior advantages which the Oriental astronomers possess in regard of climate, in their observations of the celestial luminaries; the serenity and purity of their atmosphere; the extent of their horizon in the plains of the East, added to their own diligent practice, conduce, at least, to promise some profitable results common to the interest of the science and satisfactory to the learned of other countries. The theories which they hold, the tables they construct, and the instruments they use, are all necessary to be known before we can form a just estimate of their merits, and advancement in the knowledge of astronomy.

When perusing an inquiry of this sort, I accidentally met with some astronomical measures of time relating to the sun and moon, according to the calculations of the Hindoo astronomers, and by which the Bramins, Moguls, and other Mohammedans in India chiefly go, in the reckoning of time, as noticed by Mr. Fraser in his History of Nadir Shah, p. 2 of his book, which I found on examination so justly to conform to the measures in our more popular treatises of Astronomy, that I cannot but transmit the remarks, for the consideration of your astronomical readers, and here subjoin them:

The

The lunar year they reckon 354 days, 22 gurris, 1 pull. The solar year they reckon 365 days, 15 gurris, 30 pulls, 22½ peels, Indian time;—60 peels making 1 pull, 60 pulls 1 gurri, and 60 gurris 1 day. According to which the following table is constructed.

Peels.	Pulls.	Gurris.	English time.
2½	....	....	1 second
12½	....	....	5 sec.
25	....	....	10 sec.
37½	....	....	15 sec.
50	....	....	20 sec.
75	1¼	....	30 sec.
150	2½	....	1 minute
750	12½	....	5 min.
1500	25	....	10 min.
2250	37½	....	15 min.
3600	50	....	20 min.
4500	75	1¼	30 min.
9000	150	2½	1 hour
18000	300	5	2 hours
27000	450	7½	3 hours
36000	600	10	4 hours
45000	750	12½	5 hours
54000	900	15	6 hours
81000	1350	22½	9 hours
108000	1800	30	12 hours
216000	3600	60	1 day

From this table it appears that the Indian year, of 365 days, 15 gurris, 30 pulls, and 22½ peels, is equal to 365 days, 6 hours, 12 minutes, and 9 seconds of our time; and accords with our sidereal year nearly, which is stated at 365 days, 6 hours, 9 minutes, and 14½ seconds. The Indian lunar year, reckoned at 354 days, 22 gurris, 1 pull, measures 354 days, 8 hours, 48 minutes, 24 seconds, English time; which very nearly corresponds with that settled in our tables at 354 days, 8 hours, 48 minutes, 36 seconds.—See *Ferguson's Astronomy*, chap. xxi. art. 373.

The lunar cycle, or period of 19 years, as also that called the Chaldean or ecliptic period, confessedly originated with the Eastern astronomers:—and that we may see the agreement of the Oriental astronomers with our European calculators, I here insert the measure of 19 sidereal and lunar years after both accounts: thus,

Indian

Indian Time reduced.

	Days.	Hours.	Min.	Sec.
19 × 365 days =	6935	0	0	0
19 × 6 hours =	4	18	0	0
19 × 12 min. =	0	3	48	0
19 × 9 sec. =	0	0	2	51
Indian time ..	6939	21	50	51
Ferguson's Tables, p. 190,	6939	20	55	35½
Difference	....	..	54	15½

Hence the difference between the Indian and European is 54 min. 15½ sec. in 19 sidereal years.

Indian Lunar Years reduced.

	Days.	Hours.	Min.	Sec.
19 × 354 days =	6726	0	0	0
19 × 8 hours =	6	8	0	0
19 × 48 min. =	0	15	12	0
19 × 24 sec. =	0	0	7	36
19 × 12 lunations =	6732	23	19	36
6 lunations =	177	4	24	12
1 lunation =	29	12	44	2
235 lunations =	6939	16	27	50
Do. by English tables =	6939	16	26	51
Difference	....	..	..	59

The difference between 235 lunations composing the lunar cycle of 19 years by both reckonings less than one minute! This statement I hope is accurate so far as my documents go; and considering the supposed ignorance of the Eastern astronomers in the elements of true science, their want of necessary and accurate mathematical instruments, and the skill for the more profound and elaborate calculations of our European and justly famous practitioners, their determination on the exact measures of the sidereal and lunar year is truly admirable, and deserving our highest commendation.

It remains to inquire how such extraordinary agreement in calculations so intricate, and by observers so remotely distant, and unconnected, should coincide in the instances above given.

Hipparchus, who flourished about 140 years before the Christian æra, first discovered the sidereal year to exceed the solar or tropical year; and thence concluded, that the fixed stars had a slow

slow annual motion of their own; and Thebites, an Arabian, about the year of Christ 1200 determined the sidereal year at 365 days 15 prime scruples, or 6 hours and 23 second scruples, or 9 minutes 12 seconds, which agrees nearly with the English and French astronomers, or within about 12 seconds.—See *La Caille's Elements*, translated by Robertson, art. 471, p. 204. The Indian astronomers, who compute the same at 365 days 6 hours 12 min. 9 sec., do therefore exceed the truth by at least 3 minutes; which difference, although considerable, may possibly be reconciled if we were more accurately informed of the process of their calculations. I am, sir,

Yours most respectfully,

T. Y.

XXVI. *On the Performance of the Apollonicon, constructed by Messrs.* Flight *and* Robson.

To Mr. Tilloch.

Sir,—I beg leave to present to the notice of your readers some account of a musical instrument that has for some months past been exhibiting in St. Martin's Lane, the extraordinary powers of which reflect the highest credit on the ingenious inventors, and well deserve the attention of all who have a taste for music, or for mechanism, and who are anxious to encourage the successful efforts of genius;—I allude, sir, to the Apollonicon, an instrument invented by Messrs. Flight and Robson, organ-makers. I have at different times attended the performances on this instrument; and, in return for the great gratification I have received, shall be happy, if, through your permission, my humble efforts, in thus calling the attention of amateurs of music to it, through the medium of your valuable work, should tend in the least to promote the interest of those eminent artists. I understand, sir, that two or three instruments, on the same principle as the Apollonicon, but on a much smaller scale, were previously made by Messrs. F. and R. for different noblemen and gentlemen, about six or seven years ago, which they submitted to the inspection of their musical friends, from whom they met with the highest approbation. I had often heard of the extraordinary powers of those instruments, which were built for Lord Kirkwall and the Duke of Leinster, but from particular circumstances had never an opportunity of hearing them.

It was, I understand, from the flattering marks of approbation that were bestowed on those specimens of their abilities, that the inventors were induced to commence the magnificent instrument in question, which should combine the superior delicacy of expression

pression that those instruments possessed, with the grandeur of tone, which from their want of size they were incapable of. There is in Dr. Rees's *Cyclopædia*, under the article ORGAN, a copious description of the first instrument that was built for Lord Kirkwall, explaining its principles, construction, and effects, to which I should beg to refer your readers for an explanation of its mechanical properties.

I recollect seeing a prospectus, about five years ago, stating the object of the inventors in setting about to construct the Apollonicon, at a value of 10,000*l.*, under the sanction of His Royal Highness the Prince Regent, who having heard Lord Kirkwall's organ, had been pleased to bestow his unequivocal approbation and patronage to their efforts. The instrument was at that time commenced; and accordingly, after a period of five years of labour, expense and anxiety, the Apollonicon was opened for public exhibition in June 1817; since which time it has been heard by many thousands, who by their approbation have borne testimony to its merits.

This magnificent instrument is on the principle of the organ; is twenty-four feet in height, twenty feet wide, and about twelve feet deep, and contains in the whole about three thousand pipes; the largest of which, of wood, is sixteen feet long, by eighteen and twenty-one inches wide. By certain qualities and combinations of the different pipes, the effect of flutes, oboes, clarionets, bassoons, horns, &c. &c. is produced in a very superior style; the whole powers of which, with the variety of changes they are capable of, are acted upon by three immense cylinders or barrels of six feet each in circumference, impelled by a mechanical power: on which barrels are set, at present, the celebrated overtures to *Anacreon*, and *Clemenza di Tito*. The extraordinary precision, expression, brilliancy of execution, and the rapidity with which the instrument performs the different changes, in these two pieces, have astonished and delighted the scientific and musical world.

The Apollonicon possesses also the capability of being acted upon by performers; it has five sets of keys, on which five professors may play at the same time. The principal set, on which one performer may play, commands the power of a very grand organ, with a sweetness and expression superior to any thing I have ever before heard; combining the expressive quality of the violin, the sublimity of the organ, and the extreme delicacy of the musical glasses: the other sets of keys command the effects of the different wind instruments, as flutes, oboes, bassoons, &c. &c. The whole combined bring into play the full powers of the instrument. By a judicious arrangement of the different parts of a grand piece of music the finest effect may be produced by these

these five sets of keys, equal in grandeur to an orchestra of one hundred and fifty performers.

During the winter season, Messrs. Flight and Robson have given, at stated periods, a series of instrumental evening concerts on the instrument; for the conducting of which they have been so fortunate as to engage a professor, before not sufficiently known, Mr. Thomas Adams, but whose musical abilities are such as to rank him in the highest class of his profession. Under his direction, and accompanied by four other eminent professors, have been performed some of the finest selections of classic music from the compositions of Dr. Boyce, Purcell, Bach, Handel, Haydn, Mozart, Beethoven, &c. &c. The approbation with which these performances have been honoured by a numerous class of visitors evinces that the English, who have hitherto been considered not as a musical people, are perfectly capable of appreciating and admiring, to the fullest extent, real good music, when placed before them. The highest praise is due to the proprietors of the Apollonicon, for thus introducing a species of performance that by its superiority will tend to cultivate the rising musical taste; and I trust that the lovers of music will continue to render that encouragement and patronage to the instrument, in the next season, which its superior merits are so deservingly entitled to.

I am, sir,
Your obliged servant,
An Amateur.

XXVII. *An Account of Experiments for determining the Length of the Pendulum vibrating Seconds in the Latitude of London. By Capt.* Henry Kater, *F.R.S.*

[Continued from p. 100.]

Of the Apparatus and Methods employed for the Measurement of the Distance between the Knife Edges, and for the Comparison of the British Standard Measures of the highest Authority.

The microscopes used for this purpose were made by Mr. Thomas Jones, of Cockspur-street. They are both furnished with cross threads of spider's web, as well as with a single thread for the purpose of bisecting a dot if required, and are in other respects of a similar construction with those described by Sir George Shuckburgh Evelyn, in the Philosophical Transactions for 1798, but are more powerful, and the micrometer is capable of far greater precision.

The object-glass of the micrometer microscope is of one inch focus,

focus, the distance from the object-glass to the spider's threads 3·25 inches, the focus of the compound eye-glass rather less than one inch, the magnifying power 18 times. In the other microscope, which I shall call the fixed microscope, the object-glass is of three-quarters of an inch focus, and the magnifying power consequently greater. The micrometer head is divided into one hundred parts.

Each microscope slides in a tube, which is fixed in a plate of brass forming part of its support; and this plate moves in a dovetail, by which the microscope may be brought over the object to be viewed, when it is firmly clamped by a screw.

A piece of well-seasoned mahogany, four inches and three-quarters, by three inches, served as a beam to which the supports of the microscopes were screwed, their centres being 39·4 inches asunder.

Two screws with milled heads supported the extremities of the beam in front, and a piece projecting from the middle of the beam behind served as a third leg. By means of the screws, the focus of either microscope could be nicely adjusted at pleasure, without any risk of altering their distance from each other.

My first object was to ascertain the degree of precision of which vision is capable when assisted by the microscope. For this purpose, a very fine line was drawn on a polished piece of brass; and the microscope being carefully adjusted so as to be free from parallax, by causing the image of the line to bisect the angles formed by the spider's threads, moving the eye to the right and left and remarking whether the image changed its situation, and if it did, varying the distance of the microscope from the object accordingly, until the line appeared stationary, the micrometer screw was turned back, and the spider's threads brought up again till the angle formed by them appeared to be accurately bisected by the line. The division of the micrometer was then noted, and this was repeated several times with scarcely a sensible difference in the result; and thus I assured myself that no error worthy of remark was to be apprehended from imperfection of vision.

The next step was to determine the value of one division of the micrometer head. By the kind interest of Sir Joseph Banks, I was favoured with the use of the standard scale which belonged to the late Sir George Shuckburgh Evelyn, and which is described in the Philosophical Transactions for 1798. This scale, the work of Mr. Troughton, is second to none in the kingdom in point of accuracy of division, and is too well known to render any further remark necessary. The microscope being carefully adjusted for parallax, one inch, from the 39th to the 40th, was measured by successive tenths, and the mean taken as the value of of one-tenth of an inch. The measurement of the same inch was repeated ten times at different periods, the microscopes being previously adjusted anew each time for parallax. The mean results of such measurements are as follows:

Divisions of the Micrometer to 1-10*th of an Inch.*

2335·00	2335·75
2333·75	2338·30
2337·55	2335·85
2337·32	2337·85
2334·50	
2336·90	Mean 2336·277

Hence the value of one division of the micrometer appears to be $\frac{1}{23363}$ of an inch.

In the course of these measurements, differences occurred for which I was at a loss to account; but at length it appeared that they were to be attributed to remaining parallax; for, whatever care be taken in adjusting the microscope, it is scarcely possible to bring the image of the object precisely in the same plane with the threads, and the image will consequently be of various dimensions, according to its distance from this plane. Unless, therefore, the most minute attention be paid to the adjustment for parallax, the error arising from this cause will be considerable; and I may here remark, that I believe the difficulty of bringing the image into the plane of the threads, to be the source of by far the most serious errors to which measurements by means of microscopes are liable.

I had now to examine the quality of the threads of the micrometer screw. For this purpose, two fine lines were drawn near each other on a piece of brass; and the micrometer being turned back as far as it would go, the distance of the lines was carefully measured; and this was repeated, proceeding through the whole length of the screw, always advancing the micrometer one revolution previous to each successive measurement. The result of this severe test will best appear by giving the numbers themselves.

Divisions of the Micrometer.

502·0	502·5	501·0
501·5	502 0	502·5
501·0	502·0	501·0
502·0	502·5	500·0
501·5	501·0	500·0
502·0	501·5	500·5
502·0	502·0	Mean 501·5

The mean is 501·5, and the greatest difference from the mean only one division, amounting to $\frac{1}{23363}$ of an inch, a degree of accuracy

accuracy truly surprising, when it is considered that all errors of observation are included in this minute quantity.

Comparison of the different Standards.

The microscopes being placed at the distance of 39·4 inches, were advanced by single tenths, from zero of the scale through the space of two inches; and the mean of twenty measurements thus obtained being compared with the distance from zero to 39·4 inches, this last was found to be in defect 1·2 divisions of the micrometer, or ·00005 of an inch. And as this is the portion of the scale employed in ascertaining the distance between the knife edges, this difference must ultimately be subtracted to obtain the distance of the knife edges, in parts of the mean value of the scale*.

From the high importance which attaches to General Roy's scale, as having formed the basis of the Trigonometrical Survey of the kingdom, I was particularly desirous of comparing it with that of Sir G. Shuckburgh, in order that I might be enabled to give the length of the pendulum in parts of that standard which constitutes the foundation of one of the most important scientific operations ever carried on in this country. Fortunately, this scale was purchased at the sale of General Roy's effects by Mr. Browne, who readily confided it to my care. From the mean of a number of comparisons, I found the distance from zero to 39·4 of General Roy's scale, equal to 39·40144 of Sir G. Shuckburgh's standard †.

The standard yard made by Bird in 1758, for the House of Commons, better known by the name of Bird's Parliamentary standard, is little adapted for measurements where great precision is necessary. The yard is determined by two large dots made on gold pins which are let into a bar of brass. The mean of a num-

* From an examination of this scale by the late Sir G. Shuckburgh, it appears that the greatest liability to error is ·00033 of an inch, or, as corrected by Mr. Troughton, ·000165 of an inch, the chances against which are as 9 to 1.

† The very great difference between this result and that stated by Sir Geo. Shuckburgh, in the Philosophical Transactions for 1798, renders it necessary for me briefly to detail the manner in which the comparisons were made. The two scales were placed in contact, and remained thus for twenty-four hours; after which, sixteen comparisons were taken in the course of the day; but these were rejected in consequence of the temperature having increased six degrees during the operation. When the scales had been together forty-eight hours, sixteen other comparisons were made during two succeeding days, the thermometer remaining steadily at 70°. The greatest difference between any one of these last and the mean result, did not amount to four divisions of the micrometer. The mean of the first set of observations exceeded that of the last by ·00017 of an inch. Imagining that the difference between Sir George Shuckburgh's result and mine, might possibly

a number of bisections of these dots gave their distance equal to 36·00016 inches of Sir George Shuckburgh's scale.

Measurement of the Pendulum.

The pendulum was let into a solid piece of mahogany edgewise, to such a depth that the knife edges were about one-twentieth of an inch above its surface. To one end of the pendulum a common spring steelyard was attached by its hook, and a string being passed through the ring, and fastened to an upright piece of wood screwed to the end of the mahogany case, the pendulum was extended by a force rather greater than its own weight (about ten pounds), and consequently, no error (if any such were to be apprehended) could arise from a difference in the length of the pendulum in its vertical and horizontal positions.

The knife edges were fixed as nearly as could be done by mechanical means, at right angles to the bar of the pendulum; but the bar being flexible, they would most probably, when the pendulum was extended for the purpose of measurement, be found to be not precisely parallel to each other, and would consequently require some adjustment. To effect this, two opposite screws were passed through the sides of the mahogany case, so as to act in a transverse direction against that extremity of the pendulum which was next the steelyard; and the microscopes being brought over the extreme points of the knife edges, alternately on either side of the bar, the requisite parallelism was readily obtained by means of the screws, sufficient room having been left in the mahogany case for the very small motion of the extremity of the pendulum which might be found necessary. This arrangement is represented in Plate III. fig. 5.*

To obtain the distance between the knife edges, two different methods were used. For the first, four rectangular pieces of brass were prepared, about half an inch square. Very near to the perfectly straight edge of each, a fine line was drawn, to be viewed through the microscope, and these lines were each crossed at right angles by two others, intended to indicate that part of

possibly be occasioned by an error in the divisions bounding that part of General Roy's scale which I had employed, I compared it with various other portions, and found no greater difference than might have been expected from unavoidable imperfection of division. It is to be presumed then, that the error into which Sir George Shuckburgh appears to have fallen, must have arisen from the two scales not having been of the same temperature at the time they were compared, particularly as Sir George Shuckburgh's is by far the most massive of the two. I may here add, that last winter wishing to know whether the expansion of the two scales was equal, I roughly compared them together once, at the temperature of 33°, when it appeared that 42 inches on General Roy's scale, was equal to about 42·001 inches of Sir George Shuckburgh's standard.

* [This Plate will be given with a future Number.]

 the

the first line from which the measurements were to be taken. These pieces were marked A, *a*, and B, *b*.

The pieces A and *a*, being placed with their edges in contact, in which position they were kept by the pressure of a spring, the distance between the fine lines first drawn was carefully measured with the micrometer, and from a mean of eight observations, the greatest difference between which did not exceed one division, was found to be 329·09 divisions.

The same was done with the pieces B and b, and the distance of the lines from a mean of sixteen observations appeared to be 366·96 divisions.

The knife edges being adjusted as nearly as possible parallel to each other, the pieces A, a, and B, b, were placed in contact with those parts of the knife edges on either side of the bar, on which the vibrations were to be performed, and were retained in their places by the pressure of slight springs attached to the mahogany case.

The microscopes were now brought over the pieces A and a, so as for the lines before described to bisect the cross threads, when the division of the micrometer was noted.

The same was done with the pieces B and b; and the division of the micrometer was also registered.

The pendulum being removed, the standard scale* was placed beneath the micrometer; and its zero being made to bisect the angles of the fixed microscope, the cross threads of the micrometer microscope were brought to 39·4 of the scale, and the revolutions and parts of the micrometer were noted.

From these data, and the respective distances of the lines on A and a, and on B and b, when the pieces were in contact, the distance of the knife edges on either side of the bar may be readily obtained, and the mean being taken, will obviously correct any error arising from a want of perfect parallelism in the knife edges.

It is very generally believed that measurements from a knife edge, or from a line terminating a surface, are liable to much uncertainty from what has been called *irradiation*, or indistinctness of the image. But this is by no means the fact; for, if the reflection of light from the knife edge be prevented, and it be viewed on a white ground, it may be made to bisect the cross threads of the microscope, with nearly the same precision as could be attained by the use of a line. There is, however, a correction necessary to be applied in this case, and I shall proceed to describe the method employed for ascertaining its amount.

A slip of writing-paper was pasted on the mahogany case, under each knife edge, extending beyond it about the tenth of an inch,

* The scale constantly referred to is Sir George Shuckburgh's standard.

and

and adjoining was a piece of black paper to prevent the reflection of light on the knife edge from the surrounding objects. The knife edge now appeared through the microscope, as a well defined dark object on a white ground.

Marks were made on the paper close to the knife edges at equal distances on each side of the bar. These were intended to indicate those parts of the knife edges equally distant from the middle, from which the measurements were to be taken.

The knife edges being adjusted parallel to each other, in the manner before described, the microscopes were brought successively over such marks on the paper, as were at the same distance from the bar; and the mean of each pair of observations being referred to the scale, gave a distance of the knife edges free from any error which would be occasioned by a want of parallelism.

The knife edges bisecting the cross threads of the microscopes, pieces of black paper were slid beneath them, when they appeared to start forwards towards each other, the images continuing perfectly sharp and well defined.

The distance between the knife edges appeared to be now considerably less than before; and it remained to determine the difference, in order to apply its half, as a correction to the distance first obtained.

For this purpose, the reading of the micrometer was taken when the knife edges were viewed as dark objects on a white ground, and also when they were seen as light objects on a black ground. The difference of such readings will obviously give double the correction required. The results are contained in the following table.

Divisions of the Microm. the ground being *white*.	Divisions of the Microm. the ground being *black*.	Difference.
32·0	44·0	12·00
19·5	30·0	10·50
17·5	28·0	10·50
16·5	27·7	11·20
12·5	25·0	12·50
12·5	22·0	9·50
12·0	23·0	11·00
10·0	21·0	11·00
9·7	18·0	8·30
5·5	19·0	13·50
5·7	16·5	10·80
5·0	16·5	11·50
	Mean	11.03

From

From the above table it seems that 5·51 divisions (or ·000236 of an inch) are to be subtracted from the distance obtained when the knife edges are viewed as dark objects on a light ground; and on the contrary, the same quantity to be added when they are seen as light objects on a dark ground.

From the few experiments I have made, this quantity appears to be the same, whatever may be the relative illumination of the object and its ground, so long as the difference of character is preserved. On the cause of this extraordinary fact I can hazard no conjecture, and it remains an interesting subject for future investigation.

Of the Expansion of the Pendulum.

The composition of brass is so various, that probably no two specimens possess precisely the same rate of expansion. It became therefore necessary to determine the expansion of the pendulum by direct experiment, instead of adopting the conclusions of others, and for this purpose the following method was used. A trough of deal was made of a length sufficient to receive the bar intended for the pendulum, which was placed edgewise in the middle of the trough, being secured at one end by wedges on both sides. The bar was supported on small pieces of glass tube, serving as rollers to prevent friction, and the trough was of the same depth as the width of the bar.

Two transverse lines were drawn near the extremities of the edge of the bar, distant from each other 49·5 inches, and a third line was subsequently drawn one inch beyond. The microscopes were placed over the lines, and left, together with a thermometer, for twenty-four hours previous to the experiment.

The temperature being then registered, and the microscopes having been examined to see that the lines bisected the angles formed by the spider's threads, the trough was filled with hot water to the edge of the bar, and two thermometers were placed in it, one just beneath the surface of the water, and the other at the bottom of the trough. The bar rapidly expanded, and the line on it was followed by the micrometer till it became stationary. The bisection was then perfected, and the mean of the degrees shown by the thermometers registered, together with the number of revolutions and parts made by the micrometer. The whole was now suffered to remain till the temperature had become several degrees lower, when the contraction of the bar, occasioned by such decrease of temperature, was measured, and thus several successive observations were made, which are contained in the following table.

Distance

Highest Temp.	Lowest Temp.	Diff. of Temp.	Division of the Micrometer.	Expansion in parts of the length for each degree.
Distance between the lines on the bar, 49·5 inches.				
96°	43°	53°	620	·000010116
93	43	50	580	·000010030
Distance between the lines on the bar, 50·5 inches.				
91	43	48	600	·000010616
89	84	5	70	·000011890
83	75	8	89	·000009448
75	61	14	149	·000009038
80	44	36	400	·000009436
80	60	20	215	·000009129
73	60	13	152	·000009930
		Mean of the whole,		·000009959

The mean ·000009959 may be taken as the expansion of the pendulum in parts of its length due to a change of temperature of one degree of the thermometer.

Of the Method of deducing the Length of the Pendulum vibrating Seconds.

The distance between the knife edges was taken when the standard scale and the pendulum were both of the same temperature; and as this temperature did not differ considerably from 62°, the difference in the rate of the expansion (if any) between the pendulum and the scale may be neglected as perfectly insensible, and 62° be considered as the temperature of measurement.

The number of vibrations made by the pendulum in 24 hours, having been determined at a different temperature, the length of the pendulum will be greater or less as the temperature of observation exceeds or falls short of 62°; and by applying the expansion due to such difference of temperature, derived from the experiments contained in the preceding article, the distance of the knife edges, or length of the pendulum, will be known for the temperature at which the number of vibrations was determined, whence the length of the pendulum vibrating seconds may be readily deduced, the lengths of pendulums being to each other inversely in the duplicate ratio of the number of their vibrations in 24 hours.

Of the Correction for the Buoyancy of the Atmosphere.

The length of the pendulum thus found, differing from what it would have been had the vibrations been made in vacuo, it is necessary to apply to it a correction for the buoyancy of the atmosphere.

For this correction, the weight of the pendulum, compared with that of air, at the time of observation, must be known.

The pendulum being composed of different kinds of brass, the specific gravity of each part was carefully determined, and from thence the specific gravity of the whole mass.

Part of the Pendulum.	Weight in Air.	Specific gravity.
	lb.	
3 weights (cast brass)	3·14	8·417
4 knee pieces (cast brass)	3·13	7·816
Bar (plate brass)	3·30	8·532

From the above data, the specific gravity of the pendulum is 8·469; or the weight of the pendulum compared with water is as 8·469 to 1.

It has been determined by Sir George Shuckburgh (Phil. Trans. for 1777) that water is 836 times heavier than air, when the thermometer is at 53°, and the barometer at 29·27 inches. But the specific gravity of air varies directly as the height of the barometer, and inversely as its expansion, which is known to be $\frac{1}{480}$th part of its bulk for each degree of Fahrenheit: consequently, for any other state of the barometer and thermometer, the number 836 will vary *inversely* as the height of the barometer, and *directly* $\frac{1}{480}$th part for each degree of the thermometer above 53°.

Thus the specific gravity of water, compared with that of air, may be known for the temperature and altitude of the barometer at the time of observation; and multiplying this by the specific gravity of the pendulum, the ratio of the weight of the pendulum compared with that of air will be obtained.

This ratio will express the diminution of the force of gravity arising from the buoyancy of the atmosphere; and in order that the number of vibrations may be the same in vacuo as in air, the length of the pendulum must be increased in the proportion of this ratio to 1, the lengths of pendulums vibrating in the same time, varying directly as the force of gravity.

[To be continued.]

XXVIII. *On*

XXVIII. *On the very correct Notions concerning the Structure of the Earth, entertained by the Rev.* JOHN MICHELL, *as early as the Year* 1760; *and the great Neglect which his Publication of the same has received from later Writers on Geology; and regarding the Treatment of Mr.* SMITH, *by certain Persons. By Mr.* JOHN FAREY *Sen., Mineral Surveyor.*

To Mr. Tilloch.

SIR, — IN No. LVIII. of the "Edinburgh Review," published in February last, the Mineralogical Map and other Works of our deserving countryman *William Smith*, which are enumerated in p. 180 of your last volume, are reviewed, and in the historical sketch introductory to this Critique, the Writer has done an important service to the cause of literary justice and to science (for which I beg to tender him my thanks) in pointing the attention of Geologists, to a paper in the Philosophical Transactions for the year 1760, of which the Reviewer thus speaks (in p. 316): "But the most important observations, we think beyond comparison, that have ever yet* appeared on the subject of stratification, are those of the Rev. *John Michell*, in a paper "on the Cause and Phænomena of Earthquakes."

I take shame to myself, that it was not until very lately, in consequence of the above paragraph, and the extracts from Mr. Michell's paper, which follow in the Review, that I procured the volume of Philosophical Transactions referred to, and read the paper, here so justly characterized; especially as Mr. Bakewell, in the preface to his "Introduction to Geology" published in 1813, had said, "Mr. Michell was the first person who appears to have had any clear views respecting the structure of the external parts of the earth." My long neglect of this paper has altogether arisen, from the observations regarding *Earthquakes*, in all the accounts which I remember to have read on that subject, being too much involved with the terrific and the marvellous, to appear to furnish sufficient facts, for any one to reason safely upon, as to their cause: and not from any wish to undervalue, much less to conceal what Mr. Michell had done, in what I have occasionally written regarding the progress and authors of Geological discoveries: as will, I think, be conceded to me, by all impartial persons, who will turn to the three places in your work (viz. vol. xxxvi. p. 102; vol. xxxvii. p. 175 Note; and vol. xxxix. p. 94 Note), wherein I have mentioned my endeavours, to bring to light, and publish and explain, any papers on Geological subjects,

* This historical remark is introduced, between the account of what *Lehman* published in 1756, and *Whitchurst* in 1778.

jects, which Mr. Michell may have left behind him; but unfortunately for the science, his Son-in-law Sir Thomas Turton, Bart. has stated, that none such can be found:—The Rev. Gentleman at Cambridge, who enjoys Dr. Woodward's salary *for promoting Geological knowledge*, has not (like the worthy Baronet mentioned above) satisfied my inquiry, whether any papers relating to the order, thicknesses, &c. of the British Strata, were left by Mr. Michell in the Woodwardian Museum, at the time when he surrendered the charge of the same to a successor, many years ago: but I still hope, and so do many others, that the Reverend Gentleman will condescend to do so.

My object therefore now is, to request the favour of you, to reprint Mr. Michell's paper in your Magazine, and allow me to place at the foot of some of its pages, a few explanatory Notes, in the general confirmatory, from my own experience in widely and minutely exploring the British Strata, of the extraordinary correctness of his views of the leading Geological facts of our island, considering the period at which he wrote, when and for long after which, scarcely anything which is now found to be really valuable, as to the Earth's structure, was put forth to the public.

Here I cannot refrain from again adverting to the work of a late Oxford Professor, whose conduct towards my friend Mr. *Smith**, I have already censured, (although but in a small degree, com-

* The Gentleman alluded to in p. 173 of your last volume, as having called on Mr. Smith in November last, has since avowed himself to a common friend of Mr. Smith and myself, as the Author of the article in the Edinburgh Review, which is mentioned above: he took at the time a few desultory notes, of what Mr. Smith could state from memory, as to the dates and extent of his labours and discoveries, but he positively declined then to tell Mr. S. of *what particular purpose*, he was making such application to him; and he evaded the attending of a select meeting of Mr. Smith's friends, when such a Statement as that he had requested, was to be prepared and settled, after due deliberation: and when I pressed him at Sir Joseph Banks's meeting, to mention his *intended use* of the particulars which Mr. Smith had, at his instance, requested of me to assist in drawing up, he declined a direct answer, but insinuated, that it was *to defending Dr. Kidd*, his very particular Friend, from aspersions that had been cast on him, regarding Mr. Smith! I gave a firm, and I trust suitable answer which may perhaps at some time appear in your pages, and the matter dropped.

The above particulars may somewhat account, why, throughout the Critique in question, Doctor Kidd is so ostensibly brought forwards, and as it were pitted against *poor* Mr. Smith; why so many of the quotations, and references to Authors, are artfully made to bear (many of them most unfairly, as I shall perhaps state, when leisure permits) against Mr. Smith's claims, to almost any originality or merit; but the two last paragraphs explain the whole, and show, although less openly than Dr. K went about it, that the object in view has been (as Mr. Smith from the first suspected) to push into notice, those "men of *liberality* and scientific acquirements," and their Works, who are, as the Reviewer tells us, engaged in "correcting the investigations" of "an Englishman, untaught and *unassisted*," (see your vol. xlv. p. 296 Note): the Reviewer with great complacency concludes, by complimenting the persons of "intelligence and *liberality*" alluded commensurate with its demerits, in your xlvth volume, p. 338) in order to point out, that although he professed (but untruly) to review, and pass as it were his academical judgement, *on all that had previously been written* on the structure of the Earth, he never quotes or alludes to this paper of Mr. Michell's; not even in his chapter "on Volcanos and Earthquakes."

So in like manner, Mr. M. has received entire neglect from a London Professor, who made *the history of Geological labours and discoveries*, the subject of a Lecture, introductory to a Course

alluded to, on the "elevated place," their Names are hereafter to occupy in the page of fame!

What the character of that *fame* may be I will not inquire, which attaches to the having announced (in the name of the "learned *Body*," mentioned by the Reviewer) at the critical moment, after Mr. Smith had, through 12 years or more, been prevented from putting his Map to press, by no Map-seller seeing a prospect, through "*the unfashionableness* of the mode, in which he had taken up and treated the subject," of their being remunerated, for engraving, colouring, furnishing all the learned Universities, &c. *gratis*, and advertising his Map; and to the having actually followed up this announcement, by putting into the Engraver's hands, the pretended "*corrected* Map" of which now the Reviewer so complacently speaks (see vol. xlv. p. 337) months *before the original Map could be got out* by Mr. Cary: and through which new mode of proceeding in search of *fame*, or something else, it has occurred, and there is too much reason to perceive *exultation* in the Parties at it, that Mr. Smith and Mr. Cary, remain almost wholly *unrewarded for their labours!*

Speaking of the *Edinburgh* Review, brings me to mention, that there are now two rival *Magazines*, publishing monthly in that city, the new or opposition one, of which, is said (as is indeed pretty evident to any one) to have its scientific department edited by a certain professor of "Geognosy:" there is also published in London the Monthly Annals, of another "Geognost:" to the Editors of these two Geognostic Works (many of whose peculiar Readers, might fairly be supposed to have scarcely seen the name of Mr. Smith mentioned) were sent in the usual way, the very first manuscript copies of "Mr. Smith's Claims" (see vol. li. p. 174 to 180 which were any how circulated, with a respectful request from me, that they might have insertion, as a liberal act of literary justice to Mr. Smith. A printed Copy was also forwarded, as soon as possible, to the old Edinburgh Magazine; but, neither the Editor of the old or of the new Edinburgh Magazine has condescended to give it insertion!: and notwithstanding, that the opposition Editor had, in a number almost immediately preceding, amongst the literary news (and apparently from his own pen) inserted a vaunting kind of challenge to Mr Smith and myself *by name*, to make good or renounce, in his Work, the claim, which a certain Scotch Geognost had (*unasked*) repeatedly put in, in our behalfs, as being *rival discoverers* of "*the idea* of formations," with the idolized Werner!

The London Geognostic Annalists in the same foreign interest, raised at first a cavil, at the "Statement," not coming direct from Mr. Smith, (although *purporting* to come *from his friends*,) objecting *in toto* to the Notes attached to it: and when afterwards, Mr. Smith on his return to Town, attempted to satisfy these Qualms, by approving of the "Statement" under his hand, and requesting its insertion, *provided the Notes were suffered to accompany it:* these Editors, however, in pretending to comply, in their "introductory paragraph," having first (*untruly*) asserted, that all the Notes, referred merely to the *Philosophical Magazine*, they proceeded, amongst other wanton mutilations, to strike out the very material Note (vol. li. p. 179), that showed Mr. *Smith's name* and the *object of his labours*, had found a commendatory mention, in the Transactions of the *Royal Society!*

at

at the Royal Institution, and has in the last year published the same in an 8vo volume. The conduct of the gentleman now alluded to, has even been still worse towards my friend *Mr. Smith*, whose *Geological Map* has been in the Institution Library since its publication, and during the delivery of several of the Lectures alluded to, was actually *hung up at the back of the Lecturer*, and made the diagram of his local descriptions of English Mineralogy and Geology; and yet, in two volumes which this "learned Professor" has since put forth, in 1816 and 1817, detailing the Geological facts of England, *not the least mention or allusion is made to Mr. Smith or his labours*, through the last 28 years!!

If no one else can be found to stand forward, and condemn with just severity, such gross *literary injustice*, as is displayed in the instances above alluded to, I am the person who will fearlessly do so, as long as your work remains, as heretofore, the impartial vehicle of scientific communication; and I am, sir,

Your obedient servant,

Howland-street, Aug. 16, 1818. JOHN FAREY Sen.

Conjectures concerning the Cause, and Observations upon the Phænomena, of Earthquakes; particularly of that great Earthquake of the first of November 1755, *which proved so fatal to the City of Lisbon, and whose Effects were felt as far as Africa, and more or less throughout almost all Europe; by the Rev.* JOHN MICHELL, *M.A. Fellow of Queen's College, Cambridge.* [From the Philosoph. Transactions*.]

Introduction.

Art. 1. IT has been the general opinion of philosophers, that earthquakes owe their origin to some sudden explosion in the internal parts of the earth. This opinion is very agreeable to the phænomena, which seem plainly to point out something of that kind. The conjectures, however, concerning the cause of such an explosion, have not been yet, I think, sufficiently supported by facts; nor have the more particular effects, which will arise from it, been traced out; and the connexion of them with the phænomena explained. To do this, is the intent of the following pages; and this we are now the better enabled to do, as the late dreadful earthquake of the 1st of November 1755 supplies us with more facts†, and those better related, than any other earthquake of which we have an account.

2. That

* Read Feb. 28; March 6, 13, 20, 27, 1760.

† Many of these facts are collected together in the xlixth volume of the Philosophical Transactions. The same are also to be found, with some additional

2. That these concussions should owe their origin to something in the air, as it has sometimes been imagined, seems very ill to correspond with the phænomena. This, I apprehend, will sufficiently appear, as those phænomena are hereafter recounted; nor does there appear to be any such certain and regular connexion between earthquakes and the state of the air, when they happen, as is supposed by those who hold this opinion. It is said, for instance, that earthquakes always happen in calm still weather: but that this is not always so, may be seen in an account of the earthquakes in Sicily of 1693*, where, we are told, "the south winds have blown very much, which still have been impetuous in the most sensible earthquakes, and the like has happened at other times."

3. Other examples to the same purpose we have in an account of the earthquakes that happened in New England in 1727 and 1728; the author of which says, that he could neither observe any connexion between the weather and the earthquakes, nor any prognostic of them; for that they happened alike in all kinds of weather, at all times of the tides, and at all times of the moon †.

ditional ones, in "The History and Philosophy of Earthquakes," (a work well worth the perusal of those who are desirous of being acquainted with this subject). The author of it has given us, besides the aforesaid facts, a very judicious abridgement of ten of the most considerable writers upon the subject. I have taken the greatest part of my authorities either from this author, or the Philosophical Transactions, that those who would wish to examine them may have an opportunity of doing it the more easily: some things only, which were not to be met with in these, and which yet were necessary to my purpose, I have been obliged to seek for elsewhere.

* See Philos. Trans. No. 207; or vol. ii. p. 408.—Lowthorp's Abridgement.

† See Philos. Trans. No. 409; or vol. vi. part ii. p. 202.—Eames's Abridgement.—To these authorities we may add the opinion of Mons. Bertrand, who expresses himself, upon this occasion, in the following manner: "Aristotle, Pliny, and Seneca, tell us, that earthquakes are preceded by a calm and serene air. This is, indeed, often the case, but not always. I don't know, upon an examination of the whole, if there are not as many exceptions to this rule, as examples that confirm it. Some authors again have thought, that they might look on a dark sky, lightnings, and sudden storms, as the forerunners of earthquakes." Then relating some instances of shocks that happened in calm and serene weather, he adds, "On the other hand, it appears, from the examples which we have before related, that many earthquakes have happened at the time of great rains, violent winds, and with a cloudy sky; so that one cannot find any certain prognostic of them in the state of the atmosphere."—See *Memoires Historiques et Physiques sur les Tremblemens de Terre, par Mons. Bertrand, à la Haye* 1757. This author, in these sensible memoirs, has obliged the public with a circumstantial account of all the facts he could collect, relating to the earthquakes of Switzerland, or those of other places that seemed to be connected with them. The whole seems to be done with care and fidelity, and without the least attachment to any particular system.

4. If,

4. If, however, it should still be supposed, notwithstanding these instances to the contrary, that there is some general connexion between earthquakes and the weather, at the time when they happen, yet, surely, it is far more probable, that the air should be affected by the causes of earthquakes, than that the earth should be affected in so extraordinary a manner, and to so great a depth; and that this, and all the other circumstances attending these motions, should be owing to some cause residing in the air.

5. Let us then, rejecting this hypothesis, suppose that earthquakes have their origin under ground, and we need not go far in search of a cause, whose real existence in nature we have certain evidence of, and which is capable of producing all the appearances of these extraordinary motions. The cause I mean is subterraneous fires. These fires, if a large quantity of water should be let out upon them suddenly, may produce a vapour, whose quantity and elastic force may be fully sufficient for that purpose. The principal facts, from which I would prove that these fires are the real cause of earthquakes, are as follow:

SECTION I.—6. *First,* The same places are subject to returns of earthquakes, not only at small intervals for some time after any considerable one has happened, but also at greater intervals of some ages.

7. Both these facts sufficiently appear, from the accounts we have of earthquakes. The tremblings and shocks of the earth at Jamaica in 1692*, at Sicily in 1693*, and at Lisbon in 1755*, were repeated sometimes at larger, and sometimes at smaller intervals, for several months. The same thing has been observed in all other very violent earthquakes. At Lima†, from the 28th October 1746 to the 24th February 1747 (the time when the account of them was sent from thence), there had been numbered no less than 451 shocks, many of them little inferior to the first great one which destroyed that city.

8. The returns of earthquakes also, in the same places, at larger distances of time, are confirmed by all history. Constantinople, and many parts of Asia Minor, have suffered by them, in many different ages: Sicily has been subject to them, as far back as the remains even of fabulous history can inform us of: Lisbon did not feel the effects of them for the first time in 1755: Jamaica has frequently been troubled with them, since the English first settled there; and the Spaniards, who were there before, used to build their houses of wood, and only one story high, for fear of them: Lima‡, Callao, and the parts ad-

* See the accounts of these in the Philos. Trans.

† See Antonio d'Ulloa's Voyage to Peru, part ii. book i. ch. 7.

‡ See the place above quoted.

jacent,

jacent, were almost totally destroyed by them twice, within the compass of about sixty years, scarce any building being left standing, and the latter being both times overflowed by the sea: nor were these the only instances of the like kind which have happened there: for, from the year 1582 to 1746, they have had no less than sixteen very violent earthquakes, besides an infinity of less considerable ones; and the Spaniards, at their first settling there, were told by the old inhabitants, when they saw them building high houses, that they were building their own sepulchres*.

9. *Secondly*, Those places that are in the neighbourhood of burning mountains, are always subject to frequent earthquakes; and the eruptions of those mountains, when violent, are generally attended with them.

10. Asia Minor and Constantinople may be looked upon as in the neighbourhood of Santerini. The countries also about Ætna†, Vesuvius, Mount Hæcla, &c. afford us sufficient proofs to the same purpose. But, of all the places in the known world, I suppose, no countries are so subject to earthquakes, as Peru ‡, Chili, and all the western parts of South America; nor is there any country in the known world so full of volcanos: for, throughout all that long range of mountains, known by the name of the Andes, from 45 degrees south latitude to several degrees north of the line, as also throughout all Mexico, being about 5000 miles in extent, there is a continued chain of them §.

11. *Thirdly*, The motion of the earth in earthquakes is partly tremulous, and partly propagated by waves, which succeed one another sometimes at larger and sometimes at smaller distances; and this latter motion is generally propagated much further than the former.

12. The former part of this proposition wants no confirmation: for the proof of the latter, viz. the wave-like motion of the earth, we may appeal to many accounts of earthquakes: it was very remarkable in the two which happened at Jamaica in 1687-8 ‖ and 1692 ‖. In an account of the former, it is said, that a gen-

* What is here said, is taken from d'Ulloa's Voyage to Peru, the History and Philosophy of Earthquakes, the Philos. Trans. &c., where many more examples, to the same purpose, are to be met with. See also *Memoires sur les Tremblemens de Terre;* in which are mentioned above 130 repetitions of earthquakes that have happened within the compass of 960 years in Switzerland.

† See many instances of this in vol. ii. of Lowthorp's Abr. of the Philos. Trans.

‡ Mons. Bouguer says, that scarce a week passes without earthquakes in some part of Peru.—See Hist. of Earthq. p. 205.

§ See the Maps of these countries, Condamine's Voyage down the Maranon, Acosta's Nat. Hist. of the Indies, &c.

‖ See Phil. Trans. No. 209; or vol. ii. Lowthorp's Abridgement, p. 410.

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tleman there saw the ground rise like the sea in a wave, as the earthquake passed along, and that he could distinguish the effects of it, to some miles distance, by the motion of the tops of the trees on the hills. Again, in an account of the latter, it is said, "the ground heaved and swelled, like a rolling swelling sea," insomuch that people could hardly stand upon their legs by reason of it.

13. The same has been observed in the earthquakes of New* England, where it has been very remarkable. A gentleman giving an account of one, that happened there the 18th November 1755, says, the earth rose in a wave, which made the tops of the trees vibrate ten feet, and that he was forced to support himself, to avoid falling, whilst it was passing.

14. The same also was observed at Lisbon†, in the earthquake of the 1st November 1755, as may be plainly collected from many of the accounts that have been published concerning it, some of which affirm it expressly: and this wave-like motion was propagated to far greater distances than the other tremulous one, being perceived by the motion of waters, and the hanging branches in churches, through all Germany, amongst the Alps, in Denmark, Sweden, Norway, and all over the British isles.

15. *Fourthly*, It is observed in places which are subject to frequent earthquakes, that they generally come to one and the same place, from the same point of the compass. I may add also, that the velocity with which they proceed (as far as one can collect it from the accounts of them) is the same; but the velocity of the earthquakes of different countries is very different.

16. Thus all the shocks that succeeded the first great one at Lisbon in 1755, as well as the first itself, came from the northwest‡. This is asserted by the person who says he was about writing a history of the earthquakes there: all the other accounts also confirm the same thing; for what some say, that they came from the north, and others, that they came from the west, cannot be looked on as any reasonable objection to this, but rather the contrary. The velocity also, with which they were all pro-

* See Philos. Trans. vol. l. p. 1, &c.

† See the accounts collected together, in the xlixth volume of the Philos. Trans., or in Hist. and Philos. of Earthq. and particularly p. 315, where it is said, "A most dreadful earthquake shook, by short but quick vibrations, the foundations of all Lisbon; then, with a scarcely perceptible pause, the nature of the motion changed, and every building was tossed like a waggon driven violently over rough stones, which laid in ruins almost every house, church, &c."

For the wave-like motion at Oporto, see Phil. Trans. vol. xlix. p. 418: for the same at Gibraltar, see Hist. and Philos. of Earthq. p. 322.

‡ See Philos. Trans. vol. xlix. p. 410.

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pagated, was the same, being at least equal to that of sound; for they all followed immediately after the noise that preceded them *, or rather the noise and the earthquake came together: and this velocity agrees very well with the intervals between the time when the first shock was felt at Lisbon, and the time when it was felt at other distant places, from the comparison of which it seems to have travelled at the rate of more than twenty miles per minute †.

17. An historical account of the earthquakes which have happened in New England ‡, says, that, of five considerable ones, three are known to have come from the same point of the compass, viz. the north-west: it is uncertain from what point the other two came, but it is supposed that they came from the same with the former. The velocity § of these has been much less than that of the Lisbon earthquakes: this appears from the interval between the preceding noise, and the shock, as well as from the wave-like motion before mentioned.

18. All the greater earthquakes, that have been felt at Jamaica ||, seem, by the accounts given of them, to have come from the sea, and, passing by Port-Royal, to have gone northwards. The velocity of these also was far short of the velocity of the Lisbon earthquakes.

19. The earthquake of London ¶, on the 8th of March 1750, was supposed to move from east to west. I have been credibly informed, that the same thing happened in a slight shock which was felt there in the last century, as the person who told me this had an opportunity of observing; for, being, by accident, in a scalemaker's shop at the time when it happened, he found that all the scales vibrated from east to west.

20. All the shocks that have been lately felt at Brigue in Valais have likewise come from the same point of the compass, viz. the south **.

21. *Fifthly*, The great Lisbon earthquake has been succeeded by several local ones since, the extent of which has been much less.

* See Philos. Trans. vol. xlix. p. 414; or Hist. and Philos. of Earthq. p. 315. † See art. 97. ‡ See Philos. Trans. vol. l. p. 9.

§ As in some earthquakes the velocity with which they are propagated is much less than in others, it is evident that they can by no means be owing to any cause residing in the air: for any shock communicated to the air must necessarily move with a velocity neither greater nor less than that of sounds; that is, at the rate of about thirteen miles per minute.

|| See the accounts of them in Philos. Trans. No. 209; or vol. ii.—Lowthorp's Abr. p. 410, &c.

¶ See Hist. and Philos. of Earthq. p. 250; or Philos. Trans. vol. x.—Martyn's Abr. Meteorology, passim.

** See Philos. Trans. vol. xlix. p. 620. The same has been observed at Smyrna also, see Philos. Trans. No. 195; or Martyn's Abr. vol. x. p. 526.

22. Such

22. Such were the earthquakes in Switzerland; those on the borders of France and Germany; those in Barbary, &c.*

Section II.—23. How well soever these facts may agree with the supposition before laid down, That subterraneous fires are the cause of earthquakes, one doubt, however, may perhaps remain; viz. how it is possible that fires should subsist, which have no communication with the outward air? In answer to this, I might allege the example of green plants, which take fire by fermentation, when laid together in heaps; where the admission of the outward air is so far from being necessary, that it will effectually prevent their doing so. But, to pass by this, we have many instances more immediately to the purpose.

24. It can hardly be supposed, that the fires of the generality of volcanos receive any supply of fresh air (for this must effectually be prevented by that vapour, which is continually rushing out at all their vents), and yet they subsist, and frequently even increase, for many ages. Now, these are fires of the very same kind with those which I suppose to be the cause of earthquakes. Other facts, still more expressly to the purpose, are as follow:

25. In the earthquake of the 1st of November 1755, we are told that both smoke and light flames were seen on the coast of Portugal, near Colares; and that, upon occasion of some of the succeeding shocks, a slight smell of sulphur was perceived to accompany a "fog, which came from the sea, from the same quarter whence the smoke appeared †."

26. In an account of an earthquake in New England, it is said, that at Newbury, forty miles from Boston, the earth opened and threw up several cart loads of sand and ashes; and that the sand was also slightly impregnated with sulphur, emitting a blue flame when laid on burning coals ‡.

27. One of the relaters of the earthquake in Jamaica in 1692 has these words: "In Port-Royal, and in many places all over the island, much sulphureous combustible matter hath been found (supposed to have been thrown out upon the opening of the earth), which, upon the first touch of fire, would flame and burn like a candle. . . . .

28. "St. Christopher's was heretofore much troubled with earthquakes, which, upon the eruption there of a great mountain of combustible matter, which still continues, wholly ceased, and have never been felt there since §."

29. Again, we are told, that, on the 20th November 1720,

* See the accounts of these collected together in Philos. Trans. vol. xlix; or in the Hist. and Philos. of Earthq.

† See Philos. Trans. vol. xlix. p. 414, &c.

‡ See Philos. Trans. No. 409; or vol. vi. part ii. p. 201.—Eames's Abr.

§ See Philos. Trans. No. 209; or vol. ii. p. 418.—Lowthorp's Abr.

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a burning island * was raised out of the sea, near Tercera, one of the Azores, at which place several houses were shaken down by an earthquake which attended the eruption of it. This island was about three leagues in diameter, and nearly round; from whence it is manifest, that the quantity of pumice stones and melted matter, which must have been requisite to form it, was amazingly great: in all probability it must have far exceeded all that has been thrown out of Ætna and Vesuvius together, within the last two thousand years. This may serve to satisfy us, that the fire which occasioned all this must have subsisted for many years, not to say ages, and this without any communication with the external air. It is worth observing, that several instances of this kind have happened amongst the Azores†. There are, besides many marks of subterraneous fires about these islands, several places sending up smoke or flames. These islands are also subject to violent and frequent earthquakes.

30. We have more instances to the same purpose, near the island of Santerini in the Archipelago, where there have been several little islands raised out of the sea by a submarine volcano. The eruption of one of these in the year 1708, with all the circumstances that attended it, we have a very good account of in the Philosophical Transactions‡. It was raised in a place where the sea had been formerly 100 fathoms deep, and was attended with earthquakes before it showed itself above water, as well as after. It is reported, that the island of Santerini itself was originally raised out of the sea in the same manner; but, be that as it will, we have certain accounts of new islands raised there, or additions made to the old ones, from time to time, for above 1900 years backwards, and there have always been earthquakes at the time of these eruptions.

31. Another example of the same kind happened at Manila §, one of the Philippine islands, in the year 1750. This also was attended with violent earthquakes, to which that island, as well as the rest of the Philippines, is very much subject.

32. We may add to these, the many instances of vast quantities of pumice stones ‖ which have been sometimes found floating upon the sea, at so great a distance from the shore, as well as from any known volcano, that there can be little doubt of their being thrown up by fires subsisting under the bottom of the ocean.

* See Philos. Trans No. 372; or vol. vi. part ii. p. 203.—Eames's Abr.

† See Hist. and Philos. of Earthquakes, under the titles Azores, Islands raised, &c.

‡ See No. 314, 317, and 332; or vol. v. p. ii. p. 196.—Jones's Abr.

§ See Philos. Trans. vol. xlix. p. 459.

‖ See Philos. Trans. No. 372; or vol. vi. part ii. p. 204, and No. 402; or vol. vii. part ii. p. 43.—Eames's Abr.

 33. From

33. From these instances, we may, with great probability, conclude, that the fires of volcanos produce earthquakes: I do not, however, suppose, that the earthquakes, which are frequently felt in the neighbourhood of volcanos, are owing to the fires of those volcanos themselves; for volcanos, giving passage to the vapours that are there formed, should rather prevent them, as in the instance at St. Christopher's, before mentioned.

34. We also meet with frequent instances confirming the same thing amongst the Andes. Antonio d'Ulloa (speaking of what happens amongst these mountains) says, "Experience shows us, that, upon the fresh breaking out of any volcano, it occasions so violent a shock to the earth, that all the villages which are near it are overthrown and destroyed, as it happened in the case of the mountain Carguayraso*. This shock, which we may, without the least impropriety, call an earthquake, is seldom found to accompany the eruptions, after an opening is once made; or, if some small trembling is perceived, it is very inconsiderable; so that, after the volcano has once found a vent, the shocks cease, notwithstanding the matter of it continues to be on fire." The greater earthquakes, therefore, seem rather to be occasioned by other fires, that lie deeper in the same tract of country; and the eruptions of volcanos, which happen at the same time with earthquakes, may, with more probability, be ascribed to those earthquakes, than the earthquakes to the eruptions—whenever, at least, the earthquakes are of any considerable extent. If this don't appear sufficiently manifest at present, it will, perhaps, be better understood, by applying to the present purpose, what will be said hereafter concerning local earthquakes.

SECTION III.—35. It may be asked, perhaps, why we should suppose, that several subterraneous fires exist in the neighbourhood of volcanos? In evidence of this, we have frequent instances of new volcanos breaking out in the neighbourhood of old ones: Carguayraso, just mentioned, may supply us with one example to this purpose; and in the night of the 28th of October

* It does not appear altogether certain, from the expression made use of in the French translation (from whence I have taken this), that Carguayraso might not have been a volcano in former times, which is asserted to have been the case by Mons. Condamine. It is possible also, that the same may be true of those four mentioned in the next article; and, indeed, it is difficult to know it to be otherwise, in any instance, among the Andes, where the volcanos are generally found at inaccessible heights. But allowing that all these were only old volcanos, which broke out afresh, yet they will serve at least to swell the number of them in the same neighbourhood, as well as to show us, that there may, very probably, be many more, which lie hid: for these showed no marks of their existence, till, by their eruption, they melted a vast quantity of snow, with which they were before covered, and which, being reduced to water, did great damage, by overflowing the country round about.

1746,

1746, in which Lima and Callao were destroyed, no less than four new ones burst forth in the adjacent mountains*.

36. To the same purpose, we may allege the instances of many volcanos lying together in the same tract of country: as for example, the many places, "not so few as forty," amongst the Azores, which either do now or have formerly sent forth smoke and flames; the many volcanos also amongst the Andes, already mentioned: thus Ætna, Strombolo, and Vesuvius, I may add Solfatara too, are all in the same neighbourhood: and Mons. Condamine says, he has traced lavas†, exactly like those of Vesuvius, all the way from Florence to Naples. In Iceland‡ also, we have, besides Hæcla, not only several other volcanos, but also a great number of places, that send up sulphureous vapours. But the examples of this kind are so frequent, that there are few instances to be produced of single volcanos, without evident marks, either that there have been others formerly in their neighbourhood, or that there are, at present, subterraneous fires near them.

[To be continued.]

XXIX. *Experiments on Muriatic Acid Gas, with Observations on its Chemical Constitution, and on some other Subjects of Chemical Theory.* By John Murray, *M.D. F.R.S. E. Fellow of the Royal College of Physicians of Edinburgh.*

[Continued from p. 112.]

Another form of experiment occurred to me still more direct and simple, that of transmitting muriatic acid in its gaseous form over ignited metals. If water be obtained in this experiment, it is a result which would prove subversive of the new doctrine; for muriatic acid gas is held to be the real acid, free from water, and the only change which can happen, is that of the metal decomposing the acid attracting its chlorine and liberating its hydrogen. And the experiment is further free from the only resource which remained to the advocates of that doctrine, in the case of water being obtained from muriate of ammonia, that it might be derived from the decomposition of the elements of ammonia, regarding it as an alkali containing oxygen. If water were really obtained from the combination of muriatic acid and ammoniacal gases, it would rather indicate, it was said, the de-

* See d'Ulloa's Voyage to Peru, part ii. book i. chap. 7.

† See Phil. Trans. vol. xlix. p. 624. All these lavas, as well as the volcanos just mentioned, lie in a continued line. The same thing holds good in the volcanos of the Andes also. This is a fact I must desire the reader to attend to, as it serves to confirm a very material doctrine, which I shall have occasion to mention hereafter. See art. 44, 45, and 46.

‡ See Horrebow's Natural History of Iceland.

N 2 composition

composition of nitrogen than the existence of water as a constituent of muriatic acid. No weight, I believe, is due to such an assumption; but if any importance were attached to it, it is precluded if water is obtained from the action of metals on muriatic acid gas.

I have executed the experiment in several forms; and in all with a more or less satisfactory result.

One hundred grains of iron filings, clean and dry, were strewed for a length of five or six inches, in a glass tube which was placed in an iron case, across a small furnace, so as to admit of being raised to a red heat. This tube, of about two feet in length, was connected with a wide tube eight inches long, containing dry and warm muriate of lime; and this was further connected, at its other extremity, with a retort affording muriatic acid gas, from a mixture of supersulphate of potash and muriate of soda. The open extremity of the long tube, dipped by a slight curvature in quicksilver. On the iron being raised to ignition, and the transmission of the acid gas being conducted slowly, elastic fluid escaped from the extremity of the tube, which was found to be hydrogen; and though no trace of moisture appeared in the anterior part of the tube, it immediately condensed in that part which was cold, beyond the iron filings. This accumulated in globules, and at length ran into a small portion in the bottom; the sides were bedewed for a length of six inches, and a thin film of moisture appeared beyond, nearly its whole length.

By the muriatic acid gas being extricated in the preceding experiment from nearly dry materials, and by its previous transmission over an extensive surface of loose muriate of lime, it was inferred, that it would be free from hygrometric vapour; and that it held no moisture, was apparent from no trace of it appearing in the anterior portion of the tube. To obviate, however, entirely, any supposed fallacy from this source, the experiment was performed in the following manner. One hundred grains of clean and perfectly dry iron filings were put into a long glass tube, which was placed, as before, across a small furnace. Muriatic acid gas had been kept in contact with dry muriate of lime for three days, in a jar with a stopcock adapted to it. This was connected, by a short tube with a caoutchouc collar, with the tube containing the iron filings; and a little of the muriatic acid gas being passed through the tube to expel the air, the temperature was raised to ignition. The slow transmission of the gas was continued by the pressure of the mercury in the quicksilver trough, and fresh quantities, which had been equally with the other exposed to muriate of lime, were added, as was necessary. Water almost immediately appeared in the tube beyond the iron filings, it collected in spherules, and continued to accumulate as the

the gas continued to be transmitted for a length of about seven inches. A portion of the gas which escaped from the extremity was clouded, and deposited a film of moisture on the sides of the jar in which it was received over quicksilver. The quantity of gas transmitted amounted to about thirty-five cubic inches.

There are some difficulties in conducting the experiment in the manner now described, from the consolidation of the metallic matter, and the volatilization of the product. It was also of some importance to vary the experiment. I therefore performed it in another mode. Metals scarcely act on muriatic acid gas at natural temperatures; but from such a degree of heat as could be applied by a small lamp, both iron and zinc were acted on; the gas suffered diminution of volume, hydrogen was formed, and a sensible production of moisture took place. The simplest mode of exhibiting this, is to introduce iron or zinc filings, previously dry, and warm, into a retort fitted with a stopcock; exhausting it; then admitting dry muriatic acid gas; and applying heat, by a small lamp, to the filings in the under part of the body of the retort. Moisture soon appears at its curvature in small globules, and increases on successive applications of the heat with the admission of the requisite quantities of gas.

To conduct the experiment, however, on a larger scale, I employed a different apparatus. A tubulated retort, of the capacity of twenty-five cubic inches, was connected with a jar, containing muriatic acid gas in contact with muriate of lime, on the shelf of the mercurial trough, by a tube bent twice at right angles, and fitted by its shorter leg with a collar of caoutchouc to a stopcock at the top of the jar, its longer leg passing into the tubulature of the retort, so as to terminate within an inch of its bottom, and the joinings being rendered air-tight. The retort is so placed, that heat can be applied by a lamp to the bottom, and its neck dips, by a short curved tube, under a jar filled with quicksilver, which, by the reverted position of the retort, may be placed beside the other, on the shelf of the trough. At the commencement of the experiment, the metallic filings, previously dry and warm, having been put into the retort, the atmospheric air is expelled by a moderate heat, and small portions of the muriatic acid gas are admitted, until the retort is filled with the pure gas. The stopcock is then closed, and heat is applied by a lamp to the bottom of the retort, under a considerable pressure of mercury; any small portion of gas, expelled at the extremity, being received in the small jar. The heat can thus be successively cautiously applied, and this, as the experiment proceeds, to a greater extent, in consequence of the diminution of volume that takes place. Fresh quantities of muriatic acid gas are admitted from time to time from the jar; and the stopcock being closed

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when the heat is applied, the hydrogen gas produced is expelled, with any muriatic acid gas not acted on.

In the principal experiment I employed, zinc filings were used in preference to iron, from the consideration, that muriate of zinc is less volatile than muriate of iron, and therefore would admit of a higher heat being applied to expel any water. One hundred grains of clean and dry zinc filings were introduced, while warm, into the retort; the air was expelled, and muriatic acid gas was admitted from the jar. On applying heat to the zinc, the retort, which was before perfectly dry, was bedimmed with moisture at its curvature, and small spherules collected at the top of the neck. These increased in size, and extended further as the experiment advanced. After a certain time, part of this disappeared in the interval of cooling, being absorbed by the deliquescent product; but when the heat was again applied, it was renewed, and this in increased quantity, until at length, at the end of four days, during which heat had been frequently applied, the whole tube of the retort, seven inches in length, was studded with small globules of fluid. When the heat had been raised high, a beautiful arborescent crystallization appeared in a thin film on the body of the retort, but no part of this reached the neck. The retort was now detached; the gas it contained was withdrawn by a caoutchouc bottle; a small receiver was adapted; and a slight heat having been applied, to expel a little of the air, the joining was made close by cement. The receiver was surrounded with a freezing mixture, and heat was applied by a choffer to the retort, as far as could be done without raising dense vapours. Globules of liquid, perfectly limpid, collected pretty copiously towards the middle and lower part of the neck, and the receiver, on being removed from the freezing mixture, was covered internally with a film of moisture. The globules in the neck of the retort were absorbed by a slip of bibulous paper, and the quantity was found to amount to 1·2 gr. The receiver being dried carefully, and weighed, lost by the dissipation of the moisture within, 0 4 grain. Distilled water, in which the bibulous paper was immersed, was quite acid; it gave no sensible turbidness on the addition of ammonia, or of carbonate of soda, and held dissolved, therefore, merely pure muriatic acid. The mass in the retort was of a gray colour, with metallic lustre, in loosely aggregated laminæ, somewhat flexible. It weighed 114·8 grains. Adding to this increase of weight, which the zinc had gained, the weight of the water and the hydrogen gas expelled, it gives a consumption of muriatic acid gas of about 16·8 grains, equivalent to about 43 cubic inches. Supposing the weight of water to be doubled, or nearly so, by saturation with muriatic acid, this gives the product of water in the

the experiment, as equal to nearly one grain; or about one-fifth of the whole quantity of combined water, which muriatic acid gas is calculated to contain*.

In all the preceding experiments, water has been procured from muriatic acid gas. It is obvious, that such a result cannot be accounted for on the hypothesis, that it is the real acid free from water, a compound merely of chlorine and hydrogen. On the opposite doctrine, as muriatic acid in its gaseous form is held to contain water, it may be supposed to afford a portion of it.

It may be maintained, however, in this, as it was in the experiment of obtaining water from the muriate of ammonia by heat, that the water produced is derived from hygrometric vapour in the gas. To obviate this, it is sufficient to recur to the fact established by the experiments of Henry and Gay Lussac, that muriatic acid gas contains no hygrometric vapour; and to the obvious result in the experiment, that no quantity that can be assumed, would be adequate to account for the quantity actually obtained. The circumstances of the experiment, too, are such as to preclude any such supposition; and this more peculiarly so, than in the experiment of obtaining water from the muriate of ammonia by heat; for, in the present case, the acid is alone employed, while in the other there is an additional equal volume of ammoniacal gas, which may be supposed to afford a double quantity of hygrometric vapour. In the latter, both the gases are condensed into a solid product, and any hygrometric vapour may be supposed to be liberated; but in the present experiment, there remains the hydrogen gas, capable of containing hygrometric vapour, while the muriatic acid gas contains none;

* The action of the metals on the muriatic acid gas taking place in the above experiments at a heat comparatively moderate, it occurred to me, that they might exert a similar action with no higher heat on the acid, in muriate of ammonia, and that this might afford an easy mode of exhibiting the results. I accordingly found, that on mixing different metals with sal ammoniac in powder, previously exposed to a subliming heat, and exposing the mixture to heat by a lamp, so regulated as to be short of volatilization, the salt was decomposed, ammoniacal gas was expelled, and moisture condensed in the neck of the retort; covering a space of several inches with small globules, and at length running down. The metals I employed were iron, zinc, tin, and lead; 100, 150, or 200 grains of each metal, dry, and warm, being mixed with 100 grains of the salt, likewise newly heated. To obviate any fallacy from common sal ammoniac being employed, I repeated the experiment with the salt formed from the combination of its two constituent gases, and obtained the same result. But although this affords an easy mode of exhibiting the production of water, it is not favourable to obtaining a perfect result, the heated ammoniacal gas carrying off a considerable portion of the water deposited; and accordingly, the quantity, instead of increasing as the experiment proceeds, at length diminishes, and the ammoniacal gas deposits a portion of water in passing through mercury, or in being conveyed through a cold tube.

 and

and the quantity of it thus transmitted over the humid surface, and expelled from the apparatus, must have carried off more vapour than the other, introduced at a lower temperature, could have conveyed. These circumstances, independent of the quantity of water deposited, precluded the supposition of any deposition from the condensation of hygrometric vapour. And there is no other external source whence it can be derived. In this respect nothing can be more satisfactory than the experiment with the zinc in the apparatus described. The muriatic acid gas rises from dry mercury in contact with muriate of lime, passes through a narrow bent tube, thirty inches in length, without exhibiting the slightest film of moisture, is received into the retort perfectly dry; and when the action of the metal on it is excited by heat, humidity immediately becomes apparent in the curvature of the retort, and this even while the gas is warm, and of course capable of containing more water dissolved, than it could do in its former state; and the quantity increases as the experiment proceeds. No arrangement can be supposed better adapted to prove, that any deposition of water must be by separation from its existence in the gas in a combined state.

But though I consider this conclusion as established, there is a considerable difficulty attending the theory of the experiment. The result of water being obtained is actually different from what is to be looked for, on the doctrine of muriatic acid gas containing combined water; and even when the fact is established, the theory of it is not easily assigned. On that doctrine, it must be held that in the action of metals on muriatic acid gas, the metal attracts oxygen from the water, the corresponding hydrogen is evolved, and the oxide formed combines with the real acid. No water, therefore, ought to be deposited, for none is abstracted from the acid, but what is spent in the oxidation of the metal. This will be apparent, by attending to the proportions in a single example, from the scale of chemical equivalents: 100 grains of iron combine with 29 of oxygen, and in this state of oxidation unite with 99 of real muriatic acid. This quantity of acid exists in 131·8 of muriatic acid gas, combined with 32·8 of water; and this portion of water contains 29 of oxygen with 3·8 of hydrogen. There is present, therefore, exactly the quantity of oxygen which the metal requires to combine with the acid; and no water remains above this. Or it may be illustrated under another point of view. Muriatic acid gas is composed of oxymuriatic gas and hydrogen. A metal acting on it must attract the oxymuriatic acid,—that is, the muriatic acid and oxygen,—and liberate the hydrogen. No water, therefore, ought to appear, more, on this theory, than on the other; but the real products in both must be a dry muriate, or chloride, and hydrogen gas,

gas. In the action of ignited metals on muriate of ammonia, it is equally evident, on the same principle, that no water ought to be obtained. How, then, is the production of water to be accounted for?

Though the water obtained in these experiments cannot be derived from hygrometric vapour in the gas, there is another view under which it may be regarded as present, as an adventitious ingredient. The acid having a strong attraction to water, may be supposed, in the processes in which it is usually prepared, to retain a portion not strictly essential to its constitution as muriatic acid gas, but still chemically combined,—that is, combined with it with such an attraction as to be liberated only when it passes into other combinations, and it may be this portion which is obtained in the action of metals on the gas; the other portion, that essential to the acid, being sufficient to produce the requisite oxidation of the metal.

The question with regard to the existence of water in this state, Gay Lussac and Thenard have already determined. From an extensive series of experiments, they found reason to conclude, that muriatic acid gas, in whatever mode it is prepared, is uniformly the same. From the quantity of hydrogen gas which combines with oxymuriatic gas in its formation, it follows, that it contains 0·25 of water essential to its constitution. But the gas obtained by the usual processes, afforded, they found, exactly 0 25 of water, when transmitted over oxide of lead, or combined with oxide of silver; and the same compounds are formed, as by the action of oxymuriatic acid on silver and lead in their metallic state. They prepared muriatic acid gas, by heating fused muriate of silver with charcoal moderately calcined. It contained just the same quantity of water as muriatic acid obtained from humid materials, as it afforded the same quantity of hydrogen from the action of potassium. And instead of being capable of receiving the smallest additional portion of water, a single drop of water being introduced into three quarts of it, did not disappear, nor even diminish, but, on the contrary, increased in volume*. These facts establish the conclusion, that muriatic acid gas can receive no additional portion of water, but that which is essential to it, and hence preclude the solution of the difficulty under consideration by the opposite assumption. And it is to be remarked, that should even such a portion of water exist in the gas, it cannot be supposed that the acid should carry this with it into its saline combinations, and retain it so, that it should not be expelled by heat. It cannot be supposed to exist, therefore, in muriate of ammonia thus heated, and of course can-

* *Recherches Physico-chimiques*, t. ii. p. 133.

not

not account for the water obtained by the action of the metals on this salt.

When it is proved, that no extrinsic water exists in muriatic acid gas, there remain apparently only two modes on which the production of water can be explained,—either, that the metal may require less oxygen than is supposed in combining with the acid, so that a portion of water will remain undecomposed, to be deposited; or, that the oxide attracts more real acid, so as to liberate a larger proportion of water. The first of these suppositions is improbable, from the consideration of the law which regulates the combination of metallic oxides with acids,—that the quantity of acid is proportional to the quantity of oxygen, so that if an oxide were formed in these cases, at a lower degree of oxidation, it would only combine with a proportionally smaller quantity of acid, and the quantity of water detached from the combination would be the same.

No improbability is attached to the second supposition; and it has even some support from the consideration, that many metallic saline compounds form with an excess of acid, and that it is difficult, with regard to a number of them, to procure them neutral. Metallic muriates, with excess of acid, seem in particular to be established with facility. And although an excess of metal be present in the action exerted on muriatic acid gas, this may not prevent the formation of a super-muriate, more especially as the excess is in the metallic form, and exerts no direct action, therefore, on the real acid.

To ascertain if a super-muriate were formed in these cases, the product obtained from the action of the muriatic acid on the metal was raised to a heat as high as could be applied without volatilization, so that no loosely adhering acid might remain, and the air in the retort was repeatedly drawn out by a caoutchouc bottle. The solution from the residue both of iron and zinc was very sensibly acid. Some fallacy however attends this, from the circumstance, that the liquid state is necessary to admit of the indications of acidity, and in adding water to produce this, a change occurs in the state of combination, in a number of the metallic muriates; a super-muriate being formed, which remains in solution, and a sub-muriate being precipitated, so that the acidity of the entire compound cannot justly be inferred from that of the solution. I found, accordingly, that on adding water to the product from the action of the acid gas on zinc, this change occurs; a little of a white precipitate being thrown down, while the liquor remained acid. But the fallacy can be obviated, by adding only as much water as produces fluidity, without subverting the combination. Portions, therefore, of the residue were exposed to a humid atmosphere, until, by deliquescence,

cence, liquors were formed transparent, without any precipitation; and these were strongly acid, reddening litmus paper when it was perfectly dry and warm. I further found, that the product of the solution of zinc in liquid muriatic acid, when digested with an excess of metal, and evaporated to dryness, afforded by deliquescence a liquor sensibly acid. And in both cases, even when the solid product was retained liquid by heat, acidity was indicated by litmus paper. Lastly, what is still less liable to objection, the residue in the experiment of heating the muriate of ammonia with the different metals, afforded similar indications of acidity.

These results appear to establish the production of a supermuriate in the action of these metals on the acid, and this accounts for the appearance of a portion of water, since, supposing water to exist in muriatic acid gas, the quantity combined with that proportion of acid which would establish a neutral compound, is the quantity required to oxidate the metal to form that compound; and if any additional portion of acid enter into union, the water of this must be liberated, or be at least capable of being expelled.

It was of importance, in relation to this question, to ascertain the quantity of hydrogen obtained from a given quantity of muriatic acid gas; for, if the whole water essential to the acid is decomposed by the action of the metal, half the volume of hydrogen ought to be obtained,—muriatic acid gas being composed of equal volumes of oxymuriatic gas and hydrogen gas. I made this repeatedly the subject of experiment, by heating zinc and iron in muriatic acid gas. There are difficulties in determining the proportion with perfect precision; but the quantity of hydrogen always appeared to be less than the half; and on an average, about twelve measures were obtained, when thirty measures of the other had been consumed, a result conformable to the liberation of a portion of the combined water of the gas.

Whether the production of water in these experiments is satisfactorily accounted for, on the cause now assigned, may be subject of further investigation. In the sequel, I shall have to notice another principle, on which perhaps it may fall to be explained. Whether accounted for or not, it is obvious, that the fact itself is not invalidated by the theoretical difficulty; and also, that in relation to the argument with regard to the nature of muriatic and oxymuriatic acids, it remains equally conclusive.

In the doctrine of the undecomposed nature of chlorine, muriatic acid gas contains neither water nor oxygen, and the metal employed certainly contains none. These are the only substances brought into action, and it is impossible that water should be a product of their operation. On the opposite doctrine, water

water is held to exist in muriatic acid gas to the amount of one-fourth of its weight; and it is conceivable, that by some exertion of affinities, a portion of it may be liberated. If we were unable to explain the *modus operandi*, this would remain a difficulty no doubt, but not, as in the opposite system, an impossible result.

It is to be admitted, indeed, that in none of these cases is the entire quantity of water which must be supposed to exist in muriatic acid gas obtained; and so far the proof is deficient. But neither from the nature of the experiment is this to be looked for; and I give more weight to the argument, from having always found certain portions of water to be procured, while, on the opposite doctrine, there should be none. In those cases where, supposing water to be present in muriatic acid gas, it ought to be obtained in the full quantity, it uniformly is so, though the proof from these is rendered ambiguous by the result being capable of being explained on a different hypothesis.

[To be continued.]

XXX. *On the Temperature of the Mines in Cornwall. By Mr.* Thomas Lean.

To Mr. Tilloch.

Sir, — I was requested in the year 1815, by a member of the Cornwall Geological Society, to make some observations on the temperature of the air in the mine of Wheal Abraham, in the parish of Crowan, in this county. This mine is opened to the depth of 200 fathoms from the surface, and produces a considerable quantity of copper ore;—the vein in which the copper ore (sulphuret of copper) is found, contains (sometimes) a small quantity of tin ore, zinc ore, lead ore, and iron ore;—and generally a considerable portion of mundic. The metallic ores are imbedded in quartz and feltspar. The first observations which I made was on the 9th of June; and were made in a shaft in which a current of air is generally ascending through the mine. The thermometer (Fahrenheit's) when exposed for some time to the sun stood at 74°, and in the shade at 59°; and in the mine at different depths from the surface as mentioned below; viz.

At 3 fathoms the thermometer stood at	65°
20	$64\frac{1}{2}$
50	67
80	68
100	$68\frac{1}{2}$
120	69
140	$69\frac{1}{2}$

At

At 160 fathoms the thermometer stood at 70°
180 73
190 .. .. 79; which was at that period the bottom of the mine.

At the several stations (or levels) where I placed the thermometer to settle, a stream of water comes through the vein (lode) and is conveyed to the engines; and on immersing the thermometer in it, I found the temperature to be as follows; viz.

At 100	fathoms	68°	in the cistern at the engine shaft, where the water from this level is mingled with the water from the bottom of the mine.
	do.	64	in the stream issuing through the level.
110	do.	65	do.
120	do.	68	do.
140	do.	73½	do. issuing through a vein, rich in copper ore.
160	do.	74	do. do.
180	do.	74	do.
190	do.	74	do.

I next proceeded to ascertain the temperature in those parts of the mine where the labourers were at work; and which is at a distance of 15 to 30 fathoms from any current of air or of any of the working shafts, and obtained the following indications:

At 90	fathoms	74°	the vein dry, but rich in ore.
100	do.	70	do. but poor.
110	do.	72	do. do.
120	do.	73½	do. do.
130	do.	74½	do. do.
140	do.	76	do. containing ore in the vein, but not rich.
150	do.	78	do. but poor.
160	do.	80	do. the vein rich in copper, and other ores.
170	do.	78½	do. but poor.
180	do.	78	a considerable quantity of water dropping

from the roof of the working, and issuing from other parts of the vein which is rich in copper ores.

On the 13th of December 1815 I repeated my experiments, and the results were as under; viz.

At the surface in the open air		50°
3 fathoms under the surface		52
20	do.	57
40	do.	61
50	do.	63
60	do.	63½
80	do.	64
100	do.	66
110	do.	68—water 64°
120	do.	70

At

At 130	fathoms under the surface			$71\frac{1}{2}°$
140	do.	..	..	72—water 72°
150	do.	..	..	74
160	do.	..	..	70
170	do.	..	..	71
180	do.	..	..	74
190	do.	..	..	74
200	do.	..	..	78; which was the

highest temperature of the water at this time.

Should any of your correspondents account for the increase of the temperature in the Cornish mines, or communicate any observations of their own made in other parts of the kingdom, it would give considerable pleasure to many of your readers; and if you consider these worth notice, it is my intention to communicate, occasionally, observations which I may have an opportunity of making in other parts of this county.

I remain, sir,

Your most obedient servant,

Crowan (Cornwall), Sept. 11, 1818. THOMAS LEAN.

XXXI. *Account of a Voyage to the Coast of Labrador and Quebec, including Remarks on the comparative Temperature of the Eastern and Western Hemispheres.* By JOHN HAMETT, *M.D. Communicated in a Letter to Dr.* PEARSON.

H. M. S. *Prevoyante*, Channel, Aug. 21, 1817.

MY DEAR SIR,—ON the 27th of March, at 2 P.M. we set sail from the Sound,

"——— to distant climes, a dreary scene,
Where half the convex world intrudes between."

We got down Channel, and far into the sluggish Atlantic with light and pleasant breezes. Between the 2d and 14th of April the temperature was between 56 and 67, while the wind, which was generally from the eastward, just served to fan the blood. The temperature as we now proceeded to the westward became daily diminished, until the 22d, when at 8 P.M. in long. 55° 38′ and lat. 46° 18′ the mercury was down to 22. At this hour the north wind ejected with the greatest violence the thickest hail; indeed no face could possibly withstand this assault of the weather. On Sunday the 20th, at about 6 P.M. in long. 53° 38′ and lat. 45° 34′ we fell in with an enormous mass of ice, about as high as one of our top-mast heads; also with an extensive field of it, to which were attached five vessels fishing for seals. Early in the forenoon of Friday the 25th, we got into a number of large detached sheets of it, and about 2 A.M. of the ensuing day, it blowing a strong gale from the E. by S., the ship was inclosed in in an immense broken area, which with a very heavy swell on, made her sides creak dreadfully. However, we providentially escaped from this imminent danger with trifling damage to her. We entered the gulf early in the forenoon of Monday the 28th, and at 7^h 30^m P.M. of the 29th proceeded into an immense expanse of ice, consisting at the entrance of detached spongy pieces; but about 9^h 30^m her motion was altogether impeded; and in the morning we found that she was completely locked up by it, extending to the west and north as far as the eye could reach, while the part of Newfoundland in view called up terrific ideas of desolation and hunger. About 10^h 30^m A.M. of Friday the 16th May, after a number of zigzag motions, effected by means of small anchors that had been grappled in the ice for the purpose, we got clear, leaving to the south an immense field. This, as I was informed, principally drifts from the river St. Laurence, or gets into the gulf, and becomes increased there by the remainder of immense masses from Hudson's Bay, &c. which are borne round Newfoundland, and impelled in the respective directions of the winds or currents. That in which the Prevoyante was, continued to drift through the Straits of Bell-isle.

This ice business was scarcely over, when we had to encounter others of a more alarming nature: for on the 23d of May, about 7 P.M. the ship grounded on, or literally ploughed the Traverse, a zigzag rocky shoal about sixty miles down from Quebec, eight or nine miles in length, and extending right across, with the exception of a very narrow channel of a moderate depth, and which is commonly missed in consequence of only a solitary buoy being attached to it; while, on the other hand, the great distance of land on either side, the consequent deficiency of proper objects, and the ignorance of the pilots, who are only guided by the lead, (a method which, in a river of so great a flow and ebb, is not only uncertain, but for the most part fallacious,) naturally preclude the infallible or scientific resource of angular positions; or, to use a nautical phrase, cross-bearings. We luckily got clear here with (as we afterwards learned) the loss only of her false keel.

More vessels are, I believe, on an average, wrecked, and consequently more souls (in this respect) perish, in these parts than in any other quarter of the globe. Of the continual dense fogs and mists on the banks which contribute greatly to this, I shall say a little, after making a few preliminary remarks.

In summer and in autumn, we commonly find, that in proportion as a ship in sailing to Halifax, Newfoundland, or Quebec, recedes from the shores of England, or proceeds westward to certain longitudes, the winds from the southward and southern eastward become less heating, or more refreshing; and likewise in

winter

winter or in spring, we particularly find, that in proportion as she proceeds westward to certain meridians on this side the banks, the cold winds from the north, the eastward, northern eastward, or northern westward, become less piercing, and the temperature accordingly more congenial to the feelings; and that in proceeding further westward from these, the temperature gradually decreases, and the winds, from whatever point of the compass they blow, become colder and colder, until the sensations induced by the biting blasts indicate the proximity of lands impregnated with frost or covered with snow, or the contiguity of islands or mountains of ice in winter, or of fields or islands of it in spring, that are impelled in the respective directions of the winds or currents; these (as I stated before) are the remainders of immense islands or mountains of it from Hudson's Bay, &c. driven to the southward, and borne round Newfoundland, or those fields that have drifted through the gulf, and become increased there out of the river St. Laurence, on the breaking up or partial solution of the ice.

The early or protracted congealment in winter of those vast sheets of water the lakes of Canada, and the speedy or late solution of the ice in spring, naturally implies great irregularity of temperature in those parts of America. This irregularity of congealment, or solution, particularly extends to the river St. Laurence and Gulf, and has a corresponding influence on the temperature of the adjacent coast and Newfoundland, independent of that arising from those immense masses occasionally from the northward. Sometimes the river St. Laurence is frozen up in November; sometimes not until December or January; and sometimes it is but partially and thinly congealed. On the other hand, the solution of the ice takes place sometimes in March, and oftentimes not until the end of April, or beginning of May. The river was crossed over on foot at Quebec so late as the 4th of May last; yet the Baltic was open the whole of the preceding winter. It is therefore obvious, that an early or protracted solution of the ice is not always the consequence of an early or protracted congealment of it; it is often *vice versâ*. From the slowness and protraction of the infusion of the matter of heat into the icy waters of the river St. Laurence, it is considered highly imprudent to bathe in it previous to midsummer.

The occasional rise of fogs or mists may, I think, be accounted for by some of those principles laid down elsewhere in my observations on the climate of the Mediterranean; but those continual dense fogs or heavy mists peculiar to great banks, and in all probability connected with or dependent on other occult phænomena, cannot be exclusively referred to those general laws of nature alone. Those parts noticed for different soundings, or

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as abounding with banks of different depths, are remarkable for currents, and fogs or mists. Fogs or mists have been remarked in the Channel for a week together, even in summer. The North Sea, the bottom of which is composed of sand banks, abounds with currents; and the air commonly teems with fogs and moisture; while, on the other hand, places in-land, in the same parallels of latitude, are remarkable for the peculiar brightness of their northern sky, glittering constellations, and constant appearance of the *aurora borealis*, or milky-way. The Dogger-bank is remarkable for its frequent fogs and mists; but the banks of Newfoundland are notorious for dense fogs and heavy mists, in every season and in every year. The high temperature of the waters in the powerful current of the gulf of Florida, which extends as far as Newfoundland, must produce a degree of vapour on meeting with strong contrary currents of a low temperature; and this must be further increased by a certain degree of percussion and revulsion occasioned by the various immense sand banks about it, that naturally agitate the contending currents. In addition to this, if those operations or changes (so widely different in their nature, and which I already have noticed to take place,) in the respective extensive boundaries of Newfoundland be considered, the gulf of Newfoundland itself, and the banks from their particular position and peculiar properties, (the natural focus of mutually counteracting influence,) the causes of those fogs and mists may not remain altogether inexplicable. I think the bank fogs and mists rather prevail at the change of the seasons: if this be true, variation of temperature, or winds of different qualities, also contribute to their production. That these mists or fogs tend in their turn to produce irregularity of temperature all round to a certain extent, may be reasonably inferred.

In winter, in general, so excessive is the cold at night, and so cloudless the sky by day, that the presence of even an oblique sun, in consequence of the susceptibility of the system thus induced, renders the air, though without relative increase of temperature, almost congenial to the feelings.—No one would suppose that "the bleak coast of savage Labrador" reflects in summer the most intense heat.

From these natural occurrences it will I imagine be easily inferred, as the thermometer also proves, that the temperature of this watery and woody world has its maximum extremes, intense heats and excessive colds; and further, that it greatly varies during either of these intervals or seasons.

The great mutability of temperature in this part of America, as being so generally admitted, no one, I believe, will attempt to refute; nor, indeed, its baneful influence, whatever our acquaintance with the extent of it may be among the native inhabitants.

bitants. At all events, it is, as far as I can learn, sooner or later destructive to Europeans in anywise affected with coughs, colds, or hæmorrhages from the lungs; and speedily so, to such as are at all predisposed to or affected with, tubercles or vomicæ. Various are the calamitous instances that could be adduced, both in the navy and army, of the rapid progress and fatal termination of this, even amid the prevalence of other disorders peculiar to the climate. I have had some cases of pneumonia; and have at present one of vomicæ, which originated in a voyage to Greenland about eighteen months ago. I have also in my mind at this moment the particular case of a captain of foot, who was cut off in the flower of his age, at Montreal, in the winter of 1815, by this disease, aggravated as it strikingly was by the pernicious influence of the climate. Indeed, the peculiar susceptibility of the body to be acted upon by the relaxation or particular action of the exhalants, from a temperature so varying, is very great * * * * * * * * * *

XXXII. *On Arithmetical Complements.* By Mr. PETER NICHOLSON.

I HAVE been greatly surprised to find that the use of arithmetical complements has been entirely confined to logarithms, and that they are treated as if they only resulted from the properties of those artificial numbers. But whoever has much practice in finding the roots of equations by approximation, or in any other way, must have felt the confusion of so many changes of signs which require the negative and affirmative numbers to be added together separately, and then their differences to be taken: whereas, if we were to use not arithmetical complements, but numbers found in a similar manner, we need only add the whole together in one compact sum.

And thus it may be seen that arithmetical complements are a branch of common arithmetic, and not at all peculiar to, though very useful in, logarithms, nor their uses entirely confined to logarithms, but are equally useful in arithmetic and algebra.

Let —31416 be a negative number: subtract each figure from 10, and carry unity to the next figure; set the results in a row one after the other, proceeding from the right hand to the left, and prefix unity with a negative sign before the first figure. This simple operation may be done at sight, without putting the one number under the other, thus, $\bar{1}68584$, where only the first figure is negative.

This number now found is not the arithmetical complement, but equivalent to the number itself: for $\bar{1}68584 = -100000 + 68584 = -31416$.

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The arithmetical complement of any number is what that number wants of another number, which has unity for the left hand figure followed by as many ciphers as are digits in the proposed number. This is equivalent to the usual definition; but in my opinion it is incorrect, and not congenial to any good principle of explaining this kind of practice. Authors usually direct to subtract every digit from 9, except the last, which must be subtracted from 10. But as to this, the reader may put in practice which method he thinks proper.

As my principal object is only to change such numbers as are put in opposition to affirmative numbers, and properly indicated by the sign —; and in doing this I only find equivalent numbers, each composed of a negative and an affirmative part; this is therefore not a complement, it will thus be inconsistent to employ the term arithmetical complement; but as some term must be used in order to be understood, I shall therefore call the one number the reciprocal equivalent of the number proposed, as by the same operation the one may be converted to the other by observing the proper change of the signs.

I shall here present the reader with a few examples on this species of arithmetic.

ADDITION.

To add numbers which have different signs together.

Rule.

Find the reciprocal equivalents of the negative numbers: then add these equivalents and the affirmative numbers into one sum, and deduct the negative units that may be in any column from the sum of the column.

Example.

Add 7854, 31416, —734, 65321, —2965.

Common Method.

7854		7854
31416	734	31416
65321	2965	$\bar{1}266$
104591	3699	65321
		$\bar{1}7035$
		100892

104591
3699
100892

SUBTRACTION.

Add the reciprocal equivalent of the number to be taken away to the number which is required to be reduced, and the sum is the remainder.

Examples are unnecessary.

MULTIPLICATION.

Supposing the multiplier an abstract number, multiply the affirmative part of the proposed number by the multiplier, and only set down as many of the right figures as are in the number to be multiplied; then deduct the number formed by the remaining figures on the left from the product of the negative part of the given number by the multiplier; prefix this difference with a negative sign over it before the affirmative part, and we have the product required.

Examples.

(1)	(2)	(3)
$\bar{1}5632$	$\bar{2}734$	$\bar{5}89367$
9	8	7
$\bar{4}0688$	$\overline{11}872$	$\overline{29}25569$

The reason is obvious, for $9\times\bar{1}5632=9\times-10000+9\times5632$.

Now here any one of these products may be made to contain only a single negative unit in the first place of figures; by subtracting each of the negative digits from 10, as in the preparatory rule.

Thus in the first example the product $\bar{4}0688=\bar{1}60688$ for $\bar{1}60000=-100000+60000=-40000$.

Again, $\overline{11}872=\bar{1}89872$; also $\overline{29}25569=\bar{1}7125569$.

DIVISION.

If the negative part is divisible by the divisor, write the quotient below, and place the negative sign over it: But if not divisible by the divisor, increase it till it becomes divisible; then whatever number was added to the negative part, in order to make it divisible, we must add an equal number to the affimative part and divide by the divisor, and set down the quotient after the negative part.

Examples.

(1)	(2)	(3)
$4)\bar{8}7832$	$5)\bar{7}8540$	$7)\bar{6}7983$
$\bar{2}1958$	$\bar{2}7708$	$\bar{1}2569$

The reason of this operation is obvious, since the number to be divided consists of an affirmative and a negative part; therefore by increasing each equally, viz. the negative by a negative, and the affirmative by an affirmative, the difference is still the same. Thus in example second, the number to be divided is $\bar{7}8540=-70000+8540$; then if to the first of these last numbers we add -30000; and to the second we add 30000, we shall have -100000 and 38540, which are together equal to $\bar{7}8540$.

XXXIII. *Com-*

XXXIII. *Comparative Trials of the respective Merits of "*Massey's *Patent Sounding Machine," and one known by the Name of* Gould *and* Burt's *Buoy and Knipper. Communicated in a Letter from Mr.* Edward Massey.

To Mr. Tilloch.

Sir, — To lessen the chances of misfortune in any place, is an object of consideration with the humane; but to add to the security of vessels in their passage through the ocean, is worthy of the attention, not only of the merchant, but of the statesman and the philosopher. It is needless to suggest when ships are in a rough and tempestuous sea, and in a dark night, making perhaps ten knots an hour, and sailing in the company of a fleet, how desirable it is to all commanders to obtain soundings which may be depended upon as accurate, and which may be resorted to constantly without bringing the ship to, or retarding her way.

My sounding machine has been adopted by the navy for ten years, and during that period upwards of seventeen hundred of them have been in actual use; and reports of their accuracy, from skilful and experienced naval officers, may be referred to as undoubted testimonials of their merit.

When I find, however, that within the last four years a machine has been proposed for the adoption of the navy, which is fallacious in its principle, and in its consequences must be destructive to many ships and many crews; where forced recommendation may be substituted for the test of experience, and where the opinion of interest may delude the accuracy of invention; I feel myself summoned by the voice of truth, in justice to my own interests, and those of mankind, to state the result of a public trial, respecting the comparative merits of these two machines.

The following Notice was sent to the Lords of the Admiralty, the Commissioners of the Navy, the Board of Longitude, the Trinity House, and to the Companies in the metropolis connected with shipping and navigation; and it was inserted at the same time in the daily newspapers called *The Public Ledger*, *The New Times*, and *The Morning Post*.

"To Navigators. — Edward Massey, the inventor of the Patent Sounding Machine, which has been adopted many years in the Royal Navy, invites naval officers, ship owners, sea-faring men, and every person connected with navigation, to witness a public trial of the comparative merits of his sounding machine and one known by the name of 'Gould and Burt's Buoy and Knipper,' to be made in the river Thames at London Bridge, on Thursday the sixteenth instant, at half past two o'clock in the

 afternoon,

afternoon, precisely; where those gentlemen who have used the buoy and knipper, and reported in its favour, are particularly requested to attend, to witness this trial of their comparative accuracy.—11th July 1818."

Accordingly on Thursday the 16th instant, in pursuance of this advertisement, I proceeded at the time, and to the place appointed; and in the company of several naval officers and gentlemen of high respectability, resorted to experiments which have been duly reported to the Lords of the Admiralty, the Commissioners of the Navy, and the Board of Longitude. In one experiment made with the buoy and knipper in three fathoms water, it appeared by the knipper to be four fathoms; and on repeating this experiment in three and a half fathoms, it appeared to be seven, the buoy running up the line; so that this instrument will show a depth of water, in a current, or when a ship is under way in a driving sea, so much greater than the real depth, as to endanger in the most imminent degree both the ship and crew.

In another instance, when the buoy and knipper were carried in a second boat, and the lead dropt with thirty fathoms of line across the current, the lead hung in the knipper, and would not sink; this experiment was repeated with fifteen fathoms of line across the currrent, when the same result took place—the lead not sinking.

In repeating these experiments frequently with my machine, the real depth was given in every instance, without any variation. I am, sir,

Your obedient servant,

Coventry, July 20, 1818. EDWARD MASSEY.

XXXIV. *On the Modulus of Elasticity of Air, and the Velocity of Sound. By Mr.* THOMAS TREDGOLD.

To Mr. Tilloch.

SIR,—WHEN Sir Isaac Newton investigated the propagation of sound, he considered the weight of an uniform atmosphere to be equal to the mean elastic force of common air*: and this mean elastic force has been considered to be the same as the modulus of elasticity of later writers. But the modulus of elasticity is defined to be a column of the same substance, capable of producing a pressure on its base, which is to the weight producing a certain compression, as the length is to the diminution of the length. Now this differs materially from the mean elastic force

* Principles of Natural Philosophy, props. 47, 48, 49, and 50. book ii.

force of Newton; and yet it appears to be the only correct measure of the elastic force of bodies.

Let F be the force producing a compression E in a body whose magnitude in its natural state is expressed by unity; then $E : 1 :: F : \frac{F}{E} =$ the weight of the modulus of elasticity.

Boyle, Marriotte, and some other experimentalists infer, from their experiments, that air is compressed into half its natural space by the addition of a pressure equal to the weight of the atmosphere. Let the weight of the atmosphere be equal to thirty inches of mercury, of the specific gravity 13·500; then $\frac{F}{E} = \frac{14{\cdot}65}{0{\cdot}5} = 29{\cdot}3$ lbs. the weight of the modulus of elasticity for a base of an inch square. And the weight of 100 cubic inches of air being 30·5 grains, the height of the modulus of elasticity of air will be 56,038 feet. It is shown by writers on dynamics, that the velocity of sound is equal to that acquired by a heavy body in falling through half the height of the modulus of elasticity of the medium; consequently it will be nearly equal to the square root of half the height of the modulus of elasticity multiplied by 8; and taking the value of the modulus stated above, it gives 1339 feet per second as the velocity of sound. Otherwise the height of an uniform atmosphere, according to Professor Leslie*, is 27,800 feet; and $\frac{F}{E} = \frac{27800}{0{\cdot}5} = 55{,}600$ feet, the height of the modulus of elasticity of air; and, accordingly, the velocity of sound would be 1333 feet per second.

In these calculations the elasticity of the air is supposed to be perfect, and that the compression is as the force producing it; neither of which are true of atmospheric air: besides, it always contains a considerable quantity of vapour, sometimes, according to Dr. Thomson, as much as a sixtieth part of its bulk, and it is well known that the elasticity of damp air is much inferior to that of dry.

But the chief cause of error appears to arise from considering atmospheric air a perfectly elastic fluid, and the elastic force to be as the compression. The rude experiments of Boyle and Marriotte were insufficient to establish the law of compression. More accurate experiments have, I believe, been made; but I have not an opportunity of referring to them at present.

The imperfect elasticity of air appears to be owing to the heat extricated during compression. The temperature being the same, every change in the density of the air must be accompanied by a corresponding change in the quantity of heat it contains. When

* Supp. Ency. Brit. art. *Acoustics*, p. 44.

air is compressed, the excess of heat will begin to be diffused among the surrounding bodies as soon as it is developed: and when the pressure is removed, the air cannot return to its original bulk till it be supplied with heat.

If the heat given out during compression could be retained by non-conductors round the compressed air, ready to be imbibed whenever the pressure should be removed, then undoubtedly the elastic force would be as the density; but in all cases the heat is free to move to other bodies; and when air is compressed into half the space it occupies in its natural state, according to Mr. Dalton, its temperature is increased fifty degrees: and as the rate of cooling is nearly as the excess of temperature, the elastic force will be diminished in proportion to the quantity of heat given out: consequently, under great compressing forces the elastic force must increase much slower than the density, but the precise effect will differ according to the time in which the change is produced.

To take into consideration the imperfect elasticity of air, and the effect of the vapour it contains, would render the calculation of the velocity of sound extremely intricate; but an approximate value of the modulus of elasticity might be obtained from experiment.

If Newton's measure of the elastic force of air had been correct, the velocity of sound calculated from it should have been above the result of experiment and not below it; because he supposes the air to be perfectly elastic. His hypothetical supposition respecting the magnitude of the solid particles of air is unsupported by experience or analogy: and though his reasoning respecting the effect of vapour in some measure coincides with the ingenious speculations of Mr. Dalton, yet it is not the less distant from the real laws of the elasticity of a mixture of gaseous bodies.

I am, sir, yours, &c. &c.

Sept. 21, 1818. THOMAS TREDGOLD.

XXXV. *On the Theory of Water-Spouts.* By Mr. GAVIN INGLIS.

To Mr. Tilloch.

DEAR SIR,—I HAVE been observing with no small degree of interest the various statements regarding the laws and principles of that wonderful and often alarming phænomenon, the water-spout, a satisfactory theory of which can only be deduced from a collection and collation of facts resulting from actual observation, and furnished by those who may have had an opportunity of of witnessing these marvellous operations of nature. Several papers in your useful Miscellany are so replete with information of this class, the occurrences so judiciously observed and so minutely detailed, as to render them particularly interesting to the student of meteorology.

My professional avocations have afforded me no opportunity of viewing these wonders of the deep; nevertheless, I have had the good fortune to see two; the one an ascending, and the other a descending water-spout. They are twin children of the same parent. The two I had the happiness to observe, were completely conclusive as to the water rising to the one cloud, and falling from the other; upon which I founded a theory to satisfy myself, and had preserved drawings of their respective appearances taken at the time, and notes of what I thought necessary to keep in remembrance. Independently of these, nothing could be more strongly impressed upon my memory than what I am about to detail.

I was then a very young man, and had only begun to keep a common-place book, to take drawings of whatever struck my fancy, and notes of such occurrences as I conceived worth preserving. I had gone pretty early in the morning of the 2d of July 1788, to a rising ground at the back of Kirkaldy, opposite the old Fish Ponds of Abbotshall; where some rondales or small isles had been formed and planted with large fir trees, which produced an admirable and amusing echo. This was from a desire to ascertain, whether the warm rarified air of a summer evening, or the cool dense air of the morning, was most conducive to the continued vibration of sound necessary to produce an echo. Musing and attentively listening to the responses—the sudden overcasting of the sun made me turn round in expectation of immediate rain, when my eye was attracted towards the opening of the Firth. Nothing could exceed the awful appearance of the atmosphere, conjoined with the gloom cast upon the surface of the sea, by a cloud of uncommon darkness and density that spread over the whole opening of the Firth, from Fife Ness towards St. Abb's Head. From the front of the cloud a well-defined line, to appearance about the thickness of an ordinary cable, descended to the surface of the water. Looking attentively, it soon assumed the magnitude of a man's waste, and continued to increase as it advanced up the Firth. It was some time before I could bring myself to conceive what this might be, when the idea of a water-spout driven by the east wind into the Forth from the German Ocean came into my mind. Elated with the thoughts of seeing what I had often heard described as a tale of wonder, I ran to the shore, in expectation of studying it to more advantage, where a great number of people had already

ready collected, amongst whom several old sailors whose testimony put its identity beyond all doubt.

It came up the Firth with such rapidity, propelled by its own electric velocity, outstripping the light breeze of easterly wind then blowing, so that a solitary fisherman seated in the stem of his boat, with his face all the while towards the phænomenon, had only time, after perceiving its rapid approach, to draw up his anchor-stone and pull out of its course. He, after landing, described the appearance of the surface, all round the *proboscis*, "just like a boiling caldron." The hissing noise of the water separating itself from the salt, was most distinctly heard by those on shore, which noise made them suppose the water was falling from the cloud, and prognosticated a deluge and destruction of the west end of the town. But as it neared the shore and lacked water, the *proboscis* lessened in density and thickness, and at last detached itself entirely from the water and ascended in beautiful spirals into the cloud, and passed over the town with only a few heavy drops of rain. *This* I consider most conclusive of the rise of the water from the sea to the cloud in this class of water-spouts.

While the water was rising from the sea and ascending to the cloud, and went off without injury at the *negative*, all the effects of a descending water-spout, from the lengthened course and the surcharge of water in the shape of torrential rain, was felt within the range of the *positive* end, in such quantity and with such rapidity and force, as nearly to prove fatal to some Buckhaven fishermen who had put to sea that morning. To save their lives, they had to give up their oars, and with their hats bail out the water that fell from above, to keep their boats from sinking. The cloud took a direction up the country towards Cupar, doing material damage, and created torrents where water never ran before, and converted some new-made ditches into deep and broad ravines, now the beds of small streams and purling rills. Give me leave now to state the theory I had formed before narrating the account of the descending water-spout.

I am perfectly satisfied, that the whole phænomenon of the water-spout, whether ascending or descending, which as already mentioned are but twin children of the same parent, is entirely dependent on and governed by the laws of electricity. Whether the cloud that becomes the parent of the water-spout, be positively or negatively electric, a collection of facts can best determine. My opinion is that it must be negatively so, at its first formation, and that in the escape of the equilibriating electricity from the ocean, the surface becomes violently agitated, or rises into a column elevated above the general level, which I would account for by supposing a perpendicular *proboscis* to descend from the gathering

gathering cloud. The electricity rushing at right angles in full force from every point of direction to one common centre, must by the collapsing force of currents raise the water above its level, and that elevation may be rendered apparently stationary, during the continuation of the same, or perhaps an increased electric pressure. But in cases where the *proboscis* descends obliquely, the stream of electricity at the commencement from the obliquity of the attractor will be partial; and, instead of drawing at once from every point to a focus, the first current flowing in a direct line with and towards the descending projectile, and not at right angles with the common centre, must give an oblique direction to the accumulating fluid, and instead of collapsing and raising the water, produce an eddy, turning a mass in the midstream like passing water giving motion to a horizontal wheel. In absence of a better hypothesis, electricity is always at hand, like phlogiston of old, to fill up the vacuity of every doubtful theory. If I am wrong, I shall be happy to adopt any other more satisfactory. I however must candidly confess my ignorance of any other element so universally diffused, or so powerful in its agency, that could so instantaneously be called into action, and brought, when necessary, from pole to pole, or could produce the chemical effect of separating, so rapidly and in such quantity, the fresh water from the salt, and carry it upwards from the sea to the clouds. Were the waters raised from the sea to the higher elevations of the air by mere suction, or raised as waters in a pump, the ascending water must of course carry with it all the saltness of the ocean, and the rain from such clouds must of necessity be salt: were this the case, every isle, and many parts of the continents, must be visited by saline showers, and the waters of the isles become brackish and unwholesome. If this has ever been the case, some of your better informed correspondents may be able to instruct, and I should be glad to be guided by such information. I am however inclined to believe that the allwise Distributor of every blessing has of his infinite wisdom ordered it otherwise, that all the waters drawn up from the sea, that are again to descend upon the earth for the support of creation, must go up fresh, and that the hissing and ebullating noise, the never-failing attendant of the ascending waterspout, is occasioned by the chemical separation of the water from the salt.

Such has been my opinion of the cause and origin of the ascending waterspout; the descending, upon investigation, will be found a legitimate of the same family, being only a discharge of the water from the positive, that has ascended by the electric influence at the negative part of the cloud, and which must, when the cloud is overcharged, again fall in rain in all its forms, or in a solid column, *then* denominated a waterspout; just as incidents

may

may occur to give birth to its first arrangements. From this circumstance, descending waterspouts can seldom occur at sea, except in the shape of torrential rains, having few points of attraction to draw the spark from the cloud. It is certainly on shore, and in alpine countries, where they most frequently appear, when the elevated peaks and lofty mountain tops present their grave attraction, and arrest the clouds in their progress, drawing from thence the electric matter with its concomitant destructive and deluging floods.

Let us suppose, for instance, a cloud of any magnitude to collect over the main ocean, sufficiently electric to become the parent of an ascending waterspout; suppose the connecting *proboscis* ample, the corresponding vortex must be of proportionate capacity: the ascending vapour will soon extend over a great space. Still the concentrating aggregate will be directed by the electric impulse towards the positive tendency; and thus, after the cloud has received a full charge, the confluence of the particles must produce rain from all parts within the positive range, unless some attracting body give to the whole volume a drift current to a concentrated focus, and by consolidating the particles fall in one dense mass. Or suppose the cloud to extend, and go on uninterruptedly accumulating, propelled by the wind or its own electric impulse towards, and brought in contact with, St. Helena, Teneriffe, or some other island of the ocean; we might then as well bring a powerful conductor in union with a full charged battery without a shock, as not produce a descending waterspout. Draw but one spark from the cloud, and you instantly produce a current or a vacuum: however small at the commencement, a collapsation takes place in proportion to its magnitude, the concussing globules augmenting in size by the incessant oscillant motion of the electric fluid become rain, the rain forms heavier and heavier, and in falling from the cloud with accumulating force draws more into its wake, continuing to aggregate till the whole volume of the watery matter is drawn into one centrical vortex, forming a column of dense solidity, and falling with deadly destruction on whatever it may chance to descend on.

Such I believe was the origin and termination of the waterspout which did so much mischief at St. Helena.

Amidst these suppositions, let us take another case, and imagine a cloud whose connexion with the sea had by some accident been cut off, after having attained nearly a full charge of electricity and watery vapour, and after the consequent turmoil adjusted its own equilibrium, the cloud would become positive towards the nearest or greatest point of attraction; viz. if at sea, the corresponding convexity of the surface of the globe, or the nearest or highest headland or mountain on shore. Should a

ship

ship be so unfortunate as to become the attractor of a cloud of this description, and the main mast the point of discharge, although well provided with all the necessary apparatus to save the ship from the influence of the electricity, it is hardly possible that any vessel could escape destruction from the descending water, or any one remain alive to tell the dreadful tale. It is of no consequence what is the main attractor, or what becomes the principal point of discharge: once drawn off, the electric spark and all its direful concomitants follow in destructive array with the descending waterspout.

I have now to detail the particulars of that spout alluded to in the outset of this paper.

It fell on Benardy, a hill at a short distance from this, which separates Loch Leven from Loch Orr, between two and three o'clock in the afternoon of the 18th July 1792. The morning was warm and delightful; there was no indication of rain till towards mid day, when a heavy cloud began to rise from the west, and advanced eastward, casting a particular gloom over the face of nature as it covered the meridian, and I observed the sun darkening the whole country with a more than ordinary dusk. I had scarce sat down to dinner, when one of the servants came in, and begged I would look at the extreme commotion distinctly visible in the cloud now resting over Benardy. The appearance was highly amusing, the whole cloud seemed convulsed, and frequent bursts of white vapour, like dense white smoke, issued from its dark sides; at last a flash of lightning of uncommon brilliancy and size darted from the lower part of the cloud. This was instantly followed by the spout, in shape of an inverted cone, which joined the cloud and the hill, deluging the whole country round. This was soon followed by one of the most awful thunder storms (still fresh in the memory of every one) that ever visited this part of the country. The quantity of water that fell from the cloud by the spout was quite incredible. Those who lived nearer the hill and observed its appearance more closely, described its descent from the mountain's brow like the waves of the sea in a storm. The descending water carried every thing before it, bore down many roods of Galloway dykes, filled quarries; and Loch Orr, that had just been drained, was, notwithstanding its deepening and increased outlet, soon filled to its ordinary level; Loch Leven was raised to an unusual height, the Carses were overflown, and the river Leven below Auchmoor bridge in a very short time filled considerably beyond its highest winter flood. The rain fell in torrents for miles round; notwithstanding of which, the cone on the hill remained distinctly conspicuous, although somewhat obscured, till the cloud had discharged its whole contents, and the sky became clear. Curiosity prompted several

ral people to visit the summit of the hill after the storm was over, where they found the strongly matted turf, where the spout poured out its waters, completely torn off, the subsoil washed away, and a considerable space laid bare to the rock.

This I think must be considered as conclusive of the descent of the water in this case, as the rise of the water in the other.

I remain yours ever,

29th July, 1818. GAVIN INGLIS.

XXXV. *Notices respecting New Books.*

A new Variation Chart of the Navigable Globe, from 60 *Degrees North to* 60 *Degrees South Latitude. By* THOMAS YEATES.

THE charts hitherto published being only transcripts of Dr. Halley's original chart, with few corrections for the change of variation since his time, and none of them extending beyond the Atlantic and Indian oceans; navigators have long regretted the want of an accurate variation chart comprehending the whole circuit of the navigable ocean and seas of our globe. To supply this want, the author of the present chart has with much labour and care constructed a general chart of the variation of the magnetic needle, for all the known seas within sixty degrees of the equator, north and south, from accurate documents obtained from Spanish surveys in the Pacific Ocean, journals at the Hydrographical Office, Admiralty, and the East India House, collated with tables of the variation recently formed from the observations of different navigators.

The Chart is on the Mercator's projection, drawn with the latest improvements, and the magnetic lines are drawn upon it throughout for every degree of change in the variation; with an accurate delineation of the magnetic equator and meridians, never before introduced in charts of this description, and the whole is accompanied with suitable remarks and illustrations.

The Chart is presumed to form a valuable accompaniment to all navigation charts in present use, and will be found not only important to the navigator, but interesting to the philosopher and the man of science, in exploring the theory and properties of the magnetical instruments used on sea and land.

Anderson and Chase have just published their Annual Catalogue of Books in Anatomy, Medicine, Surgery, Midwifery, Chemistry, Botany, &c. &c. new and second hand, including an extensive collection of foreign publications recently imported, a complete List of the Lectures delivered in London, with their terms,

terms, hours of attendance, &c. Also, A Manual of Practical Anatomy for the use of students engaged in dissections, by Edward Stanley, Assistant Surgeon, and Demonstrator of Anatomy, at St. Bartholomew's Hospital, in one volume 12mo.

XXXVI. *Proceedings of Learned Societies.*

SOCIETY OF SCIENCES OF HAARLEM.

The following questions in the Physical Sciences have been proposed by this Society for competition previous to the 1st of January 1820:

3. How far it is actually demonstrated that the fumigations by oxygenated muriatic gas after the manner of Guyton have served to prevent the spread of contagious maladies? What are the contagious maladies in which the effect of this gas deserves to be tried, and what ought to be principally observed in such experiments? Is there any reason to expect a more salutary effect, in the prevention of contagion, from any other mean hitherto employed or proposed?

4. What are we to regard as distinctly proved in respect of the gastric juice of the human body, and its influence in the digestion of food? Is its existence sufficiently proved by the experiments of Spallanzani and Senebier; or has it been rendered doubtful by the experiments of Montègre? What is it that comparative anatomy, and principally the opening of the stomach of animals killed, either after fasting, or in a short time after having taken food, have rendered probable in this respect? And in the case of the existence of the gastric juice in the human body being regarded as a fact perfectly established, what ought we to avoid, in order not to impair its effect in the process of digestion?

5. As the new mode of distillation which some years since was originally practised at Montpellier, and has been since adopted and improved in the south of France, according to which the substances from which spirituous liquors are extracted are not immediately exposed to the action of fire, but heated by steam—a process which is not only more œconomical than the ordinary method, but which has this additional advantage, that the spirituous liquors produced by it are of a purer and a more agreeable taste—the Society desire to know "What is the best apparatus for extracting, according to this method, with the greatest profit, the purest spirituous liquors from grain, as wine is drawn from the vines of France?"

The prize offered to those who, in the judgement of the Society, shall give the best answer to any of these questions, is a gold

gold medal, or one hundred and fifty florins, at the option of the author.

An anonymous individual has offered to the French Royal Academy of Sciences a sum of 7000 francs for the foundation of a prize of experimental philosophy.

XXXVII. *Intelligence and Miscellaneous Articles.*

THE SAFETY-LAMP.

On this subject we have received a letter from an anonymous correspondent, from which we take the following paragraph:

"In a letter to Dr. Thomson which appeared in the Annals of Philosophy for April 1816, p. 319, the following statements occur respecting the lamp for mines:

"You have not developed the principle upon which the benefits of the gauze depend. You talk of a fixedness of the air which cannot be. If an explosion takes place without any considerable extrication of heat, the contact of the adjacent wires cools down the red-hot air, and renders it incapable of kindling combustion without." It is clear, therefore, that Dr. Thomson did not before that time possess the principle upon which the non-communication of combustion through wire-gauze or multiplied holes in metal depends; and it is also clear that the prior discovery and development of the wire-gauze principle by which the inflammation of gases is arrested, belongs to the writer of that letter, if at that time no statement of the principle *had appeared in print*. If it had, Dr. Thomson would have known of it. If it had, let the time be stated. If it had not, let due credit be given to the writer of that letter."

The remaining part of our correspondent's letter, we withhold as irrelevant to the question on which he treats. He has "still some remaining doubts of the *perfect* safety of the lamp," and to have these removed he proposes that the lamp should be plunged into various mixtures of *hydrogen* and *oxygen gases*—of *artificial carburetted hydrogen gas* with *oxygen*. No such mixtures ever exist in mines, and they have therefore nothing to do with the lamp, as a safe-lamp, for mines. Our correspondent next proposes that similar experiments should be made with inflammable "gas from the mine by plunging the lamp entirely into various mixtures of it with common air in vessels in the laboratory, not applying it to blowers in the mines." Such experiments as he recommends with the gas of the mine have been often made already, and always with results favourable to the safety of the lamp.

STEAM

STEAM ENGINES IN CORNWALL.

From Messrs. Leans' Reports for July and August 1818, it appears that during these months the following was the work performed by the engines reported, with each bushel of coals.

For July.	*Pounds of water lifted 1 foot high with each bushel.*	*Load per square inch in cylinder.*
25 common engines averaged	23,761,463	various.
Woolf's at Wheal Vor ..	29,081,048	17·3 lib.
Ditto Wh. Abraham ..	34,286,774	16·8
Ditto ditto	27,932,848	6·6
Dalcouth engine	38,055,354	11·3
Wheal Abraham ditto ..	34,561,187	10·9
United Mines engine ..	33,147,791	14·4
Treskirby ditto	41,325,216	10·8
Wheal Chance ditto ..	32,441,682	11·9
For August.		
25 common engines averaged	23,851,384	various.
Woolf's at Wheal Vor ..	27,746,727	17·5
Ditto Wh. Abraham ..	45,510,419	16·8
Ditto ditto	25,944,753	6·7
Dalcouth engine	41,883,745	11·3
Wheal Abraham	38,032,270	10·9
United Mines ditto	33,289,655	14·9
Treskirby ditto	38,308,014	10·8
Wheal Chance ditto ..	31,379,570	11·9

NORTHERN EXPEDITION.

Official accounts have arrived from the vessels employed in the attempt to discover the North-west passage to the Pacific Ocean, dated July 28th and August 1st. At the date of the first despatches the Isabella and the Alexander were in latitude 75° 30′ N. longitude 60° 30′ W. very near the head of Baffin's Bay. The weather was serene and perfectly clear. The variation of the compass, by accurate observations repeatedly made on board both ships, was 89° and the dip 84° 30′. It had been perfectly calm, and the sea as smooth as glass for three or four days, and the current drifted them to the south-eastward, which raised their hopes of there being an open passage to the westward, through Alderman Jones's or Sir James Lancaster's Sound. All the way up the middle of Davis's Straits they skirted an unbroken field of ice on the left, but as they proceeded it became thinner, and apparently rotten, and they were sanguine that, the moment the breeze sprang up, the ice to the westward would allow them to reach the northern shores of America. The utmost harmony prevailed

prevailed among the officers and every part of the ship's company, and all were in perfect health.

The second dispatches of the 1st of August are the last which in all probability will be received this year, as our ships were going beyond the track of all the trading and fishing vessels, which till then had accompanied their course. Strange as it may appear, the approach of winter, which begins very early in those high latitudes, seems to have increased, instead of shutting out every hope of success. In a private letter from Captain Ross, in lat. 75° 48′ N. long. 61° 30′ W. he says, "I have but a few moments to tell you, that we have now every prospect of success, the ice is clearing away fast, and the wind is at N.E. Our variation observed on the ice, 88·13. We have killed a whale, and laid in a stock of blubber for our winter fuel."—The letters received from other persons, under his command, are of the same date, and equally promising. They state, that the ice was clearing away, and that their prospect of success was improving. The most extraordinary phænomenon of the variation of the compass had gone on increasing;—it was 88. 13. on the ice—we say on the ice, for on board ship, owing to some peculiar influence not yet ascertained, it was much more. The former letters, of which we have already given extracts, mention, that on board ship the variation was at one time 95 degrees, that is, the needle pointed, instead of *north*, to the *southward of west*. This difference between the real variation and an apparent variation on board ship was first observed by Captain Flinders, but it was supposed to be an accidental peculiarity in his ship: it is now clear that it belongs to all ships, and varies in all, and there would be little doubt that it should be attributed to the influence of the iron about the vessel, except for a curious fact which we understand has been ascertained; namely, that the compasses called insulated compasses, which are placed in boxes of iron, and which are uninfluenced by external iron, when brought near to them, are affected by the ship variation in the same degree as the common compass.—This, which is now called the *deviation*, has been found to be much greater as the experiments go northward. This is accounted for from the circumstance of the dip of the needle diminishing what is usually called its polarity, and allowing it, therefore, to be more easily affected by the local influence of the ship.

Such is the substance of the official accounts as far as we have been able to learn. There is an abundance of private letters to the friends and relations of those who have embarked in this most important enterprise. The following are extracts from some of the most interesting.

"His

"His Majesty's Ship Alexander, June 17.

"My Dear Sir,—I am now writing in the tent upon the north end of Hare or Waygatt Island, with the pendulum clock within one yard of me, and the observatory and all the instruments within half-a-dozen. We were arrested in our progress yesterday by the ice, which forms a complete bar about three miles to the northward of this island, commencing on the Greenland side, from what is called Four Island Point, and extending down the straits at a distance not greater than ten miles to the westward of this island, and 15 to the westward of Disko. Soon after entering the Straits, we found it absolutely impracticable to go up to the middle, as the ice gradually brought us into the land, till a little to the northward of Riskoll (vulgo Reef Koll) we were for a day or two totally blockaded. The ice then, by one of those unaccountable changes that so frequently occur here, opened sufficiently to give us a free passage, till yesterday we found a second bar in this place. From every account we have received, as well as from what we have already seen, it is certain that the last winter has not only been severe, but that the frost has lasted much later than has been the case for many years past. You may imagine our surprise when, on coming into this neighbourhood yesterday, we found upwards of 35 British ships at anchor upon the ice-bergs, which completely form a cluster of innumerable islands from the spot in which I at this moment view them. They have all been detained here—not days, but weeks, in spite of every exertion to get to the northward; and the fishery may be considered as hitherto an unsuccessful one, with the exception of a few of the ships in Disko Bay.

"The causes which operate upon the ice producing very sudden changes in it, are so little understood, that it is impossible to judge when any such change may take place as to enable us to get to the northward. I have just been to the top of a mountain of no inconsiderable height, to determine its altitude by the barometer: and I wish I could give you an adequate idea of the magnificent sublimity of the scene I have just witnessed. The whole horizon to the northward and westward is one complete mass of compact field ice; with the exception of above 500 ice-bergs, which, with here and there a small spot of clear blue water, serve to vary the scene, which would otherwise tire the eye with the uniformity of its dazzling whiteness. To the eastward is seen the land of Greenland, very high, almost entirely covered with snow, and frowning, as it were, upon the ocean of ice which environs its shores. To the southward is the island of Disko, with its summit (which we have never clearly seen) completely lost in the clouds. Near this island are all the Greenland ships at anchor, giving a finish to the scene, whose grandeur and beauty are far beyond any thing I have seen before. The longitudes of the places on this coast were very much in want of correction. We had a great number of excellent lunars to the southward, which, with the Isabella's chronometers, which go admirably, will, I think, determine the longitudes so far, to the nearest three or four miles. The dip of the needle in lat. 67. 22. was 82. and the variation 67. 30.

"Here the dip about the same, and the azimuths we have taken this morning we cannot work for want of a latitude, which we hope to obtain at midnight. The transit of the sun for the pendulum we hope to get to-morrow, and if the ice still remains firm, so as to prevent our leaving this place, the next day, we trust, will produce something in this way. Delighted as I am to take a part in these observations, I confess I should be glad to see the tents struck to-night, and the ice open; and you may rely upon it, that no object whatever will ever tempt our Commodore to neglect for an instant the main object of the expedition. The current that has been spoken of as coming constantly down the Straits, if it exists at all, must be to the westward of our track up the Straits; and, indeed, all the masters of the ships have a great dread of being set to the westward in our present latitude, as they insist upon it that if a ship were beset here she would probably come out in 65 degrees.

"Tuesday, June 23.

"The ice having opened a little on the evening of Saturday, we endeavoured to get over from Hare Island to the coast of Greenland, or, as the masters call it, the East Land. The Isabella was beset in making this attempt, and was drifted about with the ice by the tides till Monday morning. We were more fortunate, having succeeded in getting over to the land, and into clear water, on Sunday evening, and there made fast to a berg, to wait for the Isabella. There would be no navigating this sea but for the bergs; for, after the men have towed and warped the ship for 12 or 14 hours, she would be adrift again, and at the mercy of the ice, if you could not anchor in security to one of these enormous masses, which rests upon the ground, and perfectly secures you from every danger, except that (which has once or twice occurred to us) of drifting off with a high spring tide into deep water. A ship is almost perfectly secure from going on shore, when well anchored to them; for the smallest of them draws so much more water than any ship, that it must ground long before the ship, unless the shore immediately within it is very steep indeed. A very small ice-berg, to which we anchored

anchored on the 9th of June, was grounded in 52 fathoms, and was so firmly moored, that the levels of the dipping needle were not in the slightest degree affected.

"July 5.

"Since I last wrote, we have been incessantly occupied in attempting to get through the ice to the northward. The first stage we made was into North-east Bay, where we have been detained several days, which could only be occupied in settling the position of the several points of land, &c. and the variation of the compass, which, by the by, can never be done on board a ship with any tolerable degree of accuracy, a difference of 30 degrees arising from a change in the ship's head, on board the Isabella. On board the Alexander this difference is very apparent also, but in a much smaller degree. I do not, however, consider the experiments we have yet made to be sufficiently numerous, or sufficiently delicate, to enable us to draw any satisfactory conclusion from them on this very interesting point, till further and better opportunities offer.

"We had rather an interesting visit from two Esquimaux families the other day, but with the details of which I shall not now trouble you. In truth, I have so few moments to spare from the immediate duties which now press upon us, that I fear you will think my letter but a shabby one. These last two days have given us a run to the northward beyond our most sanguine expectation, as we are at this moment within seven miles of the northernmost of the Womn's Islands, and passed Sanderson's Hope yesterday evening.

"Our latitude, by account, to-day at noon, was 73. 10. N.; Isabella's 73. 15. long. 57. 14. W. Some of the Esquimaux from these islands were, I understand, on board the Isabella today, and report, that the place in which we now are has been clear of ice during the whole winter (is this possible?); that no whales have been here during the whole season; and that they think there is plenty of clear water to the northward. If this be true, it is delightful intelligence for us. As far as we can ourselves see, there is no reason to question the accuracy of this statement; for though the number of bergs is here, as at Riskoll, and at Waygatt Island, and Black Hook, almost beyond conception or belief, the field-ice appears to be by no means so close as to stop our progress. How long this fair prospect may continue, it is impossible to judge; but the voyage begins to acquire extreme interest, and all are anxiously looking out to the north.

"P. S.—July 6.—I have just measured the height of an ice berg, which is 123 feet and a half, and it is aground in 125 fathoms! This is literally a small one compared with some hundreds

dreds that we have seen. Feet above water, and fathoms under, seem to be the general run of their specific gravity."

"His Majesty's ship Isabella, at sea, lat. 75. 25.
long. 67. 7. variation 83. 48.—July 25.

"DEAR D——, This is our last opportunity this year, therefore I could not let it pass without writing, although nothing has passed since my last. We are now to the northward of all the ships that are fishing; we see some a long way a-stern; the boat with dispatches is going immediately to one of them; they have followed a great way this year, and have been very kind in giving us every assistance when in the ice. I sincerely wish them all safe back; they have a long way to go through the ice. The coast begins to look more and more miserable; as we get north, it has more the appearance of a chain of ice mountains than land; the sea is one solid field of ice as far as the eye can reach. When the wind blows from the north, we find narrow passages in it, and through them we pass on: sometimes the whole of our men are on the ice, dragging the ship along the edge of the flaws. From the very great variation, we cannot be a great way from the magnetic pole; you will see the variation by our last observation on the ice at the head of the letter."

MAMMOTH CAVE OF INDIANA.

The Kentucky Commentator contains a letter from a Mr. Adams, giving an account of a cave which he had explored in Indiana. The Editor of the Commentator, in his introduction to the letter, says, this cave "has never yet been fully explored, though several individuals, whose testimony is to be relied on, have penetrated from six to nine miles into this subterraneous region."

Mr. Adams states that the cave is situated in the north-west quarter of section 27, in Township No. 3 of the second eastern range in the district of lands offered for sale at Jeffersonville. It was first discovered about eleven years ago, at which time the bottom of the cave was covered with salts from six to nine inches deep; the sides were also coated in the same manner, and had the appearance of snow.

The hill in which the cave is situated is 400 feet high, the top principally covered with oak and chesnut. The entrance is about half way from the base to the summit, and the surface of the cave preserves about that elevation.

The entrance is by an aperture of 12 or 15 feet wide, and three or four feet in height: with an easy descent, you enter a room which continues a quarter of a mile, varying in height from 8 to 30 feet, and in breadth from 10 to 20; the roof arched in some places, resembling an inside view of the roof a house. At the

the extremity of this room the cave forks, the right soon terminates, the left rises by a flight of rocky stairs, nearly ten feet high, into another story, and has a S.E. direction. In this room the roof has a regular arch from 5 to 8 feet high, and from 7 to 12 feet wide, which continues to what is called the Creeping Place, where it becomes necessary to crawl 10 or 12 feet to get into the next room, from which to the distance of one mile and a quarter, there are many large and small rooms, variously situated. At the end of this journey, a stately white pillar presents itself, which is about 15 feet in diameter, and from 20 to 30 in height, regularly reeded from top to bottom. In the vicinity are several other smaller pillars of the same description. Mr. Adams was not certain what were the constituents of their columns, but lime appeared to be the base. Major Warren states that they are the satin spar.

The cave abounds in sulphate of magnesia or Epsom salts, which is found in a great variety of forms, and different stages of formation—sometimes in lumps from 1 to 10 pounds, from the surface to three feet below it,—the walls are covered with the same article. Mr. Adams removed from a spot in the cave every vestige of salt, and in four or five weeks the place was covered with small needle-shaped crystals resembling frost.

The quality of salts is very superior—the worst earth yielding four pounds to the bushel, and the best from 20 to 25 pounds.

The cave also contains great quantities of nitrate of lime or salt petre earth; nitrate of alumina, or nitrate of argil, each yielding an equal quantity of saltpetre. The sulphate of lime is seen variously formed, ponderous crystallized, soft, or light and spongy; there are also vestiges of the sulphate of iron, and small specimens of the carbonate and nitrate of magnesia. The rocks in the cave are principally of carbonate of lime or common limestone.

Mr. Adams closes his letter by stating, that near the forks of the cave are two specimens of painting, probably of Indian origin. One appears to be a Savage with something like a bow in his hand, and furnishes the hint, that it was done when that instrument of death was in use. The other is so much defaced that it is impossible to say what it was intended to represent.

PREPARATION OF HYDROSULPHURATE OF IRON. BY PROFESSOR TURTE OF BERLIN*.

"Wishing in one of my lessons to demonstrate the decomposition of water by sulphur and iron, I had in due proportions (25 and 15) mixed the filings of very fine wrought iron with

* Kastner's *Pharmacie*, p. 201.

pulverized sulphur, and by the help of water made a paste of this mixture. At the end of twenty minutes it became so sensibly hot, that the vessel containing it could not be held in the hand; and twenty minutes later it had formed itself into black sulphuret of iron. I then set a portion of it apart, and having poured muriatic acid upon it, abundance of very pure sulphuretted hydrogen gas was liberated. I put the remainder into a bottle, where it remained for many months, at the end of which it had not suffered any change, but yielded on the application of acids the same quantity of sulphuretted hydrogen gas."

Remarks on the above, communicated by Professor VAN MONS.

M. Turte has had the candour to acknowledge that hydrosulphuret of iron or sulphuret of iron by liquid process was obtained before him, by Black.

The perfect resemblance of this sulphuret to the suboxide of iron obtained by a similar process, having led me to suspect that it might consist in this suboxide mixed with sulphur, and that the hydrogen might through the medium of the sulphur proceed from the second oxidation of the iron, I mixed well, by the addition of a little water, five parts of sulphur with eleven equal parts of black suboxide of iron; and on the half of this mixture I poured some weakened muriatic acid. Not a single bubble of sulphuretted hydrogen gas was disengaged, and the mixture was not in the least degree heated. I added to the remaining half eight parts of fine iron filings, and enough of water to reduce it into paste. The mass heated, and after I had cooled it again by plunging it into cold water, it yielded copiously sulphuretted hydrogen, but only after the oxide was dissolved. The constituents of this compound are 25 of iron, 15 of sulphur, and 8·5 of water, or 32·5 of suboxide of iron, and 16 of sulphuretted hydrogen.

When solutions of submuriate of iron and of oxalate of ammonia are united, and the mixture exposed to the direct action of the sun, it forms a first muriate and carbonated muriate of ammonia, and emits carbonic gas. The mixture contains precisely the elements and the proportions of elements for these products. One half of the chlorate of corrosive sublimate passes to the carbonic oxide of the oxalic acid; whence there result mercury and phosgenic acid, which with the ammonia forms the carbonated muriate of that alkali; and the second element of the oxalic acid, which is carbonic acid, is liberated. It is of consequence here, as well as in the direct formation, that the phosgenic acid be assisted by the direct heat of the sun.

I have almost forgot to observe, that according to Thenard, the azote which is separated from the atmosphere by the aid of sulphuret of iron liquefied, is different from that which is extracted from sulphur.

AIR-

AIR-TIGHT VESSEL.

To Mr. Tilloch.

Sir,—Being but just returned from a journey through South Wales, I have had no opportunity of seeing the last number of your Magazine for April, in which my paper on the Extinction of Fires appears. Speaking of this paper you say: "The author also suggests that ships might be rendered more buoyant by making them air-tight, and forcing in air by means of an air-pump, which would elevate them to a higher level in the water, and consequently might sometimes save them *when they have got upon a bank.*"

Now by this you appear to have quite misunderstood me, for which reason I shall feel obliged if you will have the goodness in your next to state, that the object of my plan was to keep a ship afloat that would otherwise very soon sink, by confining the air she then has within her, or if necessary injecting more; by which means the influx of water would be stopped, and the ship, cargo, and many valuable lives might be saved. I am, &c.

Bristol, 25th May. John Moore.

P.S. For burnt lime *read* lime, in the last line of page 287.

SERPENTS.

The following memoir on the subject of the fascinating power of serpents, by Major Alexander Garden, of South Carolina, was read at a meeting of the New York Historical Society in September last.

"He attributed the phænomenon to an effluvium which the serpent voluntarily exhales at those times when it feels the desire of food, and the effluvium is of so deleterious a nature as to cause convulsions in the smaller and more sensitive animals, such as birds, mice, &c. He mentioned several instances in which men had been powerfully affected by this effluvium. He had been informed by the late Col. Thompson, of Belleville, that whilst riding over his estate, he came to a snake of enormous size, at which, the moment he could sufficiently collect himself, he fired. He killed the reptile, but was at the same instant assailed by an overpowering vapour, which so bewildered him that he could scarcely guide his horse home—that a deadly sickness at the stomach ensued, and a puking more violent than he had ever experienced from an emetic. He had been told by a lady, that the overseer of one of her plantations being missed, was sought for by his family, and found in a state of insensibility. On recovering, he stated that he was watching for a deer, when he heard the rattle of a snake, and that before he could remove from the threatened danger, he perceived a sickening effluvium, which

which deprived him instantly of sense. From John Lloyd, Esq. he had learned another case:—A negro working in his field was seen suddenly to fall, uttering a shriek; on approaching him, it was found that he had struck off the head of a very large rattle-snake, the body of which was still writhing. On recovering, he said that he had shrieked with horror on discovering the snake, and at the same instant had been overpowered by a smell that took away all his senses. Mr. Nathaniel Barnwell had a negro, who could, from the acuteness of his smell, at all times discover the rattle-snake within a distance of 200 feet, when in the exercise of his fascinating power, and when traced by this sense, some object of prey was always found suffering from this influence. To these facts Major Garden added some anecdotes collected from Vaillant's travels and other sources, corroborating his theory. When gorged with food the serpent is supine. It is only when under the stimulus of hunger that he exerts this fascinating faculty.

CAST-IRON BRIDGE.

It is proposed to erect a stupendous bridge over the river Forth, at Queensferry, the line of which is to begin at high-water mark, near Newhall's Inn, and is to traverse the island of Garvie, at a point, and terminate at the battery rock on the north shore. The length of the bridge will be one furlong, and its height ninety feet above the stream tide. It is to be of cast iron, upon the principle of suspension.

LECTURES.

Medical School of St. Thomas's and Guy's Hospitals.—The usual Lectures at these adjoining Hospitals, which commence the 2d of October, will be given as follows; viz.

At St. Thomas's.—Anatomy and the Operations of Surgery, by Mr. Astley Cooper and Mr. Henry Cline.—Principles and Practice of Surgery, by Mr. Astley Cooper.

At Guy's.—Practice of Medicine, by Dr. Curry and Dr. Cholmeley.—Chemistry, by 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 and Dr. Blundell.—Structure and Diseases of the Teeth, by Mr. Bell.—A Course of Clinical Lectures will be given in the Winter by Dr. Marcet:—And a Course of Practical Botany in the Spring, by Mr. Salisbury, of the Botanic Garden, Chelsea.

N. B. These several Lectures, with those on Anatomy, and on the Principles and Practice of Surgery, given at the Theatre of

St.

St. Thomas's Hospital adjoining, are so arranged, as not to interfere with each other in the hours of attendance, nor with the Medical or Surgical Practice of the Hospital: and the whole is calculated to form "A complete Course of Medical and Chirurgical Instruction." Terms and other Particulars my be learnt from Mr. Stocker, Apothecary to Guy's Hospital; who alone is empowered to receive entrance money from Pupils, for any of these Lectures delivered at Guy's Hospital.

LIST OF PATENTS FOR NEW INVENTIONS.

To Thomas Machell, of Great Ryder-street, in the parish of Saint James Westminster, in the county of Middlesex, surgeon, for his improved method of applying for medicinal purposes, the agency of atmospheric air, liquid or gaseous substances to the external surface, and to some of the internal cavities and passages of the human body, and for the more convenient and useful mode of employing oil and spirits on similar principles in lamps and other luminous apparatus.—24th August, 1818.—6 months allowed to enroll specification.

To John Bennett, of Manchester, in the county palatine of Lancaster, shopkeeper, for his certain improvements in filtering vessels, and in the filtering medium thereof.—31st August.—6 months.

To Joseph Bowyer, of Kidderminster, in the county of Worcester, carpet manufacturer, for his improvement in the machinery for making Brussels and cut pile commonly called Wilton carpeting, figured rugs, and imperial rugs.—31st August.—2 mo.

To Richard Green, of Lisle-street, Leicester-square, in the country of Middlesex, sadlers' ironmonger, for his improvement upon the spring billet for harness, and the application thereof to bridles, heads and reins, bits, sword belts, gun-springs and other purposes.—31st August.—2 months.

To William Salisbury, of Brompton, in the county of Middlesex, botanist, for a machine or implement for the purpose of preparing hemp, flax, and other vegetable fibrous substances, partly communicated to him by a foreigner in the service of His Imperial Majesty the Emperor of Russia, and partly of his own invention.—31st August.—6 months.

To Frederick Dizi, of Crabtree, in the parish of Fulham and county of Middlesex, for his discovered improvement in musical wind instruments of certain descriptions.—31st August.—6 mo.

To Henry Stubbs, of Saint James's-street, in the parish of Saint James and county of Middlesex, blind manufacturer, for his moveable heel for boots, shoes, and other purposes.—7th Sept.—6 mo.

METEORO-

METEOROLOGY.

Observations at Augsburgh, July 30. By Professor Stark.

According to the observation of the Rev. Mr. Stark, at thirty-nine minutes past 3 P.M. of yesterday, Reaumur's thermometer was in the shade at $25°\frac{6}{10}$, and one exposed to the sun $35°\frac{1}{10}$; at the same time Saussure's hygrometer indicated the highest degree of dryness or zero, and the manometer the greatest porosity (Lockerheit's) of the air $10°\frac{8}{10}$ French grains. The heat diminished only one degree till forty-three minutes past four. The positive electricity of the air was still increasing and had reached to 16°; the negative had remained for several days at zero. The evaporation of the water in the atmometer amounted to $7\frac{84}{100}$ Paris lines from a Paris square foot in twenty-four hours from the preceding day, when the thermometer was at $33°\frac{1}{2}$ in the sun, and at $24°\frac{8}{10}$ in the shade, till 4 o'clock P.M. After two thunder storms that passed off at a distance, followed by rain yesterday evening,—this morning, and at noon, the hygrometer showed at half past two the greatest moisture or 100°, after having been only twenty-three hours before at zero, in the greatest degree of dryness.

A Meteor.—Mr. Hall, Professor of Natural Philosophy in Middlebury College, United States, has published a scientific account of a meteor, of uncommon magnitude and brilliancy, seen in that vicinity on the evening of the 17th of June. To some, he informs us, the diameter appeared as large as the full moon at rising; but to others, not more than half as large. While in the heavens, it appeared to emit sparks; and some of the beholders of it say, it exploded three times with a noise like heavy thunder, or, as some represent it, like three distinct discharges of a cannon in quick succession. It had the appearance of iron in a furnace the instant it is beginning to fuse. The houses were jarred by the explosion, as they are by a slight earthquake. To one person it appeared to "roll over" in the agitation or leap, (the effect of the explosion,) and to grow less after each agitation, and shortly after the third disappeared. Many saw the light, who did not see the meteor. Its distance, and of course magnitude, have not been ascertained; and there was a difference in the computation of the time it was visible. One person thinks it was fifteen minutes after having seen the agitation, before he heard the reports; which, if correct, would have placed the meteor two hundred miles distant. Sound passes about thirteen miles in a minute. But meteors have been seen to move at the rate of one thousand miles in a minute.

The Professor is anxious to ascertain if any stones were projected from this meteor; and hopes to hear something on the subject from his friends in the eastern part of Vermont, or New Hampshire, in which direction it passed.

Meteorological Journal kept at Walthamstow, Essex, from August 15 *to September* 15, 1818.

[Usually between the Hours of Seven and Nine A.M. and the Thermometer (a second time) between Twelve and Two P.M.]

Date.	Therm.	Barom.	Wind.
August			
15	57 67	30·10	N.NW.—Cloudy & windy; distant hills hazy; wind, clouds, & gleams of sun; cloudy night.
16	58 72	30·10	N.—Wind, *cumuli*, and clear; very fine day; stars and some *cirrocumuli*. Full moon.
17	56 76	30·10	N.—Gray morn; fine day, very hot; 6 P.M. some drops of rain; clouds, clear, and *cumuli*.
18	52 72	29·99	NW—W.—Fine sunshine, and some *stratus*; fine hot day, and some wind; clear night.
19	52 67	29·95	N.NW—W.—Clear and windy; a few *cumuli* NW; *cumuli*, wind, and some sun; about 7 P.M. part of a rainbow, but not any rain; cloudy and windy.
20	53 65	30·00	NW.—Gray and windy; fine day; not much sunshine; stars and *cumuli*.
21	50 70	30·00	NW—N.—Sun and wind, and *cirrostratus*; fine day; about 7 P.M. beautiful *cirrocumuli*; night cloudy, and drops of rain.
22	51 66	30·00	NW.— Sun and *cirrostratus*, and a strong dew; very fine day; bright star-light.
23	52 67	30·20	NW—N.—Fine sun, and wind; very fine day; dark night. Moon last quarter.
24	58 69	30·15	NW—SW—NW.—Gray and windy; fine day; stars and thin *cirrostratus*.
25	55 70	30·10	NW.—W.—Clear, sun, and *cirrostratus*; fine day; *cumuli*, clear and windy; dark night.
26	51 66	30·00	SW—W.—Fine morn; sun and clouds; clouds and sun; fine day; night dark and windy.
27	56 66	29·80	W—S.—Gray, and some wind; at 10 A.M. a shower; gleams of sun, and wind; rain from about 2 P.M. to 7 P.M.; dark and hazy.
28	61 70	29·64	W—NW.—Cloudy and windy; sun and wind; fine day, and very hot; bright star-light.
29	57	29·90	S—NW.—Sun early; hazy; fine day; very hot; cloudy night.
30	64 70	29·90	W—NW.—Sun, and *cirrostratus*; set rain about 7 A.M.; fine day; sun and wind, and very warm; fine orange sunset; stars, but not very *bright*.
31	49 72	30·00	S.—Fine clear morn, but some *stratus* near the horizon; very fine hot day; clear star-light night. New moon.

September

Date.	Therm.	Barom.	Wind.
September			
1	63 73	29·60	S—SW.—Slight rain; hot, cloudy and windy; fine day; 8 P.M. drops of rain; 9 P.M. cloudy and windy; 11 P.M. bright star-light.
2	56 70	29·80	SW—NW.—Clear high, and a strong dew and haze low; very fine day; star-light and windy.
3	59 71	30·00	SW.—Hazy morn; fine day; hot; dark, windy, and hot.
4	65 74	29·95	SW.—Some rain, and windy; sun and clouds; very hot day; fine day; some drops of rain; dark and windy; rain in the night.
5	65 67	29·95	SW—SE.—Cloudy and hazy; 9½ A.M. a shower, and rain continued till about 3 P.M.; then fine till after dark; rainy evening.
6	56 67	29·70	SE—NW—SW.—Rainy till about 10 A.M.; fine day; sunshine; bright star-light, and some wind.
7	53 66	29·80	W—NW.—Clear and clouds; fine day, wind and sun; clear star-light; *aurora borealis.* Moon first quarter.
8	49 64	29·90	W—SW—SE—N—S.—Fine clear sky, and *cirrus;* fine day; sun; clouds; slight rain at 3 P.M.; at 5 P.M. dark *nimbus* and distant thunder, and slight rain; dark night early; 11 P.M. some stars and *aurora borealis.*
9	48 65	29·70	N—NW.—Very fine and clear, and *cirrus;* fine day; some set rain after 3 P.M.; star-light.
10	49 61	29·80	N—NW.—Clear, *cirrostratus,* and wind; sun and wind; fine day; cloudy night.
11	45 61	29·82	NW—W.—Perfect clear calm morning; fine day; some wind; clear stars, and some *cirrocumuli.*
12	48 64	29·95	W—NW.—Clear and windy; fine day; some drops of rain; stars, moon, *cirrocumuli,* and wind.
13	52 66	30·15	NW.—*Cumuli,* clear, and wind; fine day; moon- and star-light.
14	49 64	30·20	S—SW.—Hazy; some sun; cloudy day, but some sun; windy; cloudy night. Full moon.
15	59 68	29·90	SW.—Gray and windy; sun and clouds; rain about 2 P.M. till after 5, and then again a hard shower; cloudy night.

Errata in last month's Magazine:

23d July, *for* Barometer 72 at 10 P.M. *read* Thermometer 72 at 10 P.M., and, *for* 24th August *read* 24th July.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

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

1818	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Aug. 16	full	65·5	30·15	Cloudy
17	15	68·	30·11	Cloudy—rain P.M.
18	16	64·5	30·05	Ditto—rain A.M.
19	17	64·	30·10	Ditto
20	18	62·	30·15	Ditto
21	19	62·	30·14	Ditto—rain in the evening
22	20	60·	30·16	Ditto
23	21	64·	30·29	Fine
24	22	61·	30·21	Cloudy
25	23	63·5	30·14	Ditto
26	24	63·	30·01	Ditto—rain in the evening
27	25	66·	29·79	Ditto
28	26	63·5	29·71	Ditto
29	27	74·	30·02	Fine
30	28	66·5	29·88	Ditto
31	new	67·5	30·02	Ditto
Sept. 1	1	75·5	29·53	Ditto—showery in the morning
2	2	71·	29·86	Ditto
3	3	70·	30·09	Ditto—rain at night
4	4	74·5	29·96	Ditto do.
5	5	69·5	30·01	Cloudy—heavy rain at night
6	6	69·	29·79	Fine
7	7	62·	29·93	Cloudy
8	8	58·	29·99	Ditto
9	9	56·	29·77	Rain
10	10	55·	29·86	Fine
11	11	56·	29·90	Ditto
12	12	59·5	30·03	Cloudy
13	13	63·	30·32	Ditto—rain in the morning
14	14	66·	30·15	Ditto do. evening

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For September 1818.

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.
August 27	60	63	60	29·77	0	Rain
28	61	67	61	.90	62	Fair
29	62	76	62	30.01	66	Fair
30	60	75	59	29.93	60	Fair
31	58	69	63	·99	78	Fair
Sept. 1	67	73	60	.55	56	Fair
2	63	70	64	·90	57	Fair
3	60	70	66	·99	60	Fair
4	65	75	60	30·01	64	Fair
5	60	60	58	29·90	0	Rain
6	58	66	58	·86	47	Showery
7	57	64	55	·90	52	Fair [the evening
8	56	64	55	·90	47	Fair, thunder in
9	50	60	49	·64	49	Cloudy
10	50	58	50	·90	56	Fair
11	51	59	50	·92	49	Fair
12	50	57	51	·96	47	Fair
13	52	60	55	30·08	49	Fair
14	54	65	56	·16	45	Fair [night
15	55	65	58	29·96	46	Cloudy, rain at
16	56	66	48	·67	48	Showery
17	46	59	50	30·12	47	Fair
18	50	60	58	·05	0	Rain
19	60	64	60	29.82	0	Small rain
20	59	66	60	·59	39	Stormy
21	60	66	60	·42	30	Stormy
22	60	67	56	·67	34	Cloudy
23	58	65	57	·60	39	Cloudy
24	57	66	57	·65	37	Cloudy
25	58	64	55	·48	30	Cloudy, heavy rain
26	55	64	55	·64	99	Fair, rain at night

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

XXXIX. *On the Question "Whether Music is necessary to the Orator,—to what Extent, and how most readily attainable?"* *By* Henry Upington, *Esq.*

[Continued from p. 168.]

To Mr. Tilloch.

Blair's Hill, Cork, Sept. 16, 1818.

Sir,— You will no doubt perceive by the general tenor of my papers, but more especially by the tenor of my last, that I have aimed at little more than a comprehensive *outline* of my subject; and have therefore left to the good sense and discernment of my readers the supplying of several deficiencies which my disinclination for detail has unavoidably occasioned. Thus for example, in place of *chiefly* ascribing to the successively descending intervals with which our music abounds—the propensity of our public readers and orators to sink inaudibly through the scale at the termination of their periods; I might also have adduced the wideness of interval, extent of scale, and usually inappropriate modulation when applied to speech, with which our songs and other musical productions so frequently conclude. In speaking too of the *rhetorical cadence*, I might have added that in several cases (especially when not preceded by a pause) this cadence is less distinctively marked than in others. I might also have qualified my assertion that "the ultimate falling syllables of an ancient period could never have exceeded two," by stating the probable exception in the Roman language, of a terminating monosyllable when preceded by a word of three or more syllables whose accent is seated on the antepenult: but, as I have already said, these and several other matters of detail have been intentionally left to the good sense and discernment of the reader.

To proceed then with my inquiry. A taste manifestly vicious in the extreme having for some time publicly appeared among the propagators of novelty in this kingdom, who in addition to the hideous extension of certain syllables, and the inarticulate crowding of others—would fain violate all the chasteness of language by the introduction of a periodical *thump*, the necessary consequence of executing any passage by the *beat* of time, conformably to our present mode of barring *; my attention was, if possible, more carefully directed to the analysis of this than of any other topic. Not satisfied therefore with the coinciding opinion

* The introducers of this *barring* system are the real or pretended admirers of "*Prosodia Rationalis*," whose anti-oratorical author, Joshua Steele, would extend our ordinary speaking scale to an octave and a half; and the duration of our syllables to the monstrous ratio of *eight* to *one*—while, even

nion of my ASSOCIATE, I solicited the conjoint operation and opinion of several amateurs and professional musicians; by all of whom the following observations were unhesitatingly made.

Examination of THE SPEAKER *continued.*

OF TIME.

Observation 1st.—Alterations of the *general movement* were almost perpetual, every perceptible change of emotion producing, though even in the same clause, a corresponding acceleration or retardation of delivery; while even in the most regular clause, no appreciable *bar* whatever, with the exception of an occasional approximation to our $\frac{3}{4}$ or $\frac{3}{8}$, could be said in any way to exist. Nevertheless, a *triplet* similar to that in "God save the King" did now and then attract our attention.

Observation 2d.—Combinations more or less independent of our usual barring arrangement—and whose effect was at times peculiarly expressive—were *continually* perceived. But on these combinations we considered ourselves too inexperienced to report.

Observation 3d.—Emphatical words, the vowel of whose *emphatic* syllable was long by *nature* [*this* term is used for perspicuity], were rather frequently marked by a moderate extension of those vowels; while in the case of short vowels, similarly circumstanced, scarcely any extension of *these*, at any time took place: nevertheless, in both instances a certain extension of prolongable consonants [not mutes] was occasionally obvious.

Remarks on the preceding Observations.

The more attentively we consider the present barring system in its application to speech, the more numerous are the objections to its adoption. Besides that periodical *thump* which all time-

even in recitative, *Handel* is satisfied with *four* to *one*, and for the most part with *half* this ratio. Let us take the very first example which occurs in Steele's book, as a specimen of his taste, and conception of rhythmus.

[In executing this passage let the experimenter take care that he not only beats the time, but allots the assigned duration to every individual syllable.]

Oh	happi-ness				our		being's		end	and	aim
= 8	3	1	2	2	4	4	4	4	6	2	8

Cæsar's remark on the sing-song speaker may be justly applied to Mr. Steele: If *this* be singing, it is singing very badly. Some trifling ballad may possibly present one or two accidental lines to which, if expression be not required, this time-beating process may somewhat closely be applied; —but who would covet the execution at so great a sacrifice?

beating

beating by *forte* must inevitably produce;—besides that horrific extension and inarticulate contraction of syllables so inappropriate to oratory, which must consequently follow;—besides the impossibility, under regular time, of expressing our emotions by the immediate transitions from quick to slow and from slow to quick, to which we are instinctively prompted by those emotions*;—besides these and many other objections which may be urged with regard to *speech*—is not the musician himself aware, that in proportion as he mechanically adheres to the exact execution of his musical time, even in *song*—in that proportion must he necessarily fall short of the admirable *expression* which distinguishes the celebrated solo singer from the grosser performer?

In *recitative* (the design of which is the imitation of speech), how much more the latitude! The performer must be *heard*; and whenever superior energy and expression are intended, he must conform in a striking degree to the irregular dimensions of our syllables, articulating, with requisite length, a considerable number, which the composer, for the preservation of *imaginary* time, has represented as *very* short; and shortening a number of those, especially the particles, which the composer may have represented too long†. The performer must also, on several occasions, extend for the sake of *expression* certain notes to which too limited duration had been assigned; and he must consequently shorten others. He must likewise constantly surpass his bars by *syncopation:* he must considerably derange that order of emphasis which the habitual character of our song prescribes: he must constantly pause where the sense requires, and disregard imaginary rests: in fine—to excel in recitative, he must *unlearn*, and with no small share of difficulty, all his previously contracted time-beating habits, adopting every method which art or nature may suggest, for the annihilation of his bars, and the attainment of more appropriate expression.

Against all these objections the advocates of *barring* will plead, and apparently with reason, the necessity of some certain basis for the establishment of regular proportion, from which the performer may afterwards more or less depart, as fancy regulated by general usage shall invite him. Now with these advocates I should probably agree as to the utility of barring, [not in their way by constantly commencing forte,] were recitative not speech the ultimate object: but with regard to *speech*, whose latitude

* The ancients, whose taste in every thing that related to oratory was conspicuous, were particularly attentive to these transitions. Quintilian, in the 3d chap. of the xith book of his Institutes, points them out to the orator as indispensably connected with expression.

† Handel himself, in that superior passage which I have so often quoted, has marked equally with a semiquaver the words *a, the, I, plung'd, blow.*

compared with recitative is undoubtedly greater than that of recitative compared with song, is there not a more simple and less objectionable method of cultivating proportion, by which the speaker shall neither misspend his valuable hours in learning the mechanical operation of beating our various species of time; nor acquire, in the remotest degree, any habit that shall endanger his delivery and cost him much subsequent trouble to remove?

To this simple method our attention will speedily be directed. But although it were ever so necessary, and even *practicable*, for the orator to improve himself by speaking in barred productions—where, as I already observed, is the master capable of composing and teaching them? In what species of time, $\frac{2}{4}$ $\frac{3}{8}$, $\frac{6}{8}$, or even $\frac{5}{8}$, separately or mixed, shall they be written? And although this question which appears unanswerable should be solved—shall the orator even then sacrifice his own, and in all probability superior ideas of beauty, to the *ultimate* setting of a *Joshua Steele?*

The total *impracticability*, however, of executing speech, even with *tolerable* accuracy, in any species of musical time, is in my opinion self-evident. Nothing for the ascertainment of this fact has on my part, or on that of my ASSOCIATE and other musical assistants, been left undone. Various passages were selected, and all equally failed. At length we confined ourselves to that passage which Joshua Steele had *intentionally* chosen in the outset of his work as the foundation of his theory, "*Oh happiness, &c.*" and which I have already given as set by that fanciful gentleman. This passage was separately taken up by each individual, and set in that form which in his opinion was most analogous to the delivery of our chastest speakers, and at the same time consonant, as much as possible, with musical usage:—all these settings were compared; and after mature discussion, the following was preferred:

	Oh happiness	our being's	end and aim.
	3 1 1 1	1 2 2 1	2 1 3
Or=	12 4 4 4	4 8 8 4	8 4 12

The exact musical execution of this piece now followed: the time was regularly beat, and the relative proportion of every note observed as systematically as in *concert:* but the result was intolerable:—it was any thing but speech.

A *reader* of the superior order attended our consultation. *He* slowly *recited* the passage. Some trifling defects were observed, especially in the execution of the last word, which by over-extension produced too much the effect of an independent line without

without reference to the succeeding. These defects were remedied: the passage was practised, and finally delivered, as well as the human ear could estimate so irregular a combination, in the following proportions, the rhythmical divisions or bars being *exactly* ascertained by *viewing* an adjusted pendulum which vibrated seventy-two times in a minute. The recital of the six divisions occupied precisely five seconds of time, or six vibrations.

Oh h	appine	ss—our	being	's end—	and aim—
12	4 3 5	6 6	6 6	9 3	4 8

This unavoidable irregularity of proportion in syllables,—especially the *short* ones, which consistently with the character of language can neither be contracted nor extended, but in a very limited degree—was equally acknowledged by the ancients; Dionysius of Halicarnassus having supplied us in this respect with an interesting document unknown to the generality of our best informed. This intelligent critic, in the xvth section of his celebrated work on language, has the following passage; which proves to our satisfaction that the Greeks themselves, who possessed the most regular language ever formed by man, never entertained the chimerical notion of reciting even their *poetry* in any thing like accurate time or quantity. This passage being rather long, I shall give it a summary translation.

" It must be confessed," says Dionysius, " that the syllable is short which consists of a short vowel, suppose ο in ὁδός. Prefix to this the semivowel ῥ as ῥόδος, and the syllable remains short—not however in the same manner, as it will have a certain minute addition of time more than the former. Prefix again the mute τ, as τρόπος: this syllable will then be greater than the former syllables, and yet it remains short. Prefix a third letter, as στρόφος: and by these three audible additions it becomes still longer. The same with our long syllables: η if increased by the addition of four letters, as in σπλὴν, would certainly be rendered greater than when it consisted of the single letter. It is sufficient to say, that a short syllable differs from a short, and a long syllable from a long one; and that every short has *not* the same power, neither has every long, whether in prose or in *poetry*."

Such was the Grecian usage—such the Roman—and such must continue the necessary usage of this and of every other country.

To what purpose then, say our modern disputants, have all the regulations of imaginary quantity been established by the ancients? and have not these regulations contributed to *barbarize* both languages, particularly the Roman? These are the *general* questions of uninformed critics, and to these I shall *par-*

ticularly reply (if reply I may call it), by asking them a few questions in my turn.

1st. Have you ever known an individual who gave himself the trouble of reading either language by the outline of quantity? If not—how can you decide on the utility or inutility of those bulwarks against innovation which Grecian taste and judgement so industriously erected?

2d. Are you aware that *long* quantity does not consist in the enunciation of what is called a long vowel, but in the appropriate extension of syllables, which by the agency of vowels and prolongable consonants we are enabled to accomplish?

3d. Are you aware that syllables called by our countrymen *long*,—as the last syllable in *remove*,—can with a little practice be uttered as quickly as the last syllable in *remit?*

4th. Can you readily pronounce with considerable extension, the second syllable of *sábaoth** (emphasis or accent on the first)—or the last syllable of your own word *dedicate*, without destroying the *chaste English* character of these words? If not—learn, for I have heard it frequently done.

5th. Do you perceive, by the tendency of my two last questions, that ancient Iambics like dŏcēs may and *can* be read in *quantity*, preserving the emphasis or accent on the first syllable†?

6th. When you reflect on Quintilian's observation,—that in certain cases it required some delicacy of ear to distinguish whether

* Mr. Walker in his Classical Pronouncing Dictionary has this extraordinary note on the word *sabaoth*. "This word should not be confounded in its pronunciation with *sabbath*. *Sábaoth* ought to be heard in three syllables, by keeping the *a* and *o* separate and distinct, which it must be confessed is *not easy* to do." Not easy! wretched must be the habits of that speaker who finds it difficult.

† The character of this Iambus, when commanded, is wonderfully *martial*. I have heard the English word *cóhorts* so uttered—first syllable short; second very long—without any deviation from the usual vowel sounds: but I considered it very extraordinary that when the speaker thought proper, he could render the second syllable incomparably louder than the first, without altering what an English ear would denominate the stress or accent. This phænomenon being closely investigated, the deception was discovered: the second syllable was actually weaker than the first at its *commencement*; but having terminated in a crescendo, thus:

Co . . . < ho . . .rts

every ear was satisfied with the imaginary execution of the accent. By reversing the character, it became incredibly *soft*—as thus:

Co . . . ho . . . rts > It was more than *Italian*.

a *long*

a *long* syllable were really *so delivered*—are you not compelled to infer, that, agreeably to ancient practice, the naturally long *un*emphatic vowels were seldom extended equally with the long *emphatic* ones?

7th. Have you learned from the 14th section of Dionysius of Halicarnassus, that the *short* vowels of the ancients were not, like our short ones, incapable of prolongation without constituting a novel sound?—but that the narrowness or effeminacy [σπαδονικον] of those vowels rendered them comparatively *ineligible* for extension?

8th. Have you ever been informed that a syllable naturally short, as the first in *bodkin*, may be considered as extended by the addition of that trifling interruption which is perceptible between the *d* and *k*; in the ratio of about 4 to 3: or in other words, that this interruption or rest may, in the case of syllables equally short with *bod*, be accounted equivalent to the one-third of every such syllable?—and do you not imagine that on several occasions a delicate extension of the vowel itself*, as well as of prolongable consonants, did likewise take place, for the additional assistance of the rhythmus?

9th. Do you imagine that in reading the Classics, or even in speaking your own language, you ever iterate (in the same word) an immediately preceding consonant, as the *d* in *goddess?* And yet that it *can* be sounded, you must acknowledge by attending to your own pronunciation of the two *d*'s in *bad day, good day,* &c. You may possibly allege that such mode of pronunciation—requiring as it does the distinct delivery of every written character, and which would clearly and strongly articulate even the second syllable in *imperfection* or *tolerate*—must have rendered the Classical languages much slower than our own. I grant it. But will you insist that our speaking more slowly and intelligibly than we do, could render us more imperfect as orators, or lessen our dignity as a nation?

* In reading the ancient languages, I do not argue for any unnecessary innovation in our present manner of *sounding* all kinds of vowels, when *metre* is not in question. It is judicious, in my opinion, to conform in every possible manner to the usage of our native tongue; but certainly, if an ancient Greek had proposed the abolition of his noblest vowel ἀλφα, and the substitution of our narrow *ee* for his open ἠτα, I should compare him, and deservedly, to a musician who having in his possession five different bells, should demolish the noblest, and choke the second for the improvement of his melody. But with respect to our *rhythmus;* why not substitute the *long* vowel sound for that of the *short* in all *position* cases where the ear shall actually require such substitution? And would not the consequent melody which such reading would produce, materially influence our national elocution? Thus would the ancient Hexameter be restored to its original sublimity, and the metre of Horace, while sufficiently rough, be no longer stigmatized by the illiterate as the jargon of a Hottentot.

10th. In reading both the Greek and Latin languages, (but especially the latter,) do you regularly attend to the due extension; or in your own phraseology, do you give the *long sound* to all those vowels which are *by nature* long, regardless of their nominal length by position? And are you certain that the Grecian language with respect to its emphatic syllables (especially in its poetry, to which almost every license was extended) is regulated like the Roman *?

These are the great outlines of time or quantity (as well as forte), without the understanding and observance of which it is in my mind an impossibility to form an adequate opinion of the prosody of the ancient languages; and consequently of the effects of the various combinations which with most advantage may be introduced into our own. The idea of *barring* in the modern way, must however, to all appearance, be set down not only as destructive to oratorical delivery; but, in addition, as necessarily excluding (even by the confession of Rousseau himself) every species of combination, except that *isolated* one †, which, for the facility of keeping time in concert, such limited system would impose. Even in their *music*, the ancient latitude in the collocation of long and short was much greater than ours: and hence, for want of experience, the almost insuperable difficulty, with modern musicians, of executing with precision certain fragments which accident has preserved; such, for example, as that portion of a Pindaric ode with which we have been favoured in the *Musical Dictionary* of the before-mentioned writer.

χρυ—σε—α φορ—μιγξ Α-πολ—λω—νος &c.

In our musical way it would *bar* thus .. .. } 

But in the critical way of Dionysius of Halicarnassus it would *scan* thus } | — ◡ — | — — | ◡ — — |

* Our present conception of the *Roman* stress or accent is in my opinion invariably correct: however, in neither Greek nor Roman language does it appear in general so decisively *marked* as in our own.

† "Cette maniere d'exprimer le tems ou la mesure des notes changea entièrement durant le cours du dernier siècle. Dès qu'on eut pris l'habitude de renfermer chaque mesure entre deux barres, il fallut nécessairement proscrire toutes les espèces de notes qui renfermoient plusieurs mesures." See Rousseau's Musical Dictionary, article *Mesure.*

The opinion of Vossius too, upon this subject, has been respectfully quoted by Rousseau in the following words: "Il dit, qu'un rhythme détaché comme le nôtre qui ne représente aucune *image* des choses, ne peut avoir aucun effet; et que les anciens nombres poétiques n'avoient été inventés que pour cette fin que nous négligeons."

constituting

constituting by this judicious method, a Cretic, a Spondee, and a Bacchic,—of whose metrical characters a more *definite* notion can be formed than of those of the mixed Trochee or Iambus, which immediately lose themselves in combination.

When the Greek and Latin languages shall be rightly cultivated, and delivered as they ought; then, and not till then, may we presume to analyse the genuine character of those measures in which every poet and orator of notoriety excelled: and therefore I shall postpone, if not absolutely avoid, the intricate discussion, from the conscious difficulty of conveying my ideas in perspicuous language even when accompanied by oral exemplification. This letter on Time or Quantity must conclude then by an attempt, (and I hope not altogether a fruitless one,) finally to investigate,—on what rhythmical principle, independently of feet, the well-executed recitation of our best poetry depends.

Adhering most strictly, in the pursuit of this question, to my original design, I rejected as usual all speculative notions, and resorted to *experiment*. The *reciter* by whom "*Oh happiness*" was spoken, indulged my curiosity; and two intelligent musicians, together with my ASSOCIATE, lent me the assistance of their eyes and ears—their eyes to ascertain the *boundaries* by the movement of a pendulum (beating time with the hand being too clumsy a criterion); and ears for the subsequent measurement, in some tolerable manner, of the relative proportions.

The *poet* was next sought for, and MILTON obtained the general approbation. Half a dozen sufficiently *regular* lines of the "Paradise Lost" (the more *oratorical** ones though excelling the others in sublimity having baffled our attempts) were chosen, practised and repractised with remarkable distinctness, until every ear was pleased. Of these lines an adequate conception may be formed by the terminating one of the exordium; and this for the gratification of the reader I shall transcribe exactly as it was spoken.

[In the following experiment I have not particularly designated the *position*† syllables, such minute accuracy being too perplexing.]

Experiment.

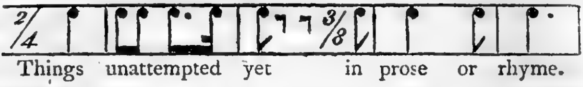
Things unat	tempted yet	—in pro	se—or rhy	me
12 7 5	10 6 8	6 6 12	4 8 12	
24	24	24	24	

* "And chiefly thou Oh! spirit" down to "mad'st it pregnant" were among this number.

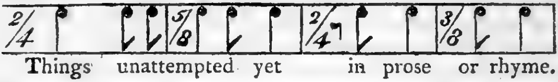
† A *position* syllable too quickly uttered cannot be considered *long*. The *dic* in *dictio*, or even the *trac* in *tractus*, may, if the speaker choose, be equalized with the shortest syllables in the Latin language.

Let

Let us reduce this passage, as nearly as we can, to some musical standard; and the *mixed* characters of time will be more conspicuous. Suppose thus:



Or even thus:



Does not this experiment throw a considerable light upon our subject? Here may be discovered that the rhythm of language is governed as it should be, by *time* and not by *noise:* that considerable deviations from all rhythmical regularity are necessary to the *sublime:* that *barring* in the ordinary way, by perpetually commencing forte, even in song, but especially in recitative, is unscientific* in the extreme: that *speech,* without torturing the character of words, has its own proportions, employs its syncopations, and commands its crescendo as well as diminuendo partitions: that *silence,* according to ancient conception as well as modern experience, must necessarily constitute a portion of the rhythmical whole: and finally, that *time* of the common, triple, and even quintuple character is so frequently and peculiarly blended, that no musical annotation can represent it.

What argument can our speech-barring advocates oppose to such undeniable facts? Of these gentlemen, then, I shall for this time take my leave, by obtruding upon their notice an established maxim of my ASSOCIATE, who was literally born a musician; that as the most grovelling of all musical performers is the country fiddler who employs the agency of force to designate his bars—so the most contemptible of all reciters is *he* who marks the boundary of his measures by the instrumentality of accent.

[To be continued.]

* Slovenly and imperfect too in its result—a certain *habitual crowding* of the several integral parts within the given boundary, and not the *relative proportions* of those integral parts themselves, being the principal requisite for the preservation of *modern* time. In proof of this assertion, deprive any ordinary tune which the musician has not previously heard, of his perpendicular guides called *bars*—and so far from playing such tune in *concert,* he will be incapable of playing it at all. ROUSSEAU has given us, under the article *Barres,* a curious anecdote confirmatory of my assertion: "Auparavant la musique étoit *simple:* cependant j'ai vu nos meilleurs musiciens embarrassés à bien exécuter l'ancienne musique d'Orlande et de Claudin. Ils se perdoient dans la mesure, faute des *barres,* et ne suivoient qu'avec peine des parties chantées autrefois couramment par les musiciens de Henri III. et de Charles IX."

XL. *A Method*

XL. *A Method of determining the specific Heat of Bodies from their Expansion.* By *Mr.* THOMAS TREDGOLD.

To Mr. Tilloch.

SIR, — THE properties of matter are generally divided into two classes, that have been termed Mechanical and Chemical: and however important the connexion between these properties may be, they have not been compared with that attention which the progress of science appears to require. Perhaps the distinction of science into Mechanical and Chemical is not favourable to such comparisons; and on that account it would be well if it were less marked than it is. The distinction has, however, been gradually lessening, and it is to be hoped will soon disappear altogether; for, in as far as they can be considered as sciences, they are founded on the same principles. In both departments it must be gratifying to every lover of science to find so many points determined by independent experiments, which serve as land-marks to keep theoretical inquiry within its proper limits.

In every inquiry respecting the properties of bodies it is desirable to show the dependence of these properties upon one another; indeed it is evident that the essential distinctions of the elementary particles of matter are few, notwithstanding its apparent diversity of forms and properties; consequently these forms and properties must be dependent on one another.

At present I will attempt to show that the specific heat of bodies may be derived from their expansion. But, as I may assume as axioms some properties of heat that are not fully established, it will be preferable to state them.—I consider heat to be a real substance, possessing weight, magnitude, attraction, &c. but its specific weight so small that it cannot be determined by our instruments. Also, that when heat is added to another body, unless it causes a change of state in the body to which it is added, it does not combine, but the compound remains what may be termed a mechanical mixture.

Let m be the magnitude of any body; and S the weight of a cube of the same body whose side is unity. Also, let h be any magnitude of heat. Then the magnitude of the body added to that of the heat will be $=m+h$; and let S' be the weight of a cube of the compound, the side of which is unity. Then, because the weight of the heat is insensible; $(m+h)\,S'=mS$; and the expansion is equal to the bulk of heat added.

Hence $\frac{m(S-S')}{S'} = h =$ the magnitude of the heat when the specific weights are known.

And $\frac{mS}{m+h} = S' =$ the specific weight of the expanded body.

From

From this view of the subject we may proceed to find the quantities of heat necessary to produce a given change in the temperature of bodies, or what is called the *specific heat.*

It is obvious that, if the principles I have advanced be correct, the quantity of expansion at any temperature will be equal to the quantity of heat required to raise the body to that temperature. Let the specific heat of the body fixed upon as a standard be denoted by unity, or 1; and its expansion for any given change of temperature =E: also let e be the expansion of the same magnitude of any other body for the same change of temperature. Then

$$E : e :: 1 : \frac{e}{E} = \text{the specific heat of a body of the same bulk.}$$

Consequently, if the same body be the standard of specific heat and specific gravity; and S be the specific gravity of the body of which the expansion is e;

$$\frac{e}{SE} = \text{the specific heat of a body of the same weight.}$$

In the annexed table it will be seen that the specific heats, according to this rule, do not agree very well with those obtained by other modes of calculation; for it is to be remembered that it is not a case that can be submitted to direct experiment. In the case of mercury the results are very near. The expansion of the different bodies is from the tables in Thomson's Chemistry, vol. i. and specific heats, in the last column, are from the same volume, pages 112 and 113.

Table of the specific Heat of Bodies.

Substance.	Expansion in bulk.	Spec. heat of bodies of the same bulk.	Spec. heat of bodies of the same weight.	Spec. heat of bodies of the same weight:—from Thomson's Tables.
Water	0·0466	1·0000	1·0000	1·0000
Air	0·375	8·047	6662·8	
Alcohol	0·11	2·36	2·888	0·76
Fixed oils	0·8	1·716	1·8	0·528
Mercury	0·02	0·429	0·0318	0·033
Zinc	0·0088	0·1888	0·026	0·0943
Glass	0·0024	0·0515	0·0215	0·187
Tin	0·00651	0·1397	0·0177	0·068
Lead	0·00855	0·1835	0·0161	0·050
Brass	0·00588	0·11974	0·0143	0·1123
Copper	0·00516	0·1107	0·0125	0·111
Iron	0·00366	0·7855	0·01	0·13
Cast iron	0·00333	0·7146	0·01	
Hardened steel	0·00323	0·6931	0·0088	

Granting

Granting that the principles I have assumed are correct, it appears that the expansion expresses the real quantity of heat necessary to produce a given change of temperature, and the ratio of the specific weights of the ultimate particles of bodies being known, the absolute quantity of heat in any body might be ascertained.

If the expansion do not express the real quantity of heat, to what cause is the increase of bulk to be attributed? On the other hand, the experiments that have hitherto been instituted for the purpose of determining the specific heat of bodies, must be shown to be founded on erroneous principles, before the view of the subject, now submitted, can be considered correct. To discuss this point, it will be necessary to consider the nature of the changes produced in bodies by the action of attraction or affinity.

Solids and fluids, whenever they are exposed to gaseous bodies, always absorb a portion of them, and gaseous bodies absorb one another; in the same manner as heat is absorbed by all bodies. But the portion of gas absorbed varies, as the temperature; consequently a specific quantity of gas is essential to fluids and solids in their natural state.

Now, unless the attraction between the particles of the body be less than its attraction for the particles of the one it absorbs, no chemical change will take place in the absorbent; but the absorption will continue till the forces are in equilibrio. If the heterogeneous attraction exceed the homogeneous, then a new combination forms, and the bodies unite in definite proportions; and whatever tends to lessen the homogeneous attraction facilitates this change.

Water is the body that has been generally used to obtain the specific heat of bodies: but water holds a considerable portion of gas in its pores, and the quantity appears to be inversely as the temperature. Hence it is that water has a *maximum* of density, which has been considered an anomaly in the law of expansion in consequence of having overlooked this circumstance. The absorption of gases by other fluids appears in like manner to be the cause of the irregularity of their expansion.

The mean temperature of any mixture will be influenced by the same cause, and therefore the specific heats determined by such methods will be incorrect.

If two portions of water of different temperatures be mixed, the temperature of the mixture will be nearly an arithmetical mean between those before mixture, because the specific gas is the same at all temperatures, in the same manner as the specific heat. But when any other body is mixed with water of a different temperature, the specific gas, as well as the specific heat, has an influence

influence on the mean temperature of the mixture; therefore, unless the specific gas be known, the specific heat cannot be determined from the mean temperature.

XLI. *Conjectures concerning the Cause, and Observations upon the Phænomena, of Earthquakes; particularly of that great Earthquake of the first of November* 1755, *which proved so fatal to the City of Lisbon, and whose Effects were felt as far as Africa, and more or less throughout almost all Europe; by the Rev.* JOHN MICHELL, *M.A. Fellow of Queen's College, Cambridge.*

[Continued from p. 195.]

37. THIS frequency of subterraneous fires, in the neighbourhood of volcanos, will appear still more probable, if we consider the internal structure of the Earth; and, as it will be necessary also, in order to understand what follows, to know a little more of this matter, than what falls under common observation, I shall endeavour to give the reader some account of it.

38. The Earth then (as far as one can judge from the appearances) is not composed of heaps of matter casually thrown together, but of regular and uniform strata. These strata, though they frequently do not exceed a few feet, or perhaps a few inches, in thickness, yet often extend in length and breadth for many miles, and this without varying their thickness considerably. The same Stratum also preserves a uniform character throughout (*a*), though the strata immediately next to each other are very often totally different. Thus, for instance, we shall have, perhaps, a stratum of potters clay; above that, a stratum of coal; then another stratum of some other kind of clay; next, a sharp grit sand-stone; then clay again; next, perhaps, sand-stone again; and coal again above that: and it frequently happens, that none of these exceed a few yards in thickness. There are, however, many instances, in which the same kind of matter is extended to the depth of some hundreds of yards; but in all these, a very few only excepted, the whole of each is not one continued mass, but is again subdivided into a great number of thin laminæ(*b*), that seldom are more than one, two, or three feet thick, and frequently not so much.

39. Be-

(*a*) It is not clear, that Mr. Michell in this passage, meant to speak strictly, or of *mineralogical* characters only; see my note on § 47, and the 9th Item of Mr. Smith's Claims, Phil. Mag. vol. li. p. 177.—J. FAREY.

(*b*) It does not clearly appear, whether Mr. M. here speaks exclusively of thin Strata, or beds parallel or nearly so, to the general stratification; or whether

39. Beside the horizontal division of the earth into strata, these strata are again divided and shattered by many perpendicular fissures, which are in some places few and narrow, but oftentimes many, and of considerable width. There are also many instances, where a particular stratum shall have almost no fissures at all, though the strata both above and below it are considerably broken: this happens frequently in clay, probably on account of the softness of it, which may have made it yield to the pressure of the superincumbent matter, and fill up those fissures which it originally had (*c*); for we sometimes meet with instances in mines, where the correspondent fissures in an upper and lower stratum are interrupted in an intermediate stratum composed of clay, or some such soft matter.

40. Though these fissures do sometimes correspond to one another in the upper and lower strata, yet this is not generally the case, at least not to any great distance: those clefts, however, in which the larger veins of the ores of metals are found, are an exception to this observation; for they sometimes pass through many strata (*d*), and those of different kinds, to unknown depths.

41. From

whether he had noticed, and meant to include herein, the thin laminæ, highly inclined to the real strata, and called by me *stratula*, which sometimes occur in the Sandstones of the Coal-measures and are very frequent in the coarse Slate districts. (Derby. Report, i. 155.)—J. F.

(*c*) In this 39th section, and the beginning of the 40th, it is to be observed, that Mr. M. is speaking exclusively of *joints* or *cutters*, dividing the individual thin strata or beds, into separate blocks, and not of Mineral Veins (§ 40), or of Faults (§ 50); although, like as with the latter, he supposes, that these joints passed at one period through some of the contiguous strata or beds, but have since disappeared from the softer ones, by the pressure of the incumbent beds, having again closed up the joints in them: this latter, however, appears an unnecessary and unwarrantable supposition: the face of hardly any deep Quarries or of Cliffs can be contemplated, without perceiving, that scarcely any two, even of the adjacent beds of Stone, have joints corresponding with each other, either in the width of open fissure, or in their places; evidently, because the individual beds, have, by their own peculiar degree of contraction or shrinking, opened these joints, in various and independent degrees; and very frequently, Strata or Beds may be found, between others intersected by joints, which have themselves suffered no lateral contraction, and are, and evidently always have been, without any joints: this is very common with strata of Clay between others of Limestone, and between Sandstones, with Basaltic Strata interlaying Limestones, like those of the Peak-hundreds of Derbyshire, &c.—J. F.

(*d*) Although Mineral Veins, including Dykes or those solidly filled with Basalt or other stony matters, pass in most cases "through many strata," their fissures will, on a careful examination appear to have been, not less certainly opened by mere shrinking, than the joints spoken of in my last Note: the Vein Fissures, appear to have preceded the joints, in the order of opening, and to have resulted from causes or tendencies to shrink, more extensive and symmetrical, as is evinced by the length, parallelism and width of

41. From this constitution of the earth, viz. the want of correspondence in the fissures of the upper and lower strata, as well as on account of those strata which are little or not at all shattered, it will come to pass, that the earth cannot easily be separated in a direction* perpendicular(*e*) to the horizon, if we take any considerable portion of it together; but in the horizontal direction, as there is little or no adhesion between one stratum and another, it may be separated without difficulty.

42. Those

of many Veins, and less influenced by the peculiar qualities of the shrinking mass, than is apparent with the individual Beds, as shown by the number and widths of their present joints; and yet, it is perfectly evident, that the shrinking which opened Mineral Veins, was in some degree influenced by the peculiar degrees of shrinkage of the several Strata, through which they have passed: this will be apparent, by consulting the 2d figure in p. 158, and p. 69, of Mr. Westgarth Forster's "Treatise on a Section of Strata;" the Durham and Northumberland Veins, being wider, and *hade* less, where they descend through Limestone, than through Shale: the limestone having first shrunk, and in greater degree, than the shale, which was perhaps in some measure torn, or rent by the retiring limestone, above and beneath it: as seems evidently, though in lesser degrees, to have happened, with the Derbyshire Toadstones.—Report i. 246.—J. F.

* What I said before of those deep clefts, in which metals are found, will not affect this conclusion; for they are considerably different from either perpendicular or plane sections of earth; they are frequently interrupted by strata of clay, or other soft matter; and they are, in most parts, either filled up with rubbish, or with ores and spars, that adhere as firmly to the rocks on both sides, as if they composed one continued stratum with them.

(*e*) Besides having in the above note, laid too much stress on the firm adherence of the Spars and Ores in mineral Veins, to their cheeks, Mr. Michell seems not to have considered, in this part of his statement and argument, that the centre part of most Veins adhere, or connect the Skirts together, *but slightly*, and that often, Tick-holes or empty lenticular cavities occur, to locally separate them altogether: Mr. M. seems here to have overlooked the influence of *Faults*, in almost totally destroying *the cohesive strength of the Earth*, for resisting a separation in a direction parallel to the horizon, by fissures nearly perpendicular thereto: his notice of the grand and instructive phænomena of *Faults*, in § 50, is indeed so slight,(founded chiefly) it would seem, on Mr. Strachey's example in the Phil.Trans. No.360, as to excite my surprise, especially, as in § 50, he admits them to be *of very common occurrence.*—J. F.

Mr. Smith having communicated to me, nothing material regarding *Faults* (P. M. xxxvii. p. 441), I set my mind with energy 11 or 12 years ago, to investigate them for myself, and have to my own satisfaction succeeded, and to that of many, in late years, of the *practical* Agents of my Employers, and others of the greatest experience, with regard to the complicated operations and effects of Faults, on particular mineral Fields, in various situations: my investigations show, that the Faults of a district, all connect together like a net-work on the surface, and have their sides polished, by the most violent friction, many times repeated; indicating, as I conclude (see Phil. Mag. xliii. p. 183) that the whole mass of the Earth is broken through, in all possible directions, by these mechanical fissures called Faults, and that the innumerable angular fragments, into which the mass is divided thereby, have

for

42. Those fissures which are at some depth below the surface of the earth, are generally found full of water; but all those that are below the level of the sea, must always be so (*f*), either from the oozing of the sea, or rather of the land waters between the strata.

43. The strata of the earth are frequently very much bent, being raised in some places, and depressed in others, and this sometimes with a very quick ascent or descent; but as these ascents and descents, in a great measure, compensate one another, if we take a large extent of country together, we may look upon the whole set of strata, as lying nearly horizontally. What is very remarkable, however, in their situation, is, that from most, if not all, large tracts of high and mountainous countries, the strata lie in a situation more inclined to the horizon, than the country

for a long period of time been in frequent motion, sliding by the side of each other; apparently for effecting, the constant change of the spheroidal figure of equilibrium, which an enormously great *tidal action* had occasioned (the excess of which action probably occasioned these fissures in the first instance); and the cessation of which action (at a period antecedent to *the creation* of the first progenitors of each now-living kind of organized Beings) has left these polished fragments of the Earth's mass, *scarcely if at all adherent to each other*, otherwise than by their mutual and conjoined attraction. This last, being a matter of frequent, and I believe of unvarying observation, by Colliers, Miners, Quarriers, Canal-diggers, &c. &c. wherever Faults are explored beneath the surface, it seems to me, materially to invalidate, all that Mr. M. and Mr. Whitehurst since, and others have written, as to *the strength and toughness*, of continuous strata of Clay, for confining down elastic or fluid matters, within the Earth; but so near to *its surface*, as to be visible thereon, by waves, or undulatory motions thereof.—J. F.

(*f*) Mr. Michell's assertion here, is much too general, as so acute a reasoner could not have failed of perceiving, if he had attended to the well-known fact, in all Coal districts, and I believe in all Mine districts likewise, in Great Britain, viz. that the Fault-stuff is *there*, invariably *Water-tight*, and, together with the unfissured argillaceous Strata, interlaying the porous or fissured Sandstones and Limestones, effect so complete *an exclusion of surface Water*, that it seems evident, with more provident management, as to not cutting through the Faults below-ground, and not sinking Shafts, or driving stone Levels without water-tight linings or floors, almost all deep Coal-pits and many Mines, *would be dry* in all their deepest parts, even far below the surface-level of the Sea, as great numbers of instances prove.

Hundreds of intelligent *practical* Men, well aware of the facts above stated, concur fully I believe with me, in viewing these breakings and dislocations of the Strata by Faults, as amongst the most evidently beneficent provisions of the Deity, for the use of Man!: and yet, in the present year, the Rev. Thomas Gisborne has put forth a Book, intended for the instruction of young Divines, wherein a voluminous mass of extracts is made, through 130 of its pages, from various Writers on Geology, from which the very contrary of the above position is deduced. The author intimates (in p. 94) that but *for the use of Sinners*, the Iron and the Coals would *not* country itself, the mountainous countries * being generally if not always, formed out of the lower strata of earth. This situation of the strata may be not unaptly represented in the following manner. Let a number of leaves of paper, of several different sorts or colours, be pasted upon one another; then bending them up together into a ridge in the middle, conceive them to be reduced again to a level surface, by a plane so passing through them, as to cut off all the part that had been raised; let the middle now be again raised a little, and this will be a good general representation of most, if not of all, large tracts of mountainous countries, together with the parts adjacent, throughout the whole world † (g).

44. From

have been arranged as at present!," the beds of Coal and the metallic veins are (he says) deeply stationed below the surface of the Earth," and through these dislocations, are "in every mode of confusion!"

Although in the Coal and Mine districts, where minerals *useful to Man*, lie deep beneath the surface, water-tight Faults are numerously supplied by the all-wise Creator, for facilitating their extraction; yet in other districts, where no such *use to Man* could result from deep excavations, like as in the chalk district above which London stands, there *the Faults are not water-tight*, as is evinced, by the inexhaustible supplies of pure Water which find their way through the chalk, from the parts thereof lying high and bare, beyond the edges of the London Clay, for such great distances under this water-tight covering of the Chalk, and ready, by the sagacity and industry of Man, to be let up, in *over-flowing Wells*, in almost any of the vast space covered by this Clay, between Newbury on the W. and Canterbury and Cromer on the E.: and the same again, near the Sea, in Lincolnshire and Yorkshire.—J. F.

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* It seems very probable, from many appearances, not only that the mountainous countries are formed out of the lower strata of the earth, but that sometimes the highest hills in them are formed out of strata still lower than the rest, which, perhaps, may always be the case, where they have volcanos in them. [See a representation of this in Plate IV. fig. 3.] In other instances, however, it often happens, that the hills, to which these high lands serve as a base, are not only formed out of the strata next above them, but they stand, as it were, in a dish, as if they had depressed the ground, on which they rest, by their weight.

† Fig. 1. represents a section of a set of strata, lying in the situation just described: the section is supposed to be made at right angles to the length of the ridge, and perpendicular to the horizon.

(g) The remarkably correct view of the *ridged* and *troughed structure* of the crust of the Earth, given by Mr. Michell in this passage, and in fig. 1. of his Plate (see the 4th Plate annexed), is not merely applicable to the vicinity of Mountains, properly so called, such as range through Wales, but it applies to every part of the surface of Great Britain; Mr. Smith, many years ago (without knowing, more than myself, what Mr. Michell had written) discovered, not that particular class of the Strata Ridges and Troughs, which

44. From this formation of the earth, it will follow, that we ought to meet with the same kinds of earths, stones, and minerals, appearing at the surface, in long narrow slips, and lying parallel to the greatest rise of any long ridges of mountains; and so, in fact, we find them. The Andes in South America, as it has been said before, have a chain of volcanos, that extend in length above 5000 miles: these volcanos, in all probability, are all derived from the same stratum*. Parallel to the Andes, is the Sierra, another long ridge of mountains, that run between the Andes and the sea; and "these two ridges of mountains run within sight of one another, and almost equally, for above a thousand leagues together†," being each, at a medium, about twenty leagues wide. The gold and silver mines wrought by the Spaniards, are found in a tract of country parallel to the direction of these, and extending through a great part of the length of them.

45. The same thing is found to obtain in North America also. The great lakes, which give rise to the river St. Laurence, are kept up by a long ridge of mountains, that run nearly parallel to the eastern coast. In descending from these towards the sea the same sets of strata ‡, and in the same order, are generally met with throughout the greatest part of their length.

46. In Great Britain, we have another instance to the same purpose, where the direction of the ridge(*h*) varies about a point from due north and south, lying nearly from N. by E. to S. by

which lie *parallel* to the mountain range, of which alone Mr.M. speaks, but others, ranging almost at right angles to these, in the middle and southern and eastern parts of England, Phil. Mag. xxxix. p. 271 Note.

It appears essential, towards the tracing and usefully understanding of *the subficial structure* of any district of Country, that all its Strata Ridge and Trough Lines, should be surveyed and mapped; I have accomplished a great deal of this, with regard to Derbyshire and its environs, to the northern parts of Wales (Phil. Mag. vol. xlv. p. 165), to a considerable portion of the south of Scotland, &c., and Mr. Smith also has much in store hereon; but while publication remains gratuitous, only to a particular Party, and expensive to other labourers in the Geological Field, these labours must necessarily be suspended.—J. F.

* See the notes to art. 36 and 53. See also fig. 3.
† See Acosta's Natural History of the Indies.
‡ See Lewis Evans's Map and Account of North America.

(*h*) In adverting so pointedly to a single mountain Ridge, through the western side of Great Britain, Mr. Michell is evidently mistaken: the Welch mountains of Coarse Slate, &c. present 3 or 4 almost parallel strata Ridges and Troughs, of great length, and the same is the case with the middle and southern parts of Scotland: the Strata Ridges do not always range through the centre of mountain masses—witness the Edinburgh Strata Ridge, which, though occasioning the Pentland Hills, does not pass through them (and others by W.* There are many more instances of this to be met with in the world, if we may judge from circumstances, which make it highly probable, that it obtains in a great number of places, and in several they seem to put it almost out of doubt.

47. The reader is not to suppose, however, that, in any instances, the highest rise of the ridge, and the inclination of the strata from thence to the countries on each side, is perfectly uniform; for they have frequently very considerable inequalities, and these inequalities are sometimes so great, that the strata are bent for some small distance, even the contrary way from the general inclination of them. This often makes it difficult to trace the appearance I have been relating, which, without a general knowledge of the fossil bodies (*i*) of a large tract of country, it is hardly possible to do.

48. At considerable distances from large ridges of mountains, the strata, for the most part, assume a situation nearly level; and as the mountainous countries are generally formed out of the lower strata, so the more level countries are generally formed out of the upper strata of the earth.

49. Hence it comes to pass, that, in countries of this kind, the same strata are found to extend themselves a great way, as well in breadth as in length: we have an instance of this in the chalky and flinty countries of England and France, which (excepting the interruption of the Channel, and the clays, sands, &c. of a few counties) compose a tract of about three hundred miles each way.

50. Besides the rising of the strata in a ridge, there is another very remarkable appearance in the structure of the earth, though a very common one; and this is what is usually called by miners, the trapping down of the strata; that is, the whole set of strata on one side a cleft are sunk down below the level of the corresponding

others of Plutonic and Geognostic celebrity), but up the vale of the Leith, nearly, completely on their north-western side.—J. F.

* Of this I could give many undoubted proofs, if it would not too far exceed the limits of my present design, and which, for that reason, I am obliged to omit.

(*i*) Mr. Smith's detractors would fain make it out, that Mr. Michell here meant *organized remains*: if this were in the least apparent, I would not hesitate an instant, in giving him the praise due to so important a suggestion: plainly however, Mr. M. intimates, that the requisite knowledge of the "fossil bodies," of whatever kind, *was not then possessed by him*:—my highly-injured Friend Mr. Smith, did possess the useful knowledge, and was liberal *in communicating it*, of the unorganized and organized bodies "of a large tract of country," years before Geognosts, or any of their hand-specimen mineralogical Theory of the whole Earth, was to be heard or read of in this country.—J. F.

sponding strata on the other side. If, in some cases, this difference in the level of the strata, on the different sides of the cleft, should be very considerable, it may have a great effect in producing some of the singularities of particular earthquakes*.

Part II.—51. In the former part of this essay, I have recounted some of the principal appearances of earthquakes, as well as those particulars in the structure of the earth, upon which I suppose these appearances to depend. From what has been already said, I think it is sufficiently manifest, that, in some instances at least, earthquakes are actually produced by subterraneous fires: it now, therefore, remains to be shown, how all the appearances above-recited, as well as many other minuter circumstances attending earthquakes, may be accounted for from the same cause.

Section I.—52. The returns of earthquakes in the same places, either at small or large intervals of time, are very consistent with the cause assigned: subterraneous fires, from their analogy to volcanos, might reasonably be supposed to subsist for many ages, though we had not those instances already mentioned †, which put the matter out of doubt. And, as it frequently happens, that volcanos rage for a time, and then are quiet again for a number of years; so we see earthquakes also frequently repeated for some small time, and then ceasing again for a long term, excepting, perhaps, now and then some slight shock. And this analogy between earthquakes, and the effects of volcanos, is so great, that I think it cannot but appear striking to any one, who will read the accounts of both, and compare them together. The raging of volcanos is not one continued and uniform effect; but an effect, that is repeated at unequal intervals, and with unequal degrees of force: thus, for instance, we shall have, perhaps, two or three blasts discharged from a volcano, succeeding one another at the interval of a few seconds only: sometimes the intervals are of a quarter of an hour, an hour, a day, or perhaps several days. And as these intervals are very unequal, so is the violence of the blasts also: sometimes stones, &c. are thrown, by these blasts, to the distance of some miles; at other times, perhaps, not to the distance of a hundred yards. The same difference is observed in the intervals and violence of

* Fig. 2. represents a section of the strata trapping down after the manner just described. The section is supposed to be made perpendicularly to the horizon, and at right angles to the direction of the cleft: an instance of this kind, amongst the coal mines of Mendip in Somersetshire, is mentioned in the Phil. Trans. See the account of it, together with a drawing, in No. 360, or Jones's Abr. vol. iv. part ii. p. 260.

† See art. 28 to 32 inclusive.

the shocks of earthquakes, which are repeated at small intervals for some time.

SECTION II.—53. The great frequency of earthquakes in the neighbourhood of burning mountains, is a strong argument of their proceeding from a cause of the same kind: and the analogy of several volcanos lying together in the same tract of country, as well as new ones breaking out in the neighbourhood of old ones, tends greatly to confirm this opinion; but what makes it still the more probable, is that peculiarity in the structure of the earth, already mentioned. I observed before, that the same strata are generally very extensive, and that they commonly lie more inclining from the mountainous countries, than the countries themselves: these circumstances make it very probable, that those strata of combustible materials *, which break out in volcanos on the tops of the hills, are to be found at a considerable depth under ground in the level and low countries near them.

* It has been imagined by some authors, that volcanos are produced by the pyrites of veins, and that they do not owe their origin to the matter of strata. In order to prove this, it is alleged, that volcanos are generally found on the tops of mountains, and that those are the places in which veins of pyrites are generally lodged. This argument being taken from observations that have their foundation in nature, ought not to go unanswered. In the first place, then, the pyrites of veins, or fissures, are not found in sufficient quantities, or extending to a sufficient breadth, to be supposed capable of producing the fires of volcanos: it very rarely happens, that we meet with a vein or fissure five or six yards wide; and when we meet with such an one, yet, perhaps, not a twentieth part of it at most shall be filled with pyrites; but the fires of volcanos, instead of being long and narrow, as if the matter that supplied them was deposited in veins, are generally round, and of far greater breadth than veins can be supposed to be. Mons. Bouguer says, that the mouth of the volcano Cotopaxi is, at this time, five or six hundred fathoms wide; [see Hist. and Phil. of Earthquakes, p. 195.] and the burning island that was raised out of the sea near Tercera, as before mentioned, was almost three leagues in diameter, and nearly round. [See art. 29.]

Besides this, it is very difficult to conceive how any matters lodged in veins can ever take fire; for, excepting where the veins are extremely narrow, they are almost always drowned in a very great quantity of water, which has free access to every part of them (*k*): neither are the pyrites of veins, by any means, so apt to take fire of themselves, as those of strata; and if, indeed, there are any of them that will do so, yet they are but few in comparison of those which will not: all those, which, beside iron and sulphur, contain copper, or arsenic, even in a very small proportion, are not at all subject to inflame of themselves. On the other hand, most of the pyrites of strata, if not all of them, have this property more or less. There are also two sorts of strata, in which pyrites are lodged in the greatest abundance, that have the same property, and that frequently in as great a degree as themselves: these are coals and aluminous earths, or shale. There are some kinds of both these, that upon being exposed to the external air for a few months, will take fire of themselves, and burn. These two sorts of

(*k*) See Note *f* in p. 257.—J. F.

them. If this should be the case, and if the same strata* should be on fire in any places under such countries, as well as on the tops of the hills, all vapours, of whatsoever kind, raised from these fires, must be pent up, unless so far as they can open themselves a passage between the strata; whereas the vapours raised from volcanos find a vent, and are discharged in blasts from the mouths of them. Now, if, when they find such a vent, they are yet capable of shaking the country to the distance of ten or twenty miles round, what may we not expect from them, when they are confined? We may form some idea of the force and quantity of these vapours from their effects: it is no uncommon thing to see them throw up, at once, such clouds of sand, ashes, and pumice stones, as are capable of darkening the whole air, and covering the neighbouring country with a shower of dust,

of strata are also near akin to each other; they are generally found to accompany each other; they are both of them generally intermixed with, or accompanied by strata of iron ore; and they both of them, for the most part, either contain, or are lodged amongst, the remains of vegetable bodies, and these remains of vegetable bodies in the aluminons earths, are frequently either wholly, or in part, converted into pyrites, or coal, or both. Numberless instances of this are to be met with in the aluminous shale of Whitby and other places.

It is very probable, that to some stratum of this kind the fires of volcanos are owing; and this seems to be confirmed by the similarity of the materials, which are thrown up or sublimated by the fires of volcanos, to the matter of the aluminous earths. Solfatara produces sulphur, alum, and sal ammoniac. The two former of these are very easily to be obtained from the aluminous earths, and, I suppose, the latter also; at least it is procurable from the soot of common fossil coals, and probably, therefore, from the soot of that coaly matter which is intermixed with such earths.

The aluminous earths, moreover, not only have several strata of iron ore lying in them, but they also contain a considerable proportion of iron in their composition. In correspondence to this, we find the lavas of volcanos, and other matters thrown out from thence, frequently containing a great deal of iron, the small dust of them readily adhering to the magnet.

As to the pyrites of veins, I much doubt whether they ever contain alum, or sal ammoniac; at least they are very rarely found to contain either the one or the other.

* It may be asked, perhaps, why a stratum liable to take fire in some places, should not take fire throughout the whole extent of it. In answer to this, it may be said, that the same stratum may differ a little in the richness of its combustible principles in different places; or, perhaps, the frequency of the fissures, either in the combustible stratum itself, or the stratum next to it, may let in so much water, as to prevent its taking fire, excepting in a few places: but, if this once happens, the fire will not easily be put out again, but it will spread itself, notwithstanding the fissures that lie in its way, though they are filled with water; for the matter on fire will be, in some degree at least, in a fluid state; and, for this reason, it must necessarily expel the water from the fissures, both on account of the extension of its own dimensions by the heat, and of the weight of the superincumbent earth, which, pressing it, will make it spread laterally.

&c. to some miles distance: great stones also, of some tons weight, are often thrown to the distance of two or three miles by these explosions: and Mons. Bouguer tells us, that he met with stones in South America, of eight or nine feet diameter, that had been thrown from the volcano Cotopaxi, by one of these blasts, to the distance of more than three leagues *.

54. If we suppose that these vapours, when pent up, are the cause of earthquakes, we must naturally expect, from what has been just said, that the most extensive earthquakes should take their rise from the level and low countries; but more especially from the sea, which is nothing else than waters covering such countries. Accordingly we find, that the great earthquake of the 1st of November 1755, which was felt at places near three thousand miles distant from each other, took its rise from under the sea; this is manifest, from that wave which accompanied it, as shall be shown hereafter. The same thing is to be understood of the earthquake that destroyed Lima in the year 1746, which, it has been said, was felt as far as Jamaica; and, as it was more violent than the Lisbon earthquake, so, if this be true, it must, in all probability, have been more extensive also. There have been many other very extensive earthquakes in South America: Acosta says, that they have been often known to extend themselves one, two, or three hundred, and some even five hundred leagues, along the coast. These have been generally, if not always, attended with waves from the sea; but any minuter circumstances accompanying them are not related. Indeed it is hardly to be expected that they should be observed, much less that they should be related, when they happened in a country so thinly inhabited, and where one may reasonably suppose, that, in general, only the grosser and more violent effects would be taken notice of.

Section III.—55. I have said before, that I imagined earthquakes were caused by vapours raised from waters suddenly let out upon subterraneous fires. It is not easy to find any other cause capable of producing such sudden and violent effects, or of raising such an amazing quantity of vapour in so small a time. That the blasts, discharged from volcanos, are always produced from this cause, is highly probable; that they are often so, cannot admit of the least doubt. There can be no doubt, that considerable quantities of water must be often let out upon the fires

* See Hist. and Philos. of Earthq. p. 195. Don Antonio d'Ulloa, an author of great veracity, speaking of the same thing, says, that "the whole plain [near Latacunga] is full of large pieces of rocks, some of them thrown from the volcano Cotopaxi, by one of its eruptions, to the distance of five leagues." See his Voyage to Peru, part i. book vi. chap. 1.

of

of these volcanos, and whenever this happens, it will be immediately raised by the heat of them into a vapour, whose elastic force is capable of producing the most violent effects*.

56. Both the tremulous and wave-like motion observed in earthquakes, may be very well accounted for from such a vapour. In order to trace a little more particularly the manner in which these two motions will be brought about, let us suppose the roof over

* There are many effects produced by the vapour of water, when intensely heated, which make it probable, that the force of gunpowder is not near equal to it. The effects of an exceeding small quantity of water, upon which melted metals are accidentally poured, are such, as, I think, could in no wise be expected from the like quantity of gunpowder. Founders, if they are not careful, often experience these effects to their cost. An accident of this kind happened about forty years since, at the casting of two brass cannon at Windmill-hill, Moorfields. "The heat of the metal of the first gun drove so much damp into the mould of the second, which was near it, that as soon as the metal was let into it, it blew up with the greatest violence, tearing up the ground some feet deep, breaking down the furnace, untiling the house, killing many spectators on the spot, with the streams of melted metal, and scalding many others in a most miserable manner." [See the note at the end of process 44th of the English translation of Cramer's Art of Assaying Metals.]

Other instances of the violence of vapours raised from water, are frequently to be met with: one of Papin's digesters being placed between the bars of a grate, where there was a fire, was, after some time, burst by the violence of the steam, the fire was all blown out of the grate, and a piece of the digester was driven against the leaf of a strong oak table, which it broke to pieces. [See Phil. Trans. No. 454, or Martyn's Abr. vol. viii. p. 465.] The marquis of Worcester also, in his Century of Inventions, tells us, that he burst a cannon by the same means.

It has been sometimes imagined, that the vapours, which occasion earthquakes, were of the same kind with those fulminating damps, of which we often meet with instances in coal mines. Now, there are several things which make it very probable that this is not the case: it is true, the force of such vapous is very great; we have had instances, where large beams of timber have been thrown to the distance of an hundred yards by them: [see Philos. Trans. No. 136, or vol. ii. p. 381. Lowthorp's Abr.] but what is this to the force of that vapour, which could throw stones of twenty or thirty ton weight to the distance of three leagues? Nor, indeed, is it at all probable, that any vapour, already in the form of a vapour, can, by suddenly taking fire, increase its dimensions so much, as to produce that immense quantity of motion, which we observe in some earthquakes: but this is rather to be expected from some solid body, such as water, which is capable of being converted,and that almost instantly, into one of the lightest, and perhaps one of the most elastic, vapours in the world. Air, when heated to the greatest degree that it is capable of receiving from the hottest fires we can make, acquires a degree of elasticity about five times as great as that of common air: the vapour of gunpowder, whilst it is inflamed, has also about five times the elastic force which it has when cold. [See Robins's excellent tract on Gunnery.] Now, if we suppose a fulminating damp, of any kind, to increase its elasticity, when inflamed in the same proportion, this will be abundantly sufficient to make it produce any effects, which we have ever seen produced by any of the damps of mines, &c. And, indeed, whoever over some subterraneous fire to fall in. If this should be the case, the earth, stones, &c. of which it was composed, would imme-

whoever carefully examines the effects, either of the damps of mines, or of those fulminating damps, that are raised from some metals, when in fusion, or when they are dissolving in acids, will rather be inclined to think, that the force of inflamed vapours is so far from exceeding the proportion of five to one, that it falls considerably short of it.

But though we should suppose that this proportion holds good, where shall we find a place capable of containing a sufficient quantity of such a vapour, to produce the great effects of earthquakes? It will be said, perhaps, in subterraneous caverns. To this we may answer, that he, who is but moderately acquainted with the structure of the earth, and the materials of which it is composed, will be little inclined to allow of any great or extensive caverns in it(*l*). But, though this should be admitted, how can it come to pass that these caverns should not be filled with water? If it is alleged, that the water is expelled, as the vapour is formed, why should not the vapour, as it is supposed to be the lighter, be expelled, rather than the water, by the same passages by which the water is to be expelled? But let us suppose this difficulty also to be got over, and the water to be removed, and we shall then have a gage for the density of the vapour; for it must be just sufficient to make it capable of sustaining a column of water, whose height is equal to that of the surface of the sea above the bottom of the cavern, in which the vapour is supposed to be contained. Now, since the mean weight of earth, stones, &c. is not less than two and a half times the weight of water, this vapour must be increased to two and a half times its original elasticity, before it can, in any wise, raise the earth above it; and if we suppose it to be increased to five times its original elasticity, it will then be no more than twice able to do so; in which case, so much vapour only can be discharged from the cavern, to produce an earthquake, as is equal to the content of the cavern: and what must the size of that cavern be, which could contain vapour enough to produce the earthquake of the 1st of November 1755, in which an extent of earth of near three thousand miles diameter was considerably moved? or how can we suppose, that the roof of such a cavern, when so violently shaken, should avoid falling in? especially, as it is hardly to be supposed, that any inflamed vapour whatsoever should be able to move the earth over these caverns, if they lay at any great depth, since the weight of less than three miles depth of earth is capable of retaining the inflamed vapour of gunpowder within the original dimensions of the gunpowder itself; and common air, compressed by the same weight (supposing the known law of its compression to hold so far), would be of greater density than water.

We may ask still further, whence such vast quantities of vapour should be formed, or what sources they must be, which would not be exhausted (if they were not again replenished) by a very few repetitions of such immense discharges.

(*l*) In this passage, we see another instance of Mr. Michell's happy sagacity, in rejecting the notion of extensive *Caverns within the Earth*, long before the observations of Maskelyne and calculations of Hutton, (Phil. Mag. vol. xxxviii. p. 112) had established a *specific gravity* for the whole Globe, quite inconsistent with such a puerile supposition: the confirmatory experiments however of Cavendish, and the sublime deductions of Laplace, in further confirmation of the same well-established fact, are yet unable to shake the belief of numbers of Plutonists and Geognosts, in the dogma common to both their Creeds, viz. that Granite, of their very precise hand-specimen kind, occupies all the central parts of the Globe!

diately

diately sink in the melted matter of the fire below: hence all the water contained in the fissures and cavities of the part falling in, would come in contact with the fire, and be almost instantly raised into vapour. From the first effort of this vapour, a cavity would be formed (between the melted matter and superincumbent earth) filled with vapour only, before any motion would be perceived at the surface of the earth: this must necessarily happen, on account of the compressibility* of all kinds of earth, stones, &c. but as the compression of the materials immediately over the cavity, would be more than sufficient to make them bear the weight of the superincumbent matter, this compression must be propagated on account of the elasticity of the earth, in the same manner as a pulse is propagated through the air; and again the materials immediately over the cavity, restoring themselves beyond their natural bounds, a dilatation will succeed to the compression; and these two following each other alternately, for some time, a vibratory motion will be produced at the surface of the earth. If these alternate dilatations and compressions should succeed one another at very small intervals, they would excite a like motion in the air, and thereby occasion a considerable noise. The noise that is usually observed to precede or accompany earthquakes, is probably owing partly to this cause, and partly to the grating of the parts of the earth together, occasioned by that wave-like motion before mentioned.

57. After the water, that first came in contact with the fire, has formed a cavity, all the rest of the water contained in the fissures,

* The compressibility and elasticity of the earth, are qualities which don't show themselves in any great degree in common instances, and therefore are not commonly attended to. On this account it is, that few people are aware of the great extent of them, or the effects that may arise from them, where exceeding large quantities of matter are concerned, and where the compressive force is immensely great. The compressibility and elasticity of the earth may be collected, in some measure, from the vibration of the walls of houses, occasioned by the passing of carriages in the streets next to them. Another instance to the same purpose, may be taken from the vibrations of steeples, occasioned by the ringing of bells, or by gusts of wind: not only spires are moved very considerably by this means, but even strong towers will, sometimes, be made to vibrate several inches, without any disjointing of the mortar, or rubbing of the stones against one another. Now, it is manifest, that this could not happen, without a considerable degree of compressibility and elasticity in the materials, of which they are composed: and if such small things as the weight of steeples, and the motion of bells in them, or a gust of wind, are capable of producing such effects, what may we not expect from the weight of great depths of earth? There are some circumstances, which seem to make it not altogether improbable, that the form and internal structure of the earth depend, in a great measure, upon the compressibility and elasticity of it. There are several things that seem to argue a considerably greater density in the internal, than the external part of the earth; and why may not this greater

fissures, immediately communicating with the hollow left by the part that fell in, must run out upon the fire, the steam taking its place. From hence may be generated a vast quantity of vapour, the effects of which shall be considered presently. This steam will continue to be generated, supposing the fire to be sufficiently great, till the fissures before mentioned are evacuated, or till the water begins to flow very slowly; when the steam already formed will be removed by the elasticity of the earth, which will again subside, and, pressing upon the surface of the melted matter, will force it up a little way into all the clefts, by which the water might continue to flow out. By this means, all communication between the fire and the water will be prevented excepting at these clefts, where the water, dripping slowly upon the melted matter, will gradually form a crust upon it, that will soon stop all further communication in these places likewise; and the fissures, that had been before evacuated, will be again gradually replenished by the oozing of the water between the strata.

58. As a small quantity of vapour almost instantly generated at some considerable depth below the surface of the earth, will produce a vibratory motion, so a very large quantity (whether it be generated almost instantly, or in any small portion of time) will produce a wave-like motion. The manner in which this wave-like motion will be propagated, may, in some measure, be

greater density be owing to the compression of the internal parts arising from the weight of the superincumbent matter, since it is probable, that the matter, of which the earth is composed, is pretty much of the same kind throughout? There is a still stronger argument for the earth's owing its form, in some measure, to the same cause; for it is found to be higher [see the French accounts of the measures of a degree of the meridian in France, Sweden, and America] at the equator, than at the poles, in a greater proportion than it would be on account of the centrifugal force, if it was of uniform density; but, if we suppose the earth to be of less density in an equatorial diameter than in the axis, the whole will then be easily accounted for, from the rising of the earth a little by its elasticity, the weight being in part taken off by the diurnal rotation: and that the earth is really a little denser in the axis, than in the equatorial diameter, seems highly probable, from the experiments of pendulums compared with astronomical observations; for the forms of the earth derived from these, cannot be reconciled with each other, but upon this supposition. [See Maclaurin's Fluxions, art. 681, &c.] It appears, from some late and accurate observations, that the equatorial parts of the planet Jupiter also, as well as those of the earth, are a little higher than they would be, if their rise was owing to the centrifugal force, and he was of uniform density; but if we suppose him to be of less density in the equatorial, than the polar regions, then the form may be such as he would assume from the respective gravitation of the several parts; and any fluid like our ocean, would not overflow the polar parts, (which, upon any other supposition, it must necessarily do,) but would follow his general form, as our ocean does that of the earth.

represented

represented by the following experiment. Suppose a large cloth, or carpet, (spread upon a floor) to be raised at one edge, and then suddenly brought down again to the floor, the air under it, being by this means propelled, will pass along, till it escapes at the opposite side, raising the cloth in a wave all the way as it goes. In like manner, a large quantity of vapour may be conceived to raise the earth in a wave, as it passes along between the strata, which it may easily separate in an horizontal direction, there being, as I have said before, little or no cohesion between one stratum and another. The part of the earth that is first raised, being bent from its natural form, will endeavour to restore itself by its elasticity, and the parts next to it beginning to have their weight supported by the vapour, which will insinuate itself under them, will be raised in their turn, till it either finds some vent, or is again condensed by the cold into water, and by that means prevented from proceeding any further.

59. If a large quantity of vapour should continue to be generated for some time, several waves might be produced by it; and this would be, in some measure, the case, if the quantity at first generated was exceedingly great, though the whole of it was generated in less time, than whilst the motion was propagated through the distance between two waves.

60. These waves must rise the higher, the nearer they are to the place from whence they have their source; but, at great distances from thence, they may rise so little, and so slowly, as not to be perceived, but by the motions of waters, hanging branches in churches, &c.

61. The vibratory motion occasioned by the first impulse of the vapour, will be propagated through the solid parts of the earth, and therefore, it will much sooner become too weak to be perceived, than the wave-like motion; for this latter, being occasioned by the vapour insinuating itself between the strata, may be propagated to very great distances; and even after it has ceased to be perceived by the senses, it may still discover itself by the appearances before mentioned.

Section IV.—62. All earthquakes derived from the same subterraneous fire, must come to the same place in the same direction; and those only which are derived from different fires, will come from different points of the compass; but as, in all probability, it seldom happens that earthquakes, caused by different fires, affect the same place, we therefore find in general, that they come from the same quarter: it is not, however, to be supposed, that this should always be the case, for it will, probably, sometimes happen to be otherwise: and this is to be expected in such places as are situated in the neighbourhood of several subterraneous

subterraneous fires; or where, being subject to the shocks of some local earthquake of small extent, they now and then are affected by an earthquake, produced by some more distant, but much more considerable cause. Of this last case, we seem to have had some instances in the earthquake of the 1st of November 1755, and those local ones, before mentioned, which succeeded it.

63. As we may reasonably infer from many earthquakes coming to the same place, from the same point of the compass, that they are all derived from the same cause, and that a permanent one; so we may reasonably infer the same thing also, from their being propagated with the same velocity; but this argument will still come with the greater force, if it be considered, that the velocity of any vapour, which insinuates itself between the strata of the earth, depends upon the depth of it below the surface; for the deeper it lies, the greater will be its velocity*. We may therefore conclude, from the sameness of the velocity of the earthquakes of the same place, that the cause of them lies at the same depth; and from the inequality of the velocity of the earthquakes of different places, that their causes lie at different depths. Both these are perfectly consistent with the supposition, that earthquakes owe their origin to subterraneous fires, since the strata in which these subsist, may be easily conceived to lie at different depths in different parts of the world.

Section V.—64. From the same cause, we may easily account for those local earthquakes, which succeed the greater and more extensive ones. If there are many subterraneous fires subsisting in different parts of the world, the vapour coming from one fire may very well be supposed, as it passes, to disturb the roof over some other fire, and, by that means, occasion earthquakes by the falling in of some part of it: and this may be the case, in some measure, even where the vapour passes at some small distance over the fire; but it will be most likely to take place, where the vapour either passes at some distance under it, or between the stratum, in which the fire lies, and that next above or below it.

[To be continued.]

* The velocity of such a vapour, depending entirely upon the elasticity of the earth which is over it, will be, *cæteris paribus*, (if I am not mistaken) in the ratio of the depth below the surface. This seems to follow from a known law of all elastic bodies, according to which they tend to return to their state of rest, when either dilated or compressed, with forces proportionable to the quantity by which they differ from their natural bounds.

XLII. *On*

XLII. *On the Swallow.*

To Mr. Tilloch.

29th August, 1818.

Dear Sir,—Intending to have collated my former observations on the Swallow with the various occurrences of the present year, I had purposely postponed any communication regarding these interesting visitors, till I had either corroborated the facts I had observed, or corrected any misconceptions in my former notes, to render the subject of your request on the cover of the Magazine for May last, as perspicuous and free from doubt as my opportunity for observation could possibly admit. I would even for that reason have deferred writing you on this subject till their final departure for the season, had not a most singular deviation from all former habits of a particular class of these mysterious and undefined migrants, induced me to forward the present notice, leaving you to judge how far an insertion of the case may be conducive to obtaining further information from others, so as to ascertain whether this remarkable event has been local or general. The circumstance I allude to is the final departure of the *white-tailed* swallow, in the first week of the present month, under circumstances rather extraordinary, leaving the chimney swallow behind.

The first swallow that made its appearance here this season was of the chimney species, and first seen carelessly skimming much higher in the atmosphere than usual, on the forenoon of the 15th of April. It continued in that elevated situation till the 19th, when it was joined by two others of the same kind. These kept close company all that day, but on the 20th the two disappeared, leaving the solitaire to float about apparently without any object in view till the evening of the 28th. During all this time the mornings and evenings had continued frosty; a complete thaw, however, came on in the afternoon of the 28th, accompanied with a warm black shower, and this was followed on the morning of the 29th by a general arrival of all the various kinds of swallow that frequent this quarter.

Prior to their arrival, I had observed with great pleasure a sparrow take possession of a swallow's nest, which had stood out the winter in the east corner of a bed-room window; and was in full expectation of seeing the repetition of a tragedy that had been acted some years before in this neighbourhood. Similar to the above, a sparrow had taken early possession of a swallow's nest, and had laid some eggs previous to the swallow's appearing to claim her castle. The sparrow firmly seated, and thus attached to the sheltering shade of its approaching brood, resisted the claim of the swallow: a stout battle ensued, in which the swal-

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low was joined by its mate, and during the conflict by several of their comrades. The sparrow, however, determinedly resisted, and successfully defended herself against the joint and repeated efforts of the assembled swallows to dislodge her. Finding themselves completely foiled in their endeavours to regain possession, they, after some consultation, had recourse to an expedient of a most extraordinary nature and singularly revengeful, and one which showed that it proceeded from a deliberate determination of the whole group that nothing short of the death of the intruder could satisfy them, or atone for this usurpation of a property unquestionably the legitimate right of its original constructor. The swallows for a time departed, leaving the sparrow apparently in the full enjoyment of her conquest. This prospect of repose, however, was only delusive; for the swallows returned with accumulated numbers, each bearing a beak full of building materials; and without any further attempt to disturb or beat out the sparrow, they instantly set to work and built up the entrance into the nest, inclosing the sparrow within the clay tenement, and leaving her to perish in the garrison she had so bravely defended.

In my expectations in the present instance, however, I was disappointed. The sparrow had but recently taken possession, had laid no eggs, and having less to contend for was sooner dislodged, but not without considerable resistance; the noise of which, with the beating of their wings against the glass, waked me in the morning, and gave the first notice of their arrival, on which I got out of bed and witnessed the termination of the action.

The bustle of regaining possession being over, a general examination of all the old nests in the different windows took place, the partial injuries sustained during the past winter were repaired; the insides cleaned out, each nest new furnished and feathered; and the old pairs were no sooner settled in quiet possession of their former abode, than they began assisting the young of the former year in choosing secure situations for new habitations for themselves, and joined in the labour of building them. The construction of the swallow's nest is never executed by the solitary labour of the single pair destined to occupy it, but by the joint labour of the community, as shall be noticed in the sequel; and is moreover a work of extreme nicety and great entertainment to an attentive observer, considering with what method and regularity these little animals conduct the whole process.

But not to lose sight of the main object of this communication—the sudden and early departure of the white-tailed swallows, whose former migrations were always in unison with the other classes of the species. They arrived with the others: it was these that were found contending with the sparrow for the nest, which they

they no sooner possessed and put in a comfortable state, than they, with the others of their kin, began the duties of the season; and by the 1st of July their first brood were full fledged and flown, and by the 4th of July the nests were thoroughly cleaned out, new furnished and feathered, and their second incubation begun, some new nests built, and one of a very particular construction completed. Notwithstanding all this, by the middle of the month they began to assemble in groups upon the roof of the drying-house, the ordinary place of general resort for all classes prior to the usual departure for the season. I was rather surprised the first two or three evenings of their grouping, knowing from their former habits that this was indicative of a change. I had all their nests examined, and found one, two, and three eggs in each,—none without an egg. I could then hardly allow myself to believe it possible during so fine a season, and at so early a period of the year, that their intentions were migratory. In this, however, I was soon undeceived; the training exercise, or preparation of the young brood for the destined flight, immediately commenced, and they finally roosted among a collection of bull-rushes growing on a bank in the midst of the Leven; from which they took their final departure on the fourth night of their occupation of this retreat, and have never since been seen.

After no doubt could remain of their being off, I conceived this an excellent opportunity of putting their fancied submerging among the flags and rushes, along the banks of lakes and rivers, to the test of actual survey and experiment. Two stout men instantly volunteered their services, and immediately stripped, went into the river, searched all the flags, rushes, banks and braes round and near the spot where they were last seen. Not a hole nor stone was left unexamined; and the roots of the rushes and even the mud were most carefully handled, and the shelvings under the banks. In short, no creature could possibly have escaped the pursuit, nor eluded in any way the unwearied anxiety and diligence of the searchers; but not a feather nor a swallow was to be found. The theory of their submerging, in my opinion, is all a dream, in which this search has gone its length to confirm me; and the following anecdote may not be improperly narrated, as it refers to the refutation of this same absurd opinion:—On the 11th of April 1812, returning from Glasgow with a friend, we stopped at Kinross to corn our horses and take a parting dinner. Before dinner was ready we took a turn down to the Old Chapel, and returning by the loch side, we both expressed our astonishment at the vast assemblage of swallows, the first we had seen that season, hovering over the surface of that corner of the lake which runs up towards the town by the south side of Kinross plantation. "What!" said my companion, "can the creatures have emerged from the water?

water? Some people assert that they hibernate at the bottom of lakes and rivers. *It must be so: see, there is one just risen.*"

To a superficial observer they certainly had all the appearance of just emerging from the bosom of the lake. But looking attentively, we perceived them regularly descending in a slanting direction, and take something from the surface of the water, in which exercise they always, in skimming, struck the water with their breast, dashing a spray round them, which looked very much like to shaking the water from their wings. This I have since observed a thousand times, in the swallow skimming the river or mill-dam, catching the water-flies; but which, to persons not interesting themselves in the result, and at some little distance from the scene of action, is certainly very delusive, and without a close inspection and very attentive observation, apt to leave that impression of their emerging from the water upon the mind. The weather was still cold, and not a fly abroad in the air to support them: no doubt remained with us of their thus gathering food; an idea in which we were soon strengthened, by stepping down to the edge of the lake, and seeing the surface of the water all along the shore, and as far as the eye could reach, swarming with innumerable insects in appearance like gross gun-powder, and the water itself filled with the maggot of a water-fly, upon which there can be no doubt whatever the birds were feeding.—Some similar occurrences must have given birth to the delusion of submerging; and the gentleman who so confidently asserts that he saw them with his own eyes coming up from the lake and shake the water from their wings, must have been deceived with his eyes open by a corresponding event.

I am perfectly satisfied, from a variety of circumstances, that the same swallows return to the same spot from whence they emigrate from year to year: in this I was particularly confirmed on the morning of their arrival this season, by a pair of chimney swallows that had nestled for years before in a cellar above the coal-shed, to which they had access by an opening in the roof that had been closed after their departure to keep out the winter drift. The first things I saw upon looking towards the boiling-house that morning, were these swallows flying about the cellar, searching with great anxiety every tile in the roof for an entry to their old dwelling. I no sooner got down than an opening was made, through which they immediately passed, while I had not retired a yard from the spot, and seemed quite overjoyed to find their former incubat entire. When I entered the boiling-house all was noise and merriment; twelve nests they had occupied the preceding year were all entire, uninjured, and again in possession of the chimney swallows, their former occupants.

I cannot suppose that stranger swallows could with equal facility

cility have found these old nests; neither would they undisturbed go so immediately about their labour, amidst a crowd of people often passing through the group at arms length, perching upon the wringers and poles within a couple of yards of the servants, chanting their little song, and looking at the men and women with all the familiarity and confidence of old friends. This was as much the case the day of their arrival as it had been at any period before their departure the preceding year. These must unquestionably be the identical swallows we have had in former years. The rattling of wringers, the thumping of buttles, the noise of wheels and pumps, nor the steam of the boilers ever annoy them, which would not likely be the case with strangers. Although the whole group of mingled men and women may be collected inside or outside the door, laughing and making sport, the swallow will pass in and out, sometimes fly through the crowd, and sometimes over and almost touching their heads, without showing the least symptom of dread, any more than if the space was unoccupied. No violence is allowed to be offered to them, and nothing gives them the least uneasiness but the appearance of a hawk or a cat. When a swallow observes either of these animals, particularly the cat, an alarm is instantly given by a particular sharp call, when a number assemble, flying about, darting at puss with such celerity and from so many points at once, as to distract, tease, and vex her, till the heart-sick tabby is fain to seek shelter under the nearest cover.

What may be termed the domestic habits of the swallow are highly amusing, and often afford no small entertainment, particularly in such situations as ours, where all their little manœuvres and gambols are open to view; and the whole colony is in intimate habits of association with the inmates, with whose appearance I am fully satisfied they really become perfectly acquainted, and do distinguish the approach of strangers. One instance may be narrated:—A pair of chimney swallows annually build in the evaporating shed, often so low that the man who attends the pans can, by raising himself a-tiptoe, look into the nest. His inclination is to protect them, and his instructions are to allow nothing whatever to molest them: they are so very familiar with this individual, and those in the daily habit of looking over and assisting him, that they build and trim their nests, set, and feed their young, without showing the least uneasiness, although he, or any with whom they are acquainted, stand and look so near as to blow their breath into the nest. Passing the shed-door one day, the swallows were heard in the greatest state of alarm, their cry indicating the utmost terror. My brother called, "Clunie, what is the matter with the swallows? look and see that no cat is near them." Not receiving so ready an answer as usual, he mounted the short trap, and after passing through the steam, found Andrew with a pair of rider spectacles on his nose, carefully and wistfully poring over a copy of Bunyan's Holy War, and keenly debating with a packman about the inputs in exchange with a well worn copy of Pilgrim's Progress, that had been his constant companion for many years. They were desired to go down and settle the balance outside the door, and the pans would be attended to till the matter was adjusted. The packman had no sooner walked out, than the swallows gave over their noise and fluttering. The sire departed in quest of food, and the dam returned to the nest as if nothing had happened, while we stood so near as by stretching out our arm we might have touched the bird on her seat.

To the swallow I feel very much inclined to attribute a degree of knowledge and sagacity rather beyond the bounds of credibility; and would allow them, in point of mental endowment, a rank amongst the feathered tribes equal to what the beaver holds in the gradations of the mammalia upon earth. That they are governed by certain laws, that may be termed instinct, cannot be refused. But that they also possess a reasoning faculty, with the power of conceiving, communicating, and receiving ideas,—in short, that they actually possess in some degree the gift of speech, I am equally satisfied.

They convene meetings for general welfare; they converse, deliberate, and consult; project plans, and act with method and regularity and in concert in the execution; unite in defence, and when necessary call in the aid of distant colonies in punishment of aggressors. They take a general interest in the individual safety of every member and family belonging to their community; repair the injuries sustained, as one common concern; protect the young, and feed the orphan, when any accident deprives them of the fostering protection and care of their natural parents.

In June 1816, some young gentlemen disappointed in duck-shooting from the wetness of the morning, after the weather cleared up a little, not knowing the protection afforded here to the gentle and inoffensive swallow, these sons of sport and festivity fired a few rounds for their amusement, and unfortunately brought some of the parent swallows to the ground, among the rest, both parents of a young brood of five, in a nest placed in the corner of one of the windows of my premises, while in the very act of skimming into the nest with a beakfull of flies. They were really sorry for this. Conceiving the young must perish from hunger, I intended taking them into the house and try to bring them up, under the care of the children, who had undertaken to catch flies for them. This, however, was found unnecessary; the news of the calamity had spread over the colony, and

and a collection of parent swallows had gathered.—The state of the nest and the young was taken under review, and arrangements immediately gone into for the protection and support of the helpless orphans: their support was brought them before leaving them for the night: and next day the sympathetic and benevolent office of feeding them was carried on with so much parental care, that they were as regularly fed, and as soon fledged and on the wing, as any nest about the field. I mention this latter part as a proof that they lacked nothing in common with the young of other nests; as I have for years uniformly found, that good or bad weather, about nursing time, will make a difference of a week, and sometimes more, in the flight of the young, which must be in proportion to the quantum of food delivered into the nest, and this must be greater in good weather when the flies are more numerous. During bad weather, when few flies are on the wing, the swallows must feed very sparingly. Their powers of digestion during the season of plenty must be very rapid. In some warm days when the flies were numerous, I have cleaned the stones under different nests to ascertain the number of drops that fell from the young, and found four and sometimes five drops from each bird in the course of an hour: in bad weather sometimes only one, two, or three from each, just in proportion to the quantity of flies given, and the state of the atmosphere for catching them. Only a few days had elapsed, when the joint assistance and labour of the whole colony was again put in requisition. A nest built in the west corner of a back room window facing the north was so much softened by rain beating in that direction, from the severity of a violent storm from the north-east, as to render it unfit to support the weight of a superincumbent load of five well grown young swallows: during the storm the nest fell into the corner below, leaving the young brood exposed to all the inclemency of the blast. To save the poor things from untimely death, a covering was thrown over them till the severity of the storm abated. This had no sooner subsided than the sages assembled, fluttering round the window and hovering over the temporary covering of the fallen nest, which was removed as soon as this careful anxiety was discovered, and the utmost joy evinced by the group on finding the young ones alive and unhurt. After feeding them, the members of this assembled community arranged themselves into working order; each division, taking its appropriate station, fell to instant labour, and before nightfall had jointly completed an arched canopy over the young, and securely covered them against a succeeding blast. Calculating the time occupied by the assembly to perform this piece of architecture, it appeared evident the young must have perished from hunger or cold before any single pair could have executed half the job.

Aware by this example of the danger of this situation, no attempt has ever been made to build in either corner of that window again. Nothing so much indicates superior understanding in the whole arranged dispositions of the swallow, or deserves so much attention, as the order, regularity, and method observed in the construction and execution of their nests, their œconomy and frugality in expediting the various operations by the division of labour, and taking the advantage of every wind that blows to accomplish the grand object; their sagacity in selecting situations, and placing the entrance into the nest to leeward of the storm; their wisdom in the choice and mixture of materials, and the wonderful art with which these are adapted to the different positions and change of form, regulated by existing circumstances;—arrangements certainly far beyond the power of instinct.

When a situation has been once fixed upon for a new nest, before a particle of building material is laid, every bearing of the intended site is minutely examined by a few of the sages, during which a great deal of conversation and reasoning goes on; plans are proposed, and one ultimately fixed upon before proceeding to the work. Matters being thus far adjusted, a number collect—sometimes above a dozen—and form themselves into divisions, for the distribution of labour, before commencing operations; the number assembled is always in proportion to the extent of work and number of nests to be carried on at the same time. I have known from two to five nests in a progressive state of forwardness, all carrying forward at the same time by the same associated band of operators.

When a place for mortar-making has been selected, the whole band commence operations by gathering a beakfull of chopped straw or hay, generally taken from dry horse droppings either about the field or from the high road; with this they repair to the place appointed, and commence mortar-making by mixing this with clayey soil, rendered additionally unctuous by their working it with their beaks; and all, as ready, fly off with their load and begin building. When the foundation has attained the size of a small walnut, one experienced builder remains stationary, a proportionate number at the mortar hole, and a division of straw gatherers and carriers carry on the work till the weight and softness of the new made materials endanger the falling of the whole. Then, this nest is left off, and time in proportion to the state of the weather given for it to harden firm and dry, and the whole band goes to the next; and after carrying it a similar length, leaves it in the same manner, for the same purpose, and goes to the third, the fourth, a fifth, and again returns in rotation to the first, and so going repeatedly over the whole till the labour is completely accomplished. During the whole of these operations

rations the grand master builder, who is sometimes relieved by turns, shows his skill and experience in the art of building, by proportioning the radius of the nest, the acuteness or obliquity of the angles, in strict proportion to the distance and bearing of the abutments, and thickens the wall proportionately to the tenacity of the materials, every morsel of which he carefully examines before it is laid on, and if satisfactory, a pleasing chit-chat passes between the parties. But if insufficient, in any respect, the tone of the grand master is soon changed, and a violent altercation takes place. I have seen the imperfect materials rejected and thrown down with great apparent wrath and indignation, and the carrier dispatched with a scold for a fresh load. It was during my attention to these little operations that I first conceived them possessed of the gift of speech. Would any one, who may have time and opportunity, with an ear capable of distinguishing the minute variations of sound, attentively listen to the different conversations that pass on these occasions; the orders given, and communications received; the varied cadence of the articulated notes; the pleasing mildness with which this is conducted in good humour; the fierce volubility and furious manner in cases of reproof or scolding; I am satisfied an impartial auditor would mark these wonderful migrants as gifted with the power of mutual communication by articulate sounds, without which how could so much harmony be conceived or works of art executed? From the sagacity of the swallow the glass-maker has received his best lesson for building his *pots*, a matter not of the least consequence in that art.

There is just now a new nest, built by the white-tailed swallow a short time before their departure, which displays more than ordinary wisdom and sagacity in the execution. It is placed in the west corner of a window facing the south, and covers more than a fourth of the glass of that pane. The operations went on much in the usual style, till better than half built; when, instead of going forward with the work, and leaving the entrance thereto at the top as usual, they made a halt at that side next the glass, to which they were conscious their mortar could not have adhered with sufficient tenacity, and where the great burden of the nest must have rested. Thus the safety of the whole would have been endangered. The outside of the nest furthest from the glass was carried up the full height, closed in, and made good to the upper lintel. Returning to the unfinished part, they carried up a pillar by the side of, and leaning to, the glass, upon which they formed a capital projecting outwards from the glass. On the opposite side, upon that part of the nest carried forward and completed, they formed a corresponding butment; between these they constructed a regular arch, and over this arch carried up a superstructure of exquisite workmanship for delicacy and lightness of execution, and covered in the whole habitation, leaving the entrance in the weakest part, now rendered secure by the slightness of the work and lightness of the materials.—Should this nest have the good fortune to stand out the storms of winter, I hope by the return of summer to have an opportunity of seeing and attending to the mode of feeding their young, through the uncovered part of the glass; by this I can see all passing within the nest from the light transmitted through the opening.—Their mode of feeding, from what I have already observed, differs from that of other birds, who feed all in rotation, pick about. The swallows, so soon as the young are able to move themselves forward to the entrance of the nest, allow one to take possession of the opening and remain there, receiving and swallowing every fly that is brought by the dam and sire, till completely gorged. He then turns round, lets fall his drop, and retires to rest, when another takes his place, acts in the same manner, and retires from the same cause, giving place to the rest in rotation. The swallows no sooner get the first brood (which in good weather is generally about the 1st of July) fairly on wing, than they, as if conscious of the value of their time, instantly commence their second incubation, and have their second brood abroad by about the middle of September, after which no further attempt is made to breed. The remaining part of their time is occupied in training the young for their ultimate flight. This is conducted with great regularity and order for ten or twelve days before the final departure. After the business of food gathering for the day is over, they assemble in multitudes from all quarters and of all kinds in one general convention upon the roof of the drying-house, chanting their little song or evening hymn. While the assembly are all seated on the roof, one (who seems commander-in-chief) keeps aloft on wing, flying round and round; at last darting with great swiftness from the rest, with a loud, sharp, and repeated call, he gives the word of command. That instant all is silent. The whole are instantly on the wing, directing their flight in perfect order, rising upwards in the most beautiful spiral track, and are in a moment beyond the reach of human eye.

In a quiet night they may be heard after they are not seen.—They remain in the upper regions of the atmosphere for, perhaps, a quarter to half an hour, when they all return by scores and dozens to the place from whence they took their flight. This they will repeat twice or thrice in an evening when the weather is fair and kindly.

The deviation of the white-tailed swallow this year I consider a very a very unaccountable event, and the only instance of the kind I ever remember. They always, in common with the other swallow, produced twice, and used to foster their young to the last. I have known both kinds in a bad season, when short of flies, to nourish and bring forward their young, abandon whole nests of the last sittings to perish, when the ultimate period of departure arrived, but never knew them separate and leave the other divisions behind. There must be some strange unknown cause for this extraordinary resolution. At first I dreaded that it prognosticated some unprecedented change of temperature, or convulsion of the elements. When entertaining that idea, I could not help remarking the appearance of a cock robin, who, two days after their departure, came hopping into the boiling house introducing one of his sons, looking round, and with his accustomed familiarity strutting through the house, viewing every one as he passed, without concern or alarm.

The ultimate hibernation of the swallow I am of opinion will for ever remain a mystery, unless aërial navigation come to that state of perfection as to make voyages from Pole to Pole. Then they may be detected in the utmost regions of the atmosphere far beyond the rage of elements and strife of storms.—To whatever clime or part of the world they proceed, their flight is at an elevation far beyond the reach of optics. They depart with the first ray of the morning, and so directly upwards as to elude all research. They return with the first dawn of the day, but from whence no man can tell; they drop as from the clouds, and take up their abode in the former haunts as if they had just left them the hour before. On the 1st of April 1809, I had occasion to be at the village of Ceres; the morning was remarkably clear, with a cloudless sky of beautiful azure. Standing a little before the door of my friend, remarking the fineness of the morning and clearness of the sky, my friend desired me to look at a small but well defined dark spot that had just caught his eye at a very great height in the air. I soon perceived the object; it remained for some time apparently stationary, but increasing in size, from the object's approach being in a direct line to the spot where we stood. Continuing to keep an eye on the object for some time, we at last discovered it to be a swallow flying with immense velocity, which like an arrow from a bow darted into an old nest in my friend's window.

These straggling swallows, "for one swallow never made summer," I conceive mere *espionage* directed from the main body, like Noah's dove to spy and report on the appearance of the earth, or to find the longitude or latitude of their flight, or to find if near the former haunt of a colony. When that is the case, the first remain,

main, a second and a third are dispatched for intelligence, who return and inform their associates; who, finding how the lands lie, depart from the body, taking possession of their domains, and wishing those behind good morning.

GAVIN INGLIS.

XLIII. *On the real Difference between the specific Gravity of the Human Body and Sea Water.* *By* KNIGHT SPENCER, *Esq.*

To Mr. Tilloch.

SIR,—IN September 1815, you did me the favour to insert in your Magazine the result of an experiment which I had made with a view to ascertain how much the human body is specifically lighter than sea-water; and, in answering your question respecting the same in the month of November of that year, I promised to repeat the experiment, with this difference, that I would make it in my usual dress, instead of being naked, the intention of which was, to ascertain what greater degree of danger (if any) would in consequence be incurred, either in the case of falling overboard, or in plunging into the sea without the delay of undressing, to prevent a fellow-creature from drowning,—and, generally speaking, to induce that fair degree of confidence which the knowledge of the *real* difference between the specific gravity of the human body and sea-water ought to inspire.

Circumstances have, until the present autumn, prevented me fulfilling my promise. I have now to state, that in August last I took the opportunity of a smooth sea for my experiment; and, in addition to my usual clothing (which weighed five pounds) I borrowed from the guide who attends the machines, a pair of heavy shoes with much iron about them, weighing three pounds and a half; and with flints in each hand weighing together six pounds two ounces, being altogether fourteen pounds ten ounces avoirdupois, I threw myself upon my back in the water, and floated upon the surface without *the smallest* motion whatever, so much at my ease as to converse with my friends on the beach, and with the toes of the heavy shoes just visible above the surface.

If the fact established by this experiment were more generally known, I think it could not fail to induce many timid persons to become expert swimmers, who are now afraid to venture into the sea at all.

Before I close this letter, permit me, Sir, to state another fact, which cannot fail to give confidence to those who are learning to swim—

swim—and all young men, both for their own safety and the welfare of society, ought to learn this pleasing and healthy art.

At the time I was about to make my experiment, I observed four boys take a boat, go into deep water, undress, and plunge in one after the other, range themselves in a line, and actually play a game at leap-frog for at least a quarter of a mile. The last boy, swimming to the boy immediately before him, placed his hands on his shoulders, popped him under water, and then swam to the next; the boy who was popped under water, on rising, swam to the boy immediately before him, popped him under in his turn, and so on till they reached the shore.

I assure you, sir, that Captain Shandy, when carrying on the siege of Dendermonde, did not feel a stronger desire to have a pull at Corporal Trim's smoking mortars, than I did to make one with the boys at leap frog. I am, sir,

Your obedient servant,
KNIGHT SPENCER.

Surry Institution,
October 8th, 1818.

P. S. I omitted to state that I swam out into deep water, without the flints, and found no other inconvenience than a slight impediment to my progress from the drag of the laps of my coat.

XLIV. *Hungarian Agriculture, and Improvements in the Management of Sheep and Cattle, as practised on the Estate of Graf* HUNYADI, *at Urmeny, in Lower Hungary* *.

We proceeded to visit one of the Graf's farms, called Keszi. A large collection of peasants, with their cattle and ploughs, were at work on one part of it. The system here adopted is nearly the common agriculture of the country, the usual succession of crops being,

1st year, wheat and rye, sown in winter.
2d year, grain of various sorts, sown in spring.
3d year, fallow.
4th year, winter grain.
5th year, spring grain.
6th year, fallow, with manure and good dunging, and so on, a simple fallow every third, and manuring every sixth year.

Maize is one of the most productive crops. It is planted in April, and cut in September or October, yielding thirty-fold, whilst other crops yield not more than ten-fold at the utmost.

Turnips have been fairly tried without success, on account of the dryness which usually prevails during the summer months.

The land is ploughed very shallow, seldom above three inches,

* From Dr. Bright's Travels.

with

with a plough which has its share almost horizontal. The whole of the grain, except the maize, is broad-cast, and rubbed in with a brush harrow. As well as I could judge, from the winter crops all looking green and well at the time I saw them, the seed is sown with great regularity. The maize is put in the ground by women who follow after the plough, and with a hoe turn back a little mould, drop a few grains, and immediately replace the earth.

At the farm of Keszi the Graf likewise keeps the most extensive flocks which he possesses in this part of Hungary, consisting of about twelve hundred sheep, which have now been ten years under improvement. In the care of them he employs one chief shepherd and six men. The sheep are divided into flocks of from one hundred to two hundred, as circumstances may require; and we arrived just as they were following their shepherds in various directions, over a wide extent of sweet pasture resembling our English downs.

Amongst other objects interesting to the agriculturist, was an excellent range of cow-stalls, and one of the large bee-houses, which are often very valuable in Hungary, but this was somewhat neglected.

It was here that we found the finest of all the Hunyadi flocks, and that upon which the proprietor chiefly depends for his future progress. I shall therefore take this opportunity to speak more at large respecting the breed, and the care taken in its improvement. The original breed of Hungarian sheep is, in fact, the real *Ovis strepsiceros* of authors, covered with very coarse wool, and bearing upright spiral horns. Improvement on this stock by crosses, with other varieties, is become so general, that a flock of the native race is seldom to be met with, excepting upon the estates of the clergy.

The great improvement has been by the introduction of Spanish blood. Some of the great proprietors have themselves imported from Spain; others have obtained rams from the flocks of the Emperor of Austria, and others from various private sources; so that at present there is scarcely any flock of importance which has not derived advantage from the Spanish cross.

In the year 1773, a royal flock was established at Mercopail, to assist in the general improvement. This has, however, been latterly somewhat neglected, on account of another since formed at Holitsch. The wool is now a great object of commerce. In 1802, it was calculated that above twelve million and a half pfund was exported from Hungary; a large portion of which goes into Austria, and is either there manufactured, or is carried to more distant markets; and much of that which is sold in England, under the denomination of Saxon wool, is actually the pro-

duce

duce of Hungary, exported in spite of the heavy duty it pays on leaving the Austrian dominions.

It is about fourteen years since the first Spanish sheep were introduced upon the Hunyadi estates, from Moravia, where Baron Geisler had been many years employed in improving the breed. Since that time the Graf has exercised unwearied assiduity in crossing and recrossing, and introducing new and more perfect Merinos. By keeping the most accurate registers of the pedigree of each sheep, he has been enabled to proceed, with a degree of mathematical precision, in the regular and progressive improvement of his whole stock. Out of the seventeen thousand sheep composing his flock, there is not one whose whole family he cannot trace by reference to his books; and he regulates his yearly sales by these registers. He considers the purity of blood the first requisite towards perfection in the fleece; but he is well aware that little can be done, unless the sheep be kept in health and condition. For this purpose, he has adopted a system of folding, which, as far as I can judge, is almost perfect; and the whole is conducted with so much accuracy, that I contemplated it with pleasure and astonishment.

At each of the head-quarters (if I may so term them) of his sheep, well-built sheds are constructed, having brick pillars at certain distances, which leave about half the side open, and thus admit a free circulation of air during summer, and afford easy means of excluding the cold in winter. The height of the sheds is about seven feet to the springing of the roof; and they are divided by little racks, into such spaces as are necessary for the division amongst the flocks. Racks are also arranged round the whole, so that all the sheep can conveniently feed at them. The floor is covered with straw, and the upper layer being continually renewed, a dry and warm bedding is obtained.

In these houses the sheep are kept almost constantly during winter, that is, from November till April, and are then fed three times a-day upon dry food. They are watered twice a-day, from a well close at hand. Even during summer, the sheep are driven under cover every evening, and they are conducted home in the day-time when it rains, or when the heat is oppressive. They always lamb in the house; the ewe being placed, upon this occasion, in a little pen by herself, where she remains unmolested. These pens are about three feet long by two feet wide, and are formed by means of hurdles. It is owing to this care that they never lose a lamb. But, to give a more perfect and connected idea of the minute order and extreme care taken in this establishment, and to gratify those who are really interested in the subject, I am induced to insert, at the end of the present chapter, the regulations made by Graf Hunyadi. They

came

came into my hands through a very circuitous medium, and I trust a sufficient apology, for publishing private directions of this nature, will be found, in a wish to point out to my English readers the assiduous exertions of a Hungarian noble, who takes the lead in the agriculture of his country, and in my unwillingness that they should be deprived of the valuable hints which may be thus afforded. Their utility makes them almost public property; and I foresee no inconvenience that can arise from making them known.

The first idea on reading these regulations will probably be, that, while they look well, in a theoretical point of view, they are too nice for practice. Such, however, is not the case. I believe, from all which I saw, that they are completely put in execution at Urmeny. The Graf has, indeed, one advantage, which will not often be found; he has inspired all his officers and servants with an interest in the subject. The number of persons employed is about one man to every hundred sheep, and each of them considers his flock as his family and pride.

The result of all this care has been a success which could scarcely have been anticipated. A conception can hardly be formed of flocks more uniformly excellent. The sheep are strong and healthy; and, for the Spanish cross, large. Their fleeces perfect,—not a lock broken or displaced,—and even the tail and legs covered with good wool. It is, of course, the wool, and not the carcase, which is the great object in a country so poor, and so thinly peopled, as Hungary.

The pfund (1·23 lb. avoirdupois) of wool on the spot, yields nearly one and a half silver gulden, which, as the gulden varies, according to the course of exchange, from one-seventh to one-ninth of a pound sterling, is between 3*s.* and 4*s.* 6*d.* Three pfund (about $3\frac{3}{4}$ lbs.) is estimated as about the average product of each sheep. Some, however, particularly the rams, yield six or seven. The whole of the wool, without any separation, and only washed on the back of the sheep, is sold at the same price; and the consequence is, that, from flocks which, if covered with the ordinary wool of the country, might be expected to yield an income of 15,000 or 20,000 guldens, not less than 50,000 guldens are now annually produced.

In this sketch of the Hunyadi œconomy, I must not neglect the horned cattle. The native Hungarian breed bears much resemblance to the wild white species which was formerly found in our own country. They are large, vigorous, and active, of a dirty white colour, with horns of a prodigious length, exceeding in this respect even the long-horned breed of Lancashire. The oxen are most excellently adapted for the plough, uniting to all the qualities of the ordinary ox, a very superior degree of activity.

The

The cow is, perhaps, deficient in milk; yet, by care in the choice of the best, and attention to regular milking, the quantity given by one has been increased to 2000 quarts in a year. In general, the dairy of the Graf consists of crosses of the Styrian breed, or the still more valued breeds of Switzerland, of which one in his possession gave 3400 quarts in the year. The cows are constantly in the house during the whole year, and are brushed and cleaned like horses daily; their stalls are kept perfectly neat, and are very well constructed. A raised passage about three feet broad, runs along the middle of the building, on each side of which the cows are arranged with their heads towards each other. This passage is boarded on its sides and floor, and enables the servants employed, both to look at and feed the cattle with the greatest convenience.

Besides common cattle, the Graf has about a dozen buffaloes, singularly uncouth animals; their carcase in proportion to their height, which is inferior to that of a cow, very round and broad. Their colour is black, their hair coarse, and so scanty that the skin is discernible in every part. The tail more nearly resembles that of the elephant than of the ox; and the head is so placed in a horizontal position, with the nose stretched forward, that their horns usually lie on their shoulders. The noise they make has no resemblance to the lowing of a cow, it is rather a shrill snort. They are bred in Hungary for the same purposes as ordinary cattle. The milk which they give is richer than other milk, and considerable in quantity. A single animal yielded 1470 quarts in the year. As beasts of labour, they are excessively strong, but they are slow and unmanageable.

The dairy establishments are distributed in different places, and put under the care of resident servants or peasants, who have to account to the proprietor for the produce chiefly in butter. A steward or trusty person attends each day to see how much milk is obtained. At certain intervals the trial is made to discover what quantities of butter should be yielded by a given quantity of milk, and thus a good check is placed upon all parties employed. A regular register is also kept of the quantity of milk given monthly by each cow, for the purpose of ascertaining what calves particularly deserve to be reared. I shall here conclude the subject by subjoining a short extract of two or three items from such a register.

Name.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Sum.	Produced Bull Calf	Produced Cow Calf.	Name of Calf.	Destination of Calf.	Destination of Cow.	Observations.
Gyurtsa. (Buffalo.)						$53\frac{3}{4}$	201	224	$161\frac{2}{4}$	234		6	880		9th June		Slaughtered.		Mother killed on account of age.
Bukross. (Buffalo.)	$105\frac{2}{4}$	87	$64\frac{2}{4}$					7	$289\frac{2}{4}$	$230\frac{2}{4}$	$179\frac{2}{4}$	$137\frac{2}{4}$	1113		7th Aug	Adyla.	at Ur-meny.	at Ur-meny.	She is good, gives much milk, and her calves are handsome.
Oudass. (Buffalo.)	33					$94\frac{2}{4}$	283	259	274	$217\frac{3}{4}$	$179\frac{3}{4}$	$141\frac{2}{4}$	$1471\frac{2}{4}$	4th June			Slaughtered.	at Ur-meny.	Do. Do.
Szorossa. (Cross of Hungarian and Styrian.)	$3\frac{2}{4}$	$136\frac{3}{4}$	168	157	$217\frac{1}{4}$	$231\frac{2}{4}$	$190\frac{1}{4}$	$114\frac{1}{4}$	32				1251	10th Dec.		Feker.	at Keszi.	at Keszi	Moderately good calves and milk.
Porossa.	$31\frac{1}{2}$	$96\frac{1}{4}$	$186\frac{1}{4}$	$168\frac{2}{4}$	$253\frac{1}{4}$	$281\frac{3}{4}$	$275\frac{3}{4}$	$191\frac{1}{4}$	89	30			1603	20th Dec		Rechter.	Do.	Do.	Sickly calves. Much milk, but bad.
Juna. (Swiss breed born in Hungary.)		$88\frac{3}{4}$	$192\frac{3}{4}$	$199\frac{2}{4}$	$161\frac{2}{4}$	$231\frac{1}{4}$	$199\frac{1}{4}$	$233\frac{1}{4}$	164	$103\frac{3}{4}$	$17\frac{2}{4}$		$1491\frac{2}{4}$	5th Jan.		Ferdinand.	at Tarany.	at Tarany.	Middling calves and milk.
Diana. (Swiss.)		86	199	$181\frac{3}{4}$	164	$249\frac{3}{4}$	223	$223\frac{1}{4}$	$155\frac{3}{4}$	$91\frac{3}{4}$	$3\frac{2}{4}$		$1582\frac{3}{4}$	5th Jan		Ferdinand	Do.	Do.	Good milk, fine calves.
Oreg Csako. Hungarian. Mixed colour; moderate size; fine horns.			$59\frac{2}{4}$	92	152	$180\frac{2}{4}$	$183\frac{2}{4}$	186	174	54			$1083\frac{2}{4}$	31st Dec.			for Labour.		

Regulations

Regulations adopted in the Care of the Flocks of Graf Hunyadi.

1st. A dry and airy shed, or cot, of which the size is proportioned to the number of the sheep, is above all things necessary for these animals. In order to give them proper room, we ought to reckon two feet and a half square for each ewe; as the hay-rack, the partition required during lambing, and the lamb itself, will occupy this space.

2dly. The cot should be cleaned out at least every four weeks, because the exhalation from the dung produces disease amongst the sheep.

3dly. All wetness and moisture is injurious, not only to the health of the sheep, but also to the wool, on which account they ought never to be driven out during rainy weather.

4thly. The dew and hoar-frost in the morning are injurious to them, occasioning cough, colds, and diseases of the lungs, and therefore they should not be taken to the pasture until the dew is gone off.

5thly. Low and marshy meadows, and such as are covered with luxuriant grass, should still more carefully be guarded against; as also stubble lands, in which the scattered grain has sprung up anew.

6thly. In the summer months, when the heat is intense, the sheep must, between the hours of ten and eleven, either be driven back to the cot, or at least be conducted to some shaded place.

7thly. It is indispensably necessary that the sheep should be twice taken to water every day, both in summer and winter.

8thly. A supply of salt is also necessary, of which, in the summer months, four pfund, and in the winter three pfund, should be furnished weekly to every hundred head of sheep, so that they may, at least twice every week, have salt to lick.

9thly. The rams should not be kept in the same house with the ewes, nor the young with the old.

10thly. For fourteen days before the coupling season, the ram should be daily fed with two halbes (equal to three pfund) of oats, and this food should be continued, not only during the coupling, but for fourteen days after; and one ram will thus be sufficient for a flock of eighty ewes, provided great care and attention be paid to him in every other respect during the whole of the season.

11thly. During the lambing period, a shepherd must be constantly day and night in the cot, not with the view of affording assistance at the birth, but in order that he may place the lamb as soon as it is cleaned, together with the mother, in a separate pen, which has been before prepared. The ewes which have lambed

lambed should, during a week, be driven neither to water nor to pasture, but low troughs of water for this purpose are to be introduced into each partition, in order that they may easily, and at all times, quench their thirst. It is also very useful to put a small quantity of barleymeal into the water, for by this means the quantity of the ewe's milk is much increased. When the lambs are so strong that they can eat, they are to be separated by degrees from their mothers, and fed with the best and finest hay and a few oats, being suffered at first to go to them only three times in each day,—early in the morning, at midday, and in the evening,—and so to continue till they can travel to pasture, and fully satisfy themselves. For a week they should then be turned in twice a-day, and for another week once a-day only, to the ewes, when they may be entirely weaned. At first it is enough if a quarter of a pfund of hay be given every day to each lamb, and one halbe of oats be divided amongst six—afterwards, and till they are driven out, half a pfund of hay, and a halbe of oats amongst four, will be sufficient.

Regulations for Winter Feeding.

1st. The winter feeding should begin as soon as the cold and the hoar-frost prevent the growth of the grass; and if, as it often happens, this should be the case so early as the beginning of October, it is not necessary that the sheep should, from this time forward, be kept constantly in the house, and receive all their food there, but they may in dry and clear weather (always observing the fourth of the foregoing regulations) be driven out so long as the grass is not rendered unwholesome by the frost, and the ground is not covered with snow. During all this time, however, they must not be sent out empty, but before going to pasture must have a third part of their usual daily allowance.

2dly. A sheep which is healthy and full grown, will require daily four pfund of food, which must consist of hay and straw. Young sheep should have one pfund less. The daily distribution of food is as follows.

a. From the time when the frost begins, while yet the sheep can go abroad, each receives, in the morning, one pfund and a half of good straw. They are then driven to water, and then to the pasture, where they remain until the dew appears.

b. From the time when the hard frost comes on, and the ground is covered with snow, till twenty days before dropping their lambs, they receive every morning at 5 o'clock, $1\frac{1}{2}$ pf. of clean straw; at 8 o'clock $\frac{1}{2}$ pf. of hay; at 9 o'clock they go to water; at 3 o'clock again $\frac{1}{2}$ pf. of good hay; at 4 o'clock they go again to water; and at 6 o'clock in the evening $1\frac{1}{2}$ pf. of clean straw is again given.

c. From

c. From twenty days before dropping their lambs, till the spring pasturage commences, they have every morning at 5 o'clock 1 pf. of clean straw; at 8 o'clock 1 pf. of good hay; at 9 o'clock they go to water; at 3 o'clock in the afternoon, 1 pf. of fine hay; at 4 o'clock they again drink; and at 6 o'clock in the evening they have again a pfund of clean straw.

3dly. The wethers require the same quantity and order in their food, with this difference alone, that in the commencement of winter these receive $\frac{3}{4}$ pf. of hay, and $3\frac{1}{4}$ pf. of straw, and when the cold weather ceases, 1 pf. of hay and 3 pf. of straw.

4thly. The young sheep have, from the period of the complete setting in of winter, till the spring pasture, every morning at 5 o'clock $\frac{3}{4}$ pf. of clean straw; at 8 o'clock $\frac{3}{4}$ pfund of good hay; at 9 o'clock they go to water; at 3 o'clock they have again $\frac{3}{4}$ pf. of good hay; at 4 o'clock they again drink; and lastly, at 6 in the evening have $\frac{3}{4}$ pf. of straw.

5thly. The lambs have generally, four weeks after their birth, or rather as soon as they can eat, dry food; at 8 o'clock $\frac{1}{8}$ pf. of fine hay each; at 12, every 6 lambs have $\frac{1}{64}$ of a metze of oats, and at 3 in the afternoon again $\frac{1}{8}$ pf. of hay; but when they become stronger, they have at each feeding $\frac{1}{4}$ pf. hay, and amongst four, they have one halbe of oats.

6thly. The lambs are early taught to lick the salt, which is placed upon boards in quantities proportionate to their numbers.

Regulations for Feeding in the Summer Months.

1st. During this season the sheep are entirely fed in the pastures. Yet we must remember, that when the sheep first come into the spring pasture, they continue to receive one half of their winter food, that is, 1 pf. of hay in the morning before they are driven out, and 1 pf. after they come home, until the grass has attained its full perfection.

2dly. As soon as the grass is grown, so that the sheep can find complete nourishment, the winter feeding ceases by little and little, and the following regulations are adopted.

In the morning they remain in the cot till the dew is dried away; they then go to water, and from that are driven to the pastures. Between 10 and 11 o'clock they return to the cot, and after 3 o'clock are driven to water, and then to the pastures, where they remain till the dew falls.

3dly. Salt, finely powdered, should be given them in small troughs every third day before they are driven from the field.

XLV. *New Method for purifying Coal Gas, and at the same time increasing the Product from a given Quantity of Coals. By Mr. S. Parker, Liverpool.*

To Mr. Tilloch.

Sir, — Having noticed in your number for April last, a singular method of purifying coal gas, I take the liberty of communicating to you some additional facts which cannot be wholly uninteresting to those who are engaged in the new and wonderful art of procuring light. Having made the crude coal gas to pass through an arrangement of three iron pipes placed horizontally in a furnace, and kept at a dull red heat, being connected together with a gun-barrel; I found to my great astonishment that the quantity of gas that could be obtained by this means from a given quantity of coal greatly exceeded the quantity obtainable in the usual manner; and further, that the gas was perfectly pure, whilst the quantity of tar produced during the process was considerably less than what is obtained in the ordinary gas-light process. The fluid, which was collected in a vessel interposed between the extremity of the ignited iron pipes through which the gas passed, and the gasometer which received it, contained no vestige of ammonia; but on the contrary, it instantly reddened litmus paper. It possessed an acid styptic taste, and a pungent sulphureous odour. It was of a black colour; and when largely diluted, produced an insoluble precipitate with muriate of barytes. It was sulphuric acid. It is therefore evident that crude coal gas, when made to traverse an ignited iron tube, suffers a remarkable change. The sulphuretted hydrogen gas, which always accompanies this gaseous fluid as obtained from coal, no doubt becomes decomposed during the process, and to it the production of the sulphuric acid must be attributed; —but by what means this decomposition is effected, would not become me to state. It is evident that not only the sulphuretted hydrogen, but the ammonia also, is decomposed; because the fluid which distils over is not alkaline, but decidedly acid. And muriate of barytes and acetate of lead show that it contains sulphuric acid strongly loaded with sulphureous acid gas.

The increase of gas, there can be no doubt, must be attributed to the decomposition which the tar suffers during this process; for it is sufficiently evident that this substance may be wholly converted into oxycarburetted hydrogen gas.

The gas thus produced is perfectly free from sulphuretted hydrogen, as well as from carbonic acid; for it neither disturbs the transparency of a solution of super-acetate of lead, nor barytic water, when made to pass through it. From these considerations there

there is reason to believe that the purification of coal gas, the application of which is daily increasing as a substitute for procuring light, might be effected in a more œconomical manner, by causing the gas to traverse ignited iron vessels, than by the application of quick-lime. The subject is worthy of a strict examination, both in a philosophical point of view, as well as with regard to practical utility. I have the honour to be, sir,

Your obedient servant,

Liverpool, Sept. 3, 1818. S. Parker.

XLVI. *Improvement in the Method of forming Electrical Planispheres.* *By Mr.* Rowland Hill.

To Mr. Tilloch.

Sir, — Nearly four years ago I had occasion to represent some of the constellations and other figures by electricity. Hitherto this had always been done upon glass; but I wished to construct the constellations on a scale which required larger plates of glass than I could conveniently procure. Paper being a non-conductor of electricity, I was induced to make trial of it, and found that it answered my purpose exceedingly well. At that time I constructed four constellations, viz. the Great Bear, the Great Dog, the Ship, and the Scorpion. I made use of that kind of paper which is called Bristol board; the tin-foil may be stuck upon it in the same manner as it is fastened to glass. I also found gold size (a liquid used by the gilders) to be very well adapted to that purpose; and the figures may be secured more completely by covering the whole with a coat of varnish. Since that time I have represented upon paper a considerable portion of the southern sky. I took several sheets of drawing-paper, pasted the edges together, and stretched the whole upon a circular wooden frame four feet in diameter. Upon this apparatus are represented all the stars of the four first magnitudes within forty degrees of the south pole.

In order to give to the stars of the different magnitudes their proper degrees of relative brightness, I took the following method.

For the stars of the first magnitude, I cut the ends of the tin-foil round, and placed them about one-twelfth of an inch asunder. For those of the second magnitude, the bits of tin-foil were pointed, and the spaces between them made as small as possible. To produce a spark of no greater brightness than the stars of the third magnitude, I made the spaces in the tin-foil similar to the last, and pasted over each a small bit of thin paper, through

 which

which the electric sparks are dimly seen. The stars of the fourth magnitude are made by spaces formed in the same manner, but covered with a thicker piece of paper. Thus I was enabled to give to each star its proper degree of brightness; and by these means I conceive a more exact representation of the celestial bodies can be given, than by any other method as yet known.

In this scheme are represented upwards of sixty stars, besides the two great *nebulæ* which appear in the southern part of the heavens. To imitate the latter, I cut two holes in the paper, in the form of the *nebulæ*.—Here I passed the train of tin-foil through the paper, and at the back of the scheme carried it round the edges of these holes, leaving a few intervals for sparks. At the back of each hole I fixed a piece of Bristol board considerably larger than the aperture, and bent so that the part opposite the hole should be about half an inch behind the level of the scheme. The paper thus fixed served as a screen to receive the light of the sparks given at the back round the holes; and being by that means illuminated, while the general face of the scheme was in darkness, filled up the aperture as it were with a nebulous whiteness, giving as I apprehend a tolerably just image of the original.

In damp weather these figures, like almost all electrical apparatus, require to be dried before a fire previously to their being used.

The advantages which are derived from the use of paper instead of glass must be obvious. It is much less expensive. By joining together a number of sheets it can be made of any size; and as it will not break, it is much more portable; which last circumstance must recommend it strongly to such as have frequent occasion to remove their apparatus from one place to another. I am, sir,

Your obedient servant,

Hill-top School, Birmingham, Oct. 12, 1818. ROWLAND HILL.

XLVII. *On the received Theory of Heat.* *By* A CORRESPONDENT.

To Mr. Tilloch.

SIR, — PERMIT me to lay before your readers my reasons for objecting to the received theory of heat. I am strongly inclined to believe there is no such thing—any more than that there is an elementary principle of sound.

Chemists and philosophers agree that the particles of heat are infinitely smaller than any other particles—that insinuating themselves

themselves between those of any other body, is the cause of all expansion—that they decompose some bodies, but never mix with any; and are repulsed by all, when containing a quantity unnatural to them.

Now, sir, my conception is (probably an erroneous one) that heat is only a sensation arising from the sudden decomposition of a body, and that when it is decomposed it cannot communicate the feeling. If a coal is burning or decomposing, it can give the disease to an adjoining coal; and this disease will pass like lightning between the particles of the bottom of an iron steam boiler, and communicate the complaint to the water: the first disorganization of which is steam. Pursue the disorganization in the steam still further, until you get a pressure of 50 or 60 lbs. to the inch, and the steam will then be only milk-warm, from the decomposition being so nearly complete. If heat were an elementary principle, and its particles went through the iron and water without mixing, they would equally pass on through the top and sides of the boiler, more particularly upon the repulsive principle. The expansion of metals may arise from a partial decomposition, which if discontinued, their particles will fall again into their former organization. I believe any sudden disorganization produces light—probably the decomposing and regenerating principle, acting rapidly upon the animal and vegetable organization on the sun's surface, may cause its light, and disorganizes on the earth less (and therefore less sensation of heat) in winter than in summer, because the sun's light then falls much more obliquely upon the earth, and meets with more atmospheric obstruction. The same principle acting upon the growing animal and vegetable system here, is very likely to stimulate by vacuum, and the stomach and capillary system to supply reorganization, and all generative principle.

I wish to throw out these ideas (very hastily put together) to your better informed readers, if you think them worthy a corner of your valuable miscellany. Your obedient servant,

Claughton House, Lancaster, S. S.
17th October, 1818.

XLVIII. *Theory of the Magnetical Variation.* By Mr. Thomas Yeates.

The discovery of the properties of the loadstone having been the labour of ages to explore, and an object with the searchers of nature from the earliest times; it would be a curious and interesting inquiry to ascertain the several stages of the knowledge of this wonderful stone. To enter upon its history is a difficult task, from the want of many notices of which history itself is silent, and without which the progression of the discovery must

 necessarily

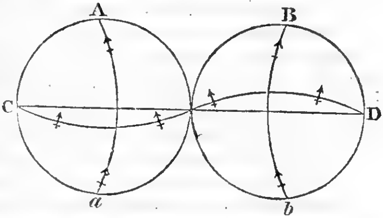
necessarily be interrupted and imperfect. Its general history, however, enables us to conclude that its attractive power for iron was its first known property; that in subsequent times its polarity was discovered, when it was found to possess a line of attraction situate in its own proper axis, and conforming its energies agreeably to that direction. In aftertimes its absolute polarity was discovered in the verticity of the needle, when it was found to conform with the poles of the world, a discovery of all others the most important to navigation and commerce: this was the origin of the mariner's compass. Lastly, the vertical or dipping needle was discovered, whose use and theory remain for the cultivation of the moderns. The variation of the horizontal needle, and the variation of that variation, have been discoveries of later times.

If, as some philosophers have asserted, the earth was originally a right sphere, having its polar and equatorial diameters equal, and its whole surface a perfect globe; it may be argued with equal probability, that at that period the magnetic power strictly conformed itself with the true poles of the world universally, and of consequeuce there could be no variation of the magnetic needle. But since extensive surveys have been made by eminent mathematicians, and it has been determined that the present figure of the earth is a spheroid, having its polar axis less than its equatorial; and that it has been gradually increasing from the power of gravity acting upon its polar surfaces; it is equally possible that the variation of the magnetic poles hath originated from a similar cause; and that so long as the earth shall obtain the form of a spheroid, and increase in its obliquity, so long will the variation of the magnetic poles continue to increase in different parts of the earth, as now we find it. In the former case, which is an hypothesis not altogether improbable, the lines of the magnetic sphere coincided with those of the true sphere; and by consequence all the other imaginary lines constituting either sphere were common, such as the magnetic meridians, equator, and parallels: but in the latter case an absolute difference is found in the existing variation, known to all persons experienced in navigation.

A projection of the lines of the magnetic sphere on a globe is not altogether impracticable, as may appear from what has already been effected in some maps and charts of the Magnetical Variations already published. The lines of quantity being transferred from the chart to the globe, the magnetic meridians, equator, and parallels, may also be laid down with equal precision; and by the help of these the whole complement may be supplied, and thereby the theory of the magnetic sphere will be most interestingly displayed and understood.

I shall

I shall for the sake of illustration give a figure of the Magnetic equator, and meridians.



Let A B represent the two hemispheres of a globe: A *a* B *b* a magnetic meridian: C D a magnetic parallel, which being the greatest of all the parallels in the magnetic sphere, is here, for distinction sake, called the magnetic equator. It is evident from inspection, that the magnetic lines are all curvilinear, and differ from the circles on a globe. That the various inclination of a magnetic meridian with the true meridian, produces the variation in quantity more or less, according to the parts of the earth so affected: and so likewise in any line drawn at right angles with a magnetic meridian; as for instance, the magnetic equator, which in its traverse round the globe, makes different quantities of variation according to its different inclination with the terrestrial equator. This variation increases towards the poles, as represented in the figure: and the same magnetic meridian which in the northern hemisphere produces a west variation, may produce an east variation in the southern hemisphere, as in the meridian marked A *a*; and that meridian which in the southern hemisphere may produce an east variation, may produce a west variation in the northern hemisphere:—in like manner, and by the same rule, the magnetic equator produces the various quantities of east and west variation.

To delineate, either on a chart or on a globe, a series of lines representing the magnetic meridians and parallels, is equivalent to projecting the north and south and east and west magnetic rhumbs. It is owing to the variation of the magnetic rhumb from the true rhumb of the course, that the mariner is frequently necessitated to make observation of the variation at sea in order to correct his course: this is most convincingly taught in the delineation of any magnetic meridian.

According to the present state of the variation between the tropics, the magnetic equator, traversing the whole compass of the globe, completes its range within 23 degrees of latitude or nearly 12 degrees north, and 12 ditto south of the equator. Its maximum quantity of variation any where known is about 23 degrees on the coast of Africa, near the line; and hence the variation increases gradually until it is lost in the lines of no variation east and west. The west variation comprehends about 110 degrees of longitude, reckoned from the western coast of South America about

about 10 degrees south, to the Peninsula of India south of Goa, where an east variation begins, and proceeding eastward through the Manillas, extends from thence over the whole compass of the Pacific Ocean, enters the west coast of South America, and, completing its course through that continent, terminates at the line of no variation on the west coast: in this manner does the magnetic equator traverse the terraqueous globe: and if it were possible for a ship to sail round the globe from any point on this magnetic line, it would perform just such a track, or very nearly so, provided it kept on the magnetic east or west rhumb.

The sum of all the degrees of variation on the magnetic equator, including the increasing and decreasing quantities of east and west, is computed at 90 degrees; whereas about the latitude of 60 degrees north the sum is double that quantity, viz. about 180 degrees.

The highest quantities are found in the west variation at this time prevailing in the North and South Atlantic Seas, and south of the Cape of Good Hope. The highest quantity of east variation at present known is to the south of Cape Horn, where it amounts to about 25 degrees. In general, where the variation is found high in quantity, there the increase or decrease is found the most rapid; but where the quantity is small the change is altogether as slow, and several degrees of longitude are contained in one degree of variation, as appears from the Charts of the Pacific and Indian Seas*.

Having thus far attempted a general description of the cause and theory of the magnetic system, and illustrated my argument by a few plain and obvious principles, I proceed to remark an experiment I some time ago made as follows:—On a globe of a convenient diameter I laid down that imaginary line here called the magnetic equator, and making this the base described several parallels at equal distances towards the arctic circle, when that parallel which might be called the magnetic arctic parallel produced the figure of a shell not unlike that of the common oyster. I mention the experiment with submission to the geologist; who, by further researches into the cause of magnetism, may hereafter afford an almost mechanical solution of that so long attributed to an occult principle. It may be also observed, that as the equatorial line laid down on the globe in the experiment above, was the result of actual experiment in observations of the compass made by recent navigators in most of those seas, so a system of lines produced on this principle alone, by far excels any of those mechanical cases so learnedly described by Dr. Lorimer, and is found to approximate the nearest of all other known methods to the true system actually existing.

* See my Variation Chart lately published.

If

If exact observations of the variation were taken at all the principal coasts, headlands, bays, harbours, and ports in Great Britain and Ireland, and in all other countries where our navigation extends, it would be a very great benefit to the shipping interest both for lives and property, since it happens that frequently the greatest danger is found in making such coasts, harbours, ports, &c. for want of soundings, and the variation:—the former indeed has been well attended to, but the latter still remains a desideratum. But that this is a subject of importance in a commercial country, that an instrument like the mariner's compass should be perfectly understood in all its usefulness, it is sufficient to instance, that without this instrument ships would cease to navigate the otherwise pathless seas, and commerce sink her head into an insignificant nothingness; nations in distant parts of the globe would cease to benefit each other by mutual advantages, and all our knowledge of distant climes and nations would avail us little. It is by this instrument, when neither sun nor moon nor stars appear, that the skilful mariner pursues his course in the most turbulent and boisterous seas; and with a dependence on this his only guide and directrix, he is guided as by an angelic hand towards the haven where he would be. How desirable then must the theory of such an excellent instrument be, and how worthy the pursuit of the mariner, the geographer, the philosopher, and the mathematician!

XLIX. *On the Seasons which are most favourable to the Growth of Fungi, &c. with an Account of some of remarkable Growth this present Year.* *By* A Correspondent.

To Mr. Tilloch.

Sir,—It has generally been believed that wet seasons were favourable to the growth of mushrooms and of *fungi*; in general, the result of the present year, however, contradicts this notion. For after one of the hottest and driest summers which has happened within the memory of the oldest man in this village, the whole of this tribe of plants have been more abundant than for many years past; and some of them have exhibited an extraordinary luxuriance of growth.

The common mushroom, *Agaricus campestris*, has been every where particularly abundant, and of large size: they began to appear in this neighbourhood just after the few days of rain early in September, and are still very numerous in all the open meadows and low lands hereabouts.

That beautiful fungus, the *Agaricus integer*, is also very abundant: we have all the three varieties,—the crimson, the fawn-coloured,

coloured, and the lead-coloured, of large size, in the moist grounds under the oak and beech woods. I have also noticed a plentiful crop of the following species:

Agaricus floccosus, the rough, tawny-yellow agarick, at the root of apple-trees in the orchard.

Agaricus denticulatus, in the long grass.

Agaricus glutinosus, in the same place, but more sparingly.

Agaricus stercorarius, numerous and remarkably beautiful specimens in the dung of horses in the fields.

Agaricus fascicularis. This species came up early even during the dry weather, and is still abundant, but not particularly large.

The *Boletus bovinus* as well as another *Boletus* (which, whether it be a variety of *bovinus* or not I am uncertain) have grown to an enormous bigness. Some have weighed above four pounds, and the diameter of their *pilei* was above fourteen inches. Several other *Boleti* are also abundant and of large growth. But unfortunately, owing to the imperfect nomenclature and arrangement of fungi yet extant, many species of *Boletus* as well as of *Agaricus* which grow here, cannot be identified. Mr. Sowerby's work constitutes a most interesting and useful *sylva fungorum*, wherein the botanist may study and arrange most of the British species, but they have never yet been accurately described.

The most beautiful species we have here is the *Agaricus plicatilis*. The genus *Peziza* has been rather scarce this year.—I am induced to think that the most fruitful sort of season in fungi is one where a hot and dry summer is succeeded by a moderately wet autumn; as by looking back in my Journals I find this frequently to have been the case, while the wholly wet summers and autumns have been less productive.

I shall prepare for some future number a table of observations for about nineteen years past, compared with weather and other collateral phænomena. Yours, &c.

Hartfield, 19th October, 1818. T. F.

L. *Notices respecting New Books.*

Dr. Spurzheim has just published a new work on the Physiology of the Brain, entitled *Observations sur la Phraenologie:* in which he has, in some measure, improved on the arrangement of the organs. He admits now the existence of 35 separate organs in the brain; among them is the convolution called by Dr. Forster, Organ of superstitiousness or mystery. The work is published at Strasburgh, Paris, and London.

Dr. Gall has likewise published another splendid folio fasciculus of plates of the brain, accompanied by descriptions and an elucidation of his doctrine.

LI. *Pro-*

LI. *Proceedings of Learned Societies.*

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

THE fifth anniversary of this institution was held in the Society's New Museum in Penzance, on the 6th of October—Davies Gilbert, Esq. M.P. F.R.S. President, in the chair. The following Annual Report of the Council was presented, and read:

"On reviewing the history of the Society since the last anniversary, the Council is happy to be able to announce the increased and increasing prosperity of the Institution.

"The extensive and elegant Museum, which is now completed, and which is calculated to meet the necessities of an establishment of this kind in its state of perfection, cannot fail to have a happy influence on the fortunes of the Society. At the same time that it affords every convenience for the prosecution of the science of mineralogy and geology, it offers a secure, ample, and elegant depository for all kinds of valuable specimens, which the liberality and public spirit of its members may wish to see concentrated and preserved, for the good of science in general, and for the interests of this county in particular.

"Much greater additions, as well of simple minerals as of geological specimens, have been made to the cabinet, than during any former year; and it is particularly gratifying, as a proof of the great and increasing interest of the Institution with the public, that this augmentation arises entirely from private donations;—the liberality of some of its members compensating the deficiencies which otherwise must have been produced by the present incompetency of the Society's funds. The principal contributors are J. H. Vivian, Esq., W. Maclure, Esq., A. Majendie, Esq., J. Paynter, Esq., Dr. Forbes, and the Rev. J. Rogers.

"In communicating this very gratifying information, the Council cannot avoid expressing their regret that so few new specimens have been obtained from the County mines; and that, consequently, the department of the cabinet set apart for the reception of indigenous ores, which ought to be particularly rich and splendid, continues to be defective, and is eclipsed by many other collections, as well public as private;—a circumstance uniformly exciting the surprise of strangers.

"Considerable accession of information respecting the geological structure of the county has been obtained, which, although not very extensive, is valuable from its accuracy, and as it furnishes plans which may be successfully extended to other districts. The chief contributors in this way are Mr. Joseph Carne, the Rev. J. Rogers, and Dr. Forbes.

"The Council earnestly request the attention of members to this—

this—the grand object of the institution. It is impossible for a few members to undertake the investigation of the whole county. It is therefore hoped, that, with the view of enabling the Society to complete its long-promised but still very defective Geological Map, members will, in their respective districts, endeavour to ascertain the nature and relations of the rocks, and transmit their observations made, and specimens collected, from time to time, to the Secretary, who will be very ready to assist their inquiries by any advice or information in his power. Any person, even although unacquainted with the principles of geological science, can, it is obvious, collect specimens of the various rocks in his vicinity: and members are requested to bear this in mind, with the assurance that collections of this kind, with the various *localities* of the specimens affixed, will very materially promote the important object in view. One grand *desideratum*, and which might be very easily supplied by members resident in the different parts of the county,—is, to ascertain the exact *limits of the different granite and killas districts*.—The farmers and miners, in any part of Cornwall, could give this information to any gentleman that would take the trouble to record it, or to trace the boundary lines on any of the county maps.

" Owing to the great expenses necessarily incurred by the establishment of a new Museum, &c. the funds of the Society cannot be said to be in the most flourishing state:—It is however true, that, chiefly through the liberal donations of some distinguished members, they are so far in a state of progressive improvement as to permit the Council to promise that, before the next annual meeting, all incumbrances will be cleared, and a balance left for promoting the various objects of the Institution.

" By order,

" Oct. 6, 1818. " John Forbes, Secretary."

The following papers were then presented and read:

1. The first paper was by the Secretary, Dr. Forbes, and was a sort of " *Eloge* on Natural History." In descanting on the various advantages arising from this study, the author took notice of its effects in augmenting our relish for the works of nature, by superadding the higher intellectual pleasures to the delights afforded by the mere contemplation of beautiful or sublime objects; its power in preventing the evils flowing from an excessive and vague admiration of the works of nature; its ready and uncumbersome association with other pursuits; its tendency to promote health and cheerfulness; its power in averting and relieving unhappiness; its beneficial influence in leading to religion; its conferring a relish for simple pleasures; its influence in improving the taste and judgement, and in quickening our habits

habits of observation. In considering the very great advantages derived by the *traveller* from this study, he paid a high yet well-merited compliment to Sir Humphry Davy, in nearly the following terms:—'Nor is it only as enabling him more fully to enjoy the natural productions of the countries he visits, that a knowledge of natural history is useful to the traveller: it is a sure introduction and passport to the most valuable acquaintance. Science, like a nobler freemasonry, unites in bonds of friendly fellowship all its cultivators, without regard to kindred, tongue, or nation; and to be a distinguished chemist, mathematician, or naturalist, is to have an irresistible claim on the attention and regard of all the noblest minds of all nations. With this introduction and passport, our truly illustrious townsman, Sir Humphry Davy, is at this very moment riding in triumph through all the most polished nations of Europe. With a consequence which rank or riches alone never could confer, he passes from city to city, conscious that his *name* alone will procure him that attention which the common traveller must want, or owe to other means. To the great and learned of every land he can freely express all his wishes, assured of their ready gratification: in every University and Society, nay in every palace, *He*

"Claims kindred there, and has his claims allow'd."

2. An extremely interesting paper by Mr. Jos. Carne, "On the relative Age of the Veins of Cornwall;" in which the ingenious and industrious author attempts by fair deductions from an immense collection of facts to establish six or seven classes of veins, differing in the order and period of their formation. This paper does not admit of abridgement. It is of considerable length, and was characterized by the Secretary, who read it, as the most valuable communication that had yet been presented to the Society.

3. Two very valuable papers from the pen of the learned Mr. John Hawkins: one "On the Nomenclature of the Cornish Rocks," as fixed by Werner, from specimens presented to that great geologist by Mr. Hawkins: another "On Floors of Tinstone." On this occasion the Society elected Mr. Hawkins an honorary member.

4. A paper "On the Hornblend Formation of the parish of St. Cleer, and on the Geology of other parts of Cornwall," by the Rev. Mr. John Rogers. In this communication the author detailed the various relations and localities of this formation, and illustrated the whole by a map of the district, and numerous specimens of the rocks. Several interesting specimens were also presented by Mr. Rogers, from the slate quarries of Tintagel, illustrating the nature of those appearances that have hitherto been generally considered as exhibiting the impression of shells, and,

and, consequently, as demonstrating the secondary nature of our Cornish slate. Mr. Rogers is of opinion—and it would seem justly—that these supposed organic impressions are mere varieties of structure of the slaty matter itself.

5. A paper by Miss Hill, of Barnstaple, "On the Discovery of Hydrargyllite." From this communication it appears that the brother of Miss Hill, late surgeon in Barnstaple, and not Dr. Wavell, as is commonly believed, was the original discoverer of this mineral.

6. A paper by Dr. Forbes, "On the Geology of that part of Cornwall lying to the westward of Hayle and Cuddan Point;" illustrated by numerous specimens, and by an elegant geological map, and many drawings by Mr. Moyle, assistant secretary. On the present occasion Dr. F. had only time to read that portion of his paper which treated of the *granite* of the Land's-End district, and of the *slate formation*, observable on the shores of the parishes of Burian Sennen, St. Just, Zennor, Towednack, St. Ives, and Lelant. In this paper the author denied the stratification of the Cornish granite; stated the slate formation of the district, which he described to consist of the following five rocks, *hornblend rock*, *greenstone*, *felspar rock*, *slaty felspar*, and *clay slate*; and expressed his belief of the contemporaneous origin of these rocks, and the fundamental granite. As an irresistible argument in favour of this opinion, and as of itself subversive of the Huttonian theory, he adduced the frequent instances observable on the Cornish shores, of granite veins originating in the same rock, intersecting each other, and exhibiting at the point of intersection the appearance called *a shift* or *heave*.

7. Two very interesting papers "On the Tin Trade of the Ancients;"—one by the Rev. Mr. Greatheed, the other by the treasurer, H. Boase, Esq. The latter gentleman brought forward many ingenious arguments in support of a somewhat heterodox opinion which he holds, respecting the knowledge of Britain possessed by the Ancients. He denies that Cornwall was ever visited by the Phœnicians, and maintains, that if any islands denominated *Cassiterides* really did exist, they certainly formed no part of the present British dominions.

Besides the papers above mentioned, there were some before the Society that were not read. Notices were also delivered in by Mr. Joseph Carne, of the quantity of tin and copper raised in Cornwall, Ireland, and Wales, during the year ending June 30th, 1818; and several catalogues of geological and other specimens were presented to the Society by different gentlemen.

In the course of the meeting Lord De Dunstanville took occasion to notice the presentation of a piece of plate, value 150 guineas, to Dr. Paris; to whom, also, thanks were voted for superintending

ing the publication of the first volume of the Society's Transactions.

A Resolution was proposed by Sir Rose Price, respecting the accidents that still too frequently occur in our mines, from the premature explosion of gunpowder. The Hon. Baronet, in commenting on these accidents, animadverted severely on the conduct of those mine agents and proprietors, whose apathy or prejudice continues to permit the occurrence of such fatal accidents, when simple and efficacious means of prevention exist in the *safety instruments* invented by Captain Chenhalls.

All the Officers of the Society were re-elected, and the following gentlemen chosen Vice Presidents and Members of the Council for the present year: viz.

Vice Presidents—Sir C. Hawkins, Bart.; W. Rashleigh, Esq.; F. H. Rodd, Esq.; Rev. John Rogers.

Members of the Council—Jos. Carne, L. C. Daubuz, R. W. Fox jun.; W. R. Hill, H. Grylls, S. Davey, S. John, H. P. Tremenheere, Esquires; Rev. W. Hockin; and Capt. E. Scobell, R.N.

From the Report of the Curator, Mr. Edward Giddy, of whose correct, lucid, and elegant arrangement of the mineralogical cabinet much approbation was expressed by the Meeting, it appears that upwards of 1600 new specimens have been added to the cabinet since last anniversary; an augmentation which, we understand, arises entirely from private donations.

LII. *Intelligence and Miscellaneous Articles.*

NORTHERN EXPEDITION.

The following curious and interesting letter to the Honourable Captain Napier, R.N. from on board the Isabella, one of the ships at present employed in the Northern Expedition, is understood to be from the pen of Captain Sir John Ross.

"His Majesty's Ship Isabella, off Sugar Loaf Bay, Davis's Straits, July 12, 1818. lat. 74. 2. N. long. 58. W.

"My dear Sir,—I take the opportunity of a Leith ship to let you know what we are about in this icy region; a few extracts from the log will give some idea of our proceedings. On the 3d May left Shetland, and had a tolerable fair passage across the Atlantic; on the 22d were in longitude off Cape Farewell; 2 deg. south of it found our variation increasing as we went west, temperature of air and water nearly the same as at Shetland, thermometer at 42 or 43 deg. On the 26th saw the first iceberg, lat. 58. 38. long. 50. 54.: we now had snow and sleet, thermometer at freezing, a good deal of loose ice all round. June 2, in lat. 65. long. 56. were close in with the main west ice, which we supposed extended the whole way to the American coast; on the

the 4th made the Greenland coast, in lat. 65. 42. but did not stand close in; the land here appeared something like the north coast of Spain, and about the same height, the mountains very precipitous, and terminating in ragged peaks. We continued our course to the northward, as the winds and ice permitted, keeping on the edge of the main west ice, which we found trending to N. E. On the 8th, in lat. 68. 20. long. 55. 50. a few leagues off the Greenland coast, we were so hemmed in with ice on all sides that we could not run through; a fine S. W. gale was blowing, and we were obliged to tack about where we could find room. On the 9th we made fast to an ice-berg aground in 38 fathoms, about a mile off shore. The mode of anchoring to ice is very easy: the boat goes a-head with the anchors, and fixes them before the ship approaches; when ready, the ship stands in and makes fast, bow to the ice: a low berg that the bowsprit lies over is preferred, and aground if it can be had. On the 10th were obliged to get under way, a small change of wind setting a large body of ice upon us; we continued plying where we could find open water, and fell in with a whaler, the first we had seen, who informed us that none of the whale ships had been able to get past 70½ deg.; that the ice to the northward was still fast. On the 14th called at the Whale Islands, where there is a Danish factory. The Danish Resident came on board; from him we could get little information, except that the preceding winter had been very severe. On the 16th we reached 70. 39. N. no clear water to be seen northward: made fast to an ice-berg about a mile off the N.W. end of Waygat or Hare Island. We found here most of the whale fishers waiting for an opening to go north, the fishery to the southward having failed this season. Waygat is eight or nine miles long, 1200 or 1500 feet high, uninhabited, some of the rocks basaltic. Coal is found near the surface on the N. E. part of this island. Some grouse were shot, the cock perfectly white, the hen not unlike that of Scotland. I saw one hare pure white. On the 20th the ice opened a little to the northward, when we began to warp and tow the ship through the slack, the winds light and variable, and frequent calms. On the 26th were only 20 miles from Waygat, where we got into a piece of clear water that carried us to the land ice on the north side of Jacob's Bight, lat. 70. 2½. We found ourselves in 54. 17. W. per lunars, which agreed well with chronometers. We swung the ship, and took azimuths on board at every four points. Corresponding azimuths were taken at the same time on the ice. The observations were not taken in so correct a manner as might be done to form a just estimate of the deviation of the compass by ship's attraction. The idea here at present is, that the compasses are not attracted in a line with the

the ship, but obliquely. From my own observation, I find that the bearings of distant objects with the ship's head north and south correspond, which would not be the case if the attraction of the ship was not fore and aft, but athwart. The azimuths taken with the ship's head north or south generally agree. It is supposed likewise that the error arising from the ship's attraction has increased with the variation and dip. As there were no observations made before leaving England on the ship's attraction, we must have patience until the variation is again decreased.

"I think that the error has been constant the whole voyage. The ship's head at west gives, according to my own observation, an increase of variation 16 deg.; at east a decrease of 16 deg. On the 27th we cast off from the ice with the prospect of an opening, and cruized about in a narrow pool till the 2d July, when a fine fresh breeze opened a passage for us. On the 3d we were in 71. 30.—on the 4th 72. 30. On the 7th, in lat. 74. were again obstructed by ice, the bergs and flaws much heavier by far than those we have hitherto seen.

"We are now in the same place that Baffin, two hundred years ago, anchored: we find the Three Islands just as he describes them; he makes them in 74. 4. We make them 74. 1½. Baffin gives an honest account of them. We stretched to the westward on the 9th and 10th, but found the sea all fast. We are now in daily expectation of the wind shifting to the N.E. and blowing strong, which is the only thing that will do us good. It is strange that, at the same time of the year, almost to a day, Baffin should have been stopped by ice in the same place; he likewise stood west without finding clear sea— His account takes him to 78. N. but he does not say he was at the top of the Bay, or saw land there. Our voyage hitherto has been very pleasant—Since the middle of June we have had very fine weather, the thermometer in sun 76.—sometimes in the shade it is at a mean about 33. or 34. sometimes below the freezing point. For five or six weeks we have only had occasion to take in the first reef once. The water is as smooth as a mill-pond all weathers. We have scarcely seen rain—our changes of weather are from cloudy to thick fogs, and sometimes light falls of snow. Sometimes the sun shines unclouded the whole twenty-four hours. We have seen two whales only, and have heard but of one being killed since we have been here—they are all north of us. Bears are as scarce—one has been seen. A great number of the gull tribe have been shot, and we sometimes procure a mess of eider ducks; seals are more abundant, but we don't trouble them. The coast of Greenland, where we saw it, to the southward of 70½, is higher than to the northward of that latitude. Here the coast consists of many high bold bluff-like head lands, which, closer to, are found to be islands. The main land is one continued ridge of smooth snow, which appears like a cloud. I suppose the ground has not been uncovered since the Flood. The islands in general are clear of snow.—There are no inhabitants to the north of 72. 30. on this coast. We had some of the natives on board from 68. 30. 70½. and 72½—they are all the same people, the women dressed in the same manner as the men, only their hair tied on the crown of their head, and a small sort of peak on the fore and after part of their jackets. We have been so anxious to get on the more interesting part of our voyage, that little attention has been paid to the natives here. The most astonishing things to be seen here are the ice-bergs—their size an dnumber surpass fancy. From the 65th deg. to this, the sea is literally covered with bergs, and we see no end to them: where they are generated is yet unknown to us; it is not in 74. or to the southward on this coast. That they are formed on the land is certain, from the many stones of great size which are seen: some of them are covered with sand and dirt, others have regular strata of sand and stones running through them horizontally. They are of all forms—generally they have a high cleft on one side, and shelve down to the water on the other: some exceed 200 feet perpendicular all round.—Loose or stream ice consists of pieces about the size of an acre and under, about a foot above the surface: when it is blown together by strong winds, one piece is edged up on the top of another, it is then called packed ice, or a pack. Flaws are large pieces of field ice. The ice generally drifts with the wind, though a current *must* set southward, or how would the bergs find their way south? We have not been able to detect any current. The flood tide sets here from southward. At Waygat we had a rise and fall of seven feet at spring tides. Where the ice-bergs drift into shallow water (that is to say 150 fathoms or under), they ground, and obstruct the passage of the smaller ice, and form barriers which it is difficult to pass. In 68. there is a reef, in 70½. another, in 74. another, generally found full of ice by the fishers; we have found it the same. In standing a few leagues from land we find 85 fathoms here, closer on 150, 90, and so on. The water runs in small streams from the bergs, so we have no difficulty in procuring it. I am now more sanguine of getting a long way north and west than I was at the first of the voyage. I am of opinion that the ice will clear away, and that very soon. The small ice has been for some time consuming fast, and will be all dissolved by the end of this month, even without wind to break it.

"*July* 18.—Yesterday an opening in the ice enabled us to get to 74. 43. when we were again stopped—the ice here much heavier, and in fields. We are at present fast to a field, in thick fog, which

which freezes as it falls, and covers every thing with ice. When at the Three Islands we made some further observations on the bearings of distant objects by compass, and found a change of bearings of three points at east and west. The compasses for some time have traversed very sluggishly; this, we suppose, is owing to the increase of dip. I think it not at all improbable, that, as the terrestrial magnetism begins to act more inclined to the compass needle, it will act with less force—the iron of the ship still acting at the same angle, draws the needle towards the centre of the ship, which causes this great deviation of the compass; and should we reach the place where the dip is 90, I think the compass will stand always north and south by the magnetism of the ship. We did not speak the Leith ship; this must therefore take its chance of any craft. The description of Greenland given in Dr. Brewster's Encyclopædia is so correct, that no one need add any thing more on that subject until the face of the country is again changed. I had picked up some stones from the different parts where we touched, for Dr. Brewster; but having since read the article Greenland in his book, I find that he knows more than a ship load could tell him*. We have been unfortunate in killing animals, so that I have got no crystalline lenses for him. I bespoke some eyes of whales from the fishers; but the chance of their killing fish, or of our falling in with them again, is doubtful.—If we fall in with a Leith ship, I will send the stones.

" *July* 22.—Yesterday we got an opening, which brought us to the 75th deg. The whales begin to make their appearance, several having been killed within these eight days. The main land appears one continued smooth ridge of snow, only here and there the black peak of a mountain appearing; some large islands on the coast less covered with snow; the land ice extends three or four leagues off, so there is no prospect of approaching the coast hereabouts. We sound occasionally in from 200 to 400 fathoms soft mud and small stones. Three days we were beset in the ice; could not observe any current, by the lead lying at the bottom, though the ice on the surface was in motion.

" *July* 25.—Lat. 75. 21. long. 60. 30. Got here this morning, and now see more clear water than we have seen for some time past. We must now be crossing the magnetic Pole fast, as the variation increases so much. It is puzzling to find out exactly how the ship is steering by the compass; what with the great variation and error, arising from the ship's attraction and the sluggish traversing of the compasses, we must consider some time before a course or wind can be properly named. We are now the

* The article 'Greenland' was written by Sir Charles Giesecke, who spent seven years in that country.

northernmost ship, and have made fast to the ice, on purpose to send away a few letters. The fish are turning so very plenty, that all the ships are employed, and probably will proceed no further north this season. This afternoon we got jammed between two flaws, and seeing a ship taking fish at a short distance from us, Captain Ross sends all his dispatches with her, in case of not falling in with any other, or ice opening and separating us. You will hear from me by every oppportunity.

"I am, &c. J. R.

"P. S. While writing these last lines, the ice has closed all round us, and fast to the northward. You may guess how fickle it is. We are now about three miles off a small rocky island, in 270 fathoms mud; the island four or five leagues from the main land, and ice connecting it. The temperature of the water today is 36 deg. higher than it has been for some weeks. We see land bearing N.W. by W. true."

Failure of Captain BUCHAN'S *Branch of the Expedition.*

On Oct. 15, Mr. Fisher, an officer of the Dorothea (discovery ship), Captain Buchan, arrived at the Admiralty with dispatches, announcing the return of that ship and her consort the Trent sloop from the North Pole. It appears that the highest latitude the ships ever attained was about 80. 30. longitude 12 east. They attempted proceeding to the westward; but, as in the case of Captain Phipps in the Racehorse in 1773, they found an impenetrable barrier of ice. The ships proceeded nearly over the same space as Captain Phipps did, and met with similar impediments to those experienced by that officer.

Although the object of the discovery ships under Captain Buchan has been defeated for the present by the unfortunate accident which befel the Dorothea, and in consequence thereof the question of a practicable passage over the North Pole remains precisely where it did, there is every reason to believe that the North-west Expedition, under Captain Ross, will prove successful.

The following is an extract of a letter from Mr. William Hurst, master of the ship Ariel, to his owners, Messrs. Hammond and Smith, dated Stromness, Oct. 8:

"A heavy gale came on on the 9th of August from the southward, and we got close beset amongst heavy flaws of ice, where we were detained till the 3d of September, without any possibility of getting out. The ship was in great danger while we were beset, but happily we escaped clear off; and I observed in lat. 76. 8. N. and there found an open sea. We stood off to the westward for 12 hours, *and met with no ice*. The discovery ships

ships got out of sight of us about the middle of August; and, from the appearance it had when we left the ice, I doubt not but they may find their wished-for passage."

The following is an extract of another letter, dated Stromness, 9th October:

"The Everthorpe of Hull, Captain Hawkins, is arrived here, all well, with fifteen fish, one of them a small one, said to be the last ship from Davis's Straits this season—says, he is certain the discovery ships have got round the land, as he was in lat. 76. 28. on the 4th September, and could not see a bit of ice to the N.W. or N.E. of him at that time."

The Arctic Regions.

As it is now very probable that the persons employed on the northern expeditions will winter in these drear and inhospitable regions, it may not be uninteresting to our readers to recall to their recollection the features of the revolving year, as observed in the Arctic Circle.

After the continued action of the sun has at last melted away the great body of ice, a short and dubious interval of warmth occurs. In the space of a few weeks, visited only by slanting and enfeebled rays, frost again resumes his tremendous sway. It begins to snow as early as August, and the whole ground is covered to the depth of two or three feet before the month of October. Along the shores and the bays, the fresh water, poured from rivulets, or drained from the thawing of former collections of snow, becomes quickly converted into solid ice. As the cold augments, the air deposits its moisture, in the form of a fog, which freezes into a fine gossamer netting, or spicular icicles, dispersed through the atmosphere, and extremely minute, that might seem to pierce and excoriate the skin. The hoar frost settles profusely, in fantastic clusters, on every prominence. The whole surface of the sea steams like a lime-kiln; an appearance called the *frost-smoke*, caused, as in other instances of the production of vapour, by the water's being still relatively warmer than the incumbent air. At length the dispersion of the mist, and consequent clearness of the atmosphere, announce that the upper stratum of the sea itself has become cooled by the same standard; a sheet of ice spreads quickly over the smooth expanse, and often gains the thickness of an inch in a single night. The darkness of a prolonged winter now broods impenetrably over the frozen continent, unless the moon chance, at times, to obtrude her faint rays, which only discover the horrors and wide desolation of the scene. The wretched settlers, covered with a load of bear-skins, remain crowded and immured in their hut, every chink of which they carefully stop up against the piercing external cold; and cowering about the stove or the lamp, they seek to doze away the tedious night. Their slender stock of provisions, though kept in the same apartment, is often frozen so hard as to require to be cut with a hatchet. The whole of the inside of their hut becomes lined with a thick crust of ice; and, if they happen for an instant to open a window, the moisture of the confined air is immediately precipitated in the form of a shower of snow. As the frost continues to penetrate deeper, the rocks are heard at a distance to split with loud explosions. The sleep of death seems to wrap up the scene in utter and oblivious ruin.

At length the sun re-appears above the horizon; but his languid beams rather betray the wide waste, than brighten the prospect. By degrees, however, the further progress of the frost is checked. In the month of May, the famished inmates venture to leave their hut in quest of fish on the margin of the sea. As the sun acquires elevation, his power is greatly increased. The snow gradually wastes away; the ice dissolves apace; the vast fragments of it, detached from the cliffs, and undermined beneath, precipitate themselves on the shores with the noise and crash of thunder. The ocean is now unbound, and its icy dome broken up with tremendous rupture. The enormous fields of ice, thus set afloat, are, by the violence of winds and currents, again dissevered and dispersed. Sometimes impelled in opposite directions, they approach and strike with a mutual shock, like the crash of worlds; sufficient, if opposed, to reduce to atoms in a moment the proudest monuments of human power. It is impossible to picture a situation more awful than that of the poor crew of a whaler, who see their frail bark thus fatally inclosed, expecting immediate and inevitable destruction.

Before the end of June, the shoals of ice in the arctic seas are commonly divided, scattered, and dissipated. But the atmosphere is then almost continually damp, and loaded with vapour. At this season of the year, a dense fog generally covers the surface of the sea, of a milder temperature indeed than the frost-smoke, yet produced by the inversion of the same cause. The lower stratum of air, as it successively touches the colder body of water, becomes chilled, and thence disposed to deposit its moisture. Such thick fogs, with mere gleams of clear weather, infesting the northern seas during the greater part of the summer, render their navigation extremely dangerous. In the course of the month of July, the superficial water is, at last, brought to an equilibrium of temperature with the air, and the sun now shines out with a bright and dazzling radiance. For some days before the close of the summer, such excessive heat is accumulated in the bays and sheltered spots, that the tar and pitch are sometimes melted, and run down the ships' sides.

The

The ice which obstructs the navigation of the arctic seas consists of two very different kinds; the one produced by the congelation of fresh, and the other by that of salt water. In those inhospitable tracts, the snow which annually falls on the islands or continents, being again dissolved by the progress of the summer's heat, pours forth numerous rills of limpid streams, which collect along the indented shores, and in the deep bays inclosed by precipitous rocks. There, this clear and gelid water soon freezes, and every successive year supplies an additional investing crust, till, after the lapse of several centuries, the icy mass rises, at least to the size and aspect of a mountain, commensurate with the elevation of the adjoining cliffs. The melting of the snow, which is afterwards deposited on such enormous blocks, likewise contributes to their growth; and by filling up the accidental holes or crevices, it renders the whole structure compact and uniform. Meanwhile, the principle of destruction has already begun its operations. The ceaseless agitation of the sea gradually wears and undermines the base of the icy mountain, till at length, by the action of its own accumulated weight, when it has perhaps attained an altitude of a thousand or even two thousand feet, it is torn from its frozen chains, and precipitated with a tremendous plunge into the abyss below. This mighty launch now floats like a lofty island on the ocean, till, driven southward by winds and currents, it insensibly wastes and dissolves away in the wide Atlantic.

STEAM ENGINES IN CORNWALL.

From Messrs. Leans' Report for September 1818, it appears that the following was the work performed during that month, by the engines reported, with each bushel of coals.

		Pounds of water lifted 1 foot high with each bushel.	*Load per square inch in cylinder.*
23 common engines averaged		23,005,446	various.
Woolf's at Wheal Vor	..	28,377,658	17·8 lib.
Ditto Wh. Abraham	..	47,540,653	16·8
Ditto ditto ..	..	21,565,551	6·7
Wheal Abraham engine	..	36,753,433	10·9
United Mines ditto	..	39,608,998	18·7
Treskirby ditto ..	..	37,320,477	10·8
Wheal Chance ditto	..	30,817,772	11·9

SCIENTIFIC RESEARCHES IN BRASIL.

Mr. William Swainson, F.L.S. has just returned to this country from Brasil. He quitted England in the autumn of 1816, for the sole purpose of exploring that distant country, and collecting its splendid and extraordinary productions. Mr. S. proceeded in in the first instance to Pernambuco, where he was detained by the insurrection which broke out there the following year. On tranquillity being again restored, he proceeded (partly by land) to Bahia, where he remained till the beginning of this year, chiefly occupied in visiting different parts of that province, and in a journey towards the interior, in the desert tracts of which, besides many other unknown birds, he was fortunate in discovering the superb *Psittacus augustus*, the hyacinthine Macaw *. At Bahia Mr. S. fell in with the two Prussian naturalists sent out by that government, Mr. Freyeries and Dr. Sellow, the latter a young but able botanist; they had just completed an arduous journey along the coast from Rio de Janeiro, and which had taken them eighteen months. During part of this time they had lived with the Bootocoodi Indians, a tribe possessing customs the most singular, and of whom little hitherto is known. While Mr. S. staid in this province, these naturalists did not go beyond the shores of the bay. Among other unknown animals, Mr. Freyeries had discovered a species of bat, perfectly white, with an appendage at its tail resembling the two last joints of a small rattle-snake. From Bahia Mr. S. proceeded to Rio de Janeiro, where he met with an assemblage of scientific men sent out by almost every continental sovereign, though by none with such munificence as the Emperor of Austria, whose daughter the Princess Leopoldina is united to the heir of the Portuguese and Brasilian crowns. The scientific mission which accompanied her to Brasil consisted of no fewer than seven persons, viz. Professor Micken, botanist; M. Schott, gardener; Dr. Pohl, mineralogist; M. Buckberger, botanical painter; and M. Enter, landscape painter; M. Natterer, zoologist, with an assistant. It is lamentable however to add, that with means so liberal and enlightened, little, comparatively, has been done; for, from various causes, not one of the party had been more than forty miles from the capital; all had embarked for Europe this spring, with the exception of the two latter, who are preparing for a journey into the vast province of Matto Grosso, situated in the centre of South America, and which, in every sense, may be considered as almost unknown. France has to boast of M. Augr. de St. Hilaire, who, as a botanist, has ably explored the province of Minas, and the banks of the Rio St. Francesco, and who is meditating another journey; and Dr. Langsdorff, the Russian Minister, is supplying the Imperial Museum with a multitude of objects in every branch of natural history. Even the principality of Tuscany has sent out an experienced botanist, Professor Raddi, of

* The only specimen ever seen of this bird was purchased alive by the then Lord Orford, for 200 guineas. See also Shaw's General Zoology, vol. viii. p. 393.

Florence;

Florence; and the Portuguese Court, ashamed of seeing other nations employed in collecting and recording the productions of their own woods and mountains, have recently established a National Museum, and taken measures for active researches. Added to all these, the King of Bavaria some time ago sent out Messrs. Spix and Martins, the one a zoologist, the other a botanist, both known by their works to the scientific world, and who are now travelling the provinces between Rio de Janeiro and Bahia. By such enlightened policy, and various talent, this luxuriant country will soon be better known. But the ignorance which until very lately has existed respecting it, added to the vast extent of territory it covers, will for years render it a wide and almost boundless field for the researches of the philosopher and the naturalist. The collections made by Mr. Swainson, in botany, ornithology, and entomology, are, we understand, very extensive, particularly in the latter department, and a relation of his travels may probably be laid before the public.

DEER DESTROY SERPENTS.

The following extract from Col. Maurice Keating's Travels presents a curious fact in natural history:—"Mr. Dowling, who passed many years of his life in and about St. Ildefonso, in the course of adverting to the progress of his manufactory, had frequently been eye-witness to a very surprising occurrence here—deer swallowing live serpents. He describes the fact as follows: The deer, after discovering, examines the serpent for some time; he then places both his fore feet successively on it, standing somewhat straddling, so as to keep the reptile distended to its utmost length. He has probably in the first instance secured the head. The deer then puts his mouth down to the middle of the snake, thereby taking it in; and then raising his head and neck to a horizontal level with his body, and protruding his chin, so as to make his head on a line with his neck, he appears to suck the snake down double, moving his jaws for the purpose, but not chewing; the head and tail of the reptile, writhing, being the last parts of it seen. Of this strange appetite and extraordinary process Mr. Dowling had seen numerous instances. It brings to mind the '*cervi pasti serpente medulla*' (a necromantic ingredient) of the poet."

The foregoing fact brings to our recollection another of the same kind. Swine are also devourers of serpents. In some parts of America they take advantage of this fact. When a piece of ground infested with these reptiles is to be cleared, having first inclosed the piece sufficiently to prevent the swine from wandering beyond their allotted boundary, they drive a number of them into the ground; and when these have had sufficient time

time to clear it of snakes, the work of clearing away the wood is proceeded on.

PRASE DISCOVERED IN SCOTLAND.

We understand that Dr. MacCulloch has recently discovered *prase* in Scotland, and that it is found in Loch Hourn forming veins in a gneiss which contains actinolite schist. To this substance it is known to be indebted for its colour. Our mineralogical readers will be glad thus to know that they may increase their collections of British minerals by a variety of quartz which is no where very common. It may probably not be known to them that the "prase" mentioned in Jamieson's Mineralogy is a quartz penetrated and coloured by chlorite, a substance very common in Argyllshire, but essentially distinct from the mineral in question.

TAR LIGHT FOR STREET LAMPS.

It is stated in an American newspaper that Professor Hare, of William and Mary College in Virginia, has invented an apparatus for burning tar instead of oil, in lighting cities and manufactories.—It is said that tar burned in this apparatus gives a strong and clear light; and it is computed, that four or five barrels of tar will serve a lamp for one year, and will give eight times the light of a common street lamp. The following is given in the Union as a description of the apparatus:—It "consists of a fountain reservoir to hold four or five pounds of tar to supply the lamp at a uniform height, and a lantern with a draught pipe attached to it.—The lamp presents at one end a cylindrical mouth for receiving the pipe of the reservoir; at the other end a cylindrical cup, in which the tar is ignited, the flame being drawn up through a central hole in the bottom of the lantern so as to occupy its axis in passing to the draught pipe. All the air which supplies this is made to meet in the same axis, and thus to excite the combustion."

LIST OF PATENTS FOR NEW INVENTIONS.

To Thomas Parker the younger, of Seven Oaks, in the county of Kent, bricklayer, for his method or methods of regulating and improving the draught of chimneys.—2 months allowed for specification, dated 5th Oct. 1818.

To William Finch, of Birmingham, gentleman, for certain improvements in bridles for horses, which he intends to denominate "Philanthropic bridles."—12th Oct.—2 months.

To Samuel Hobday, of Birmingham, snuffer-maker, for his new and improved method or principle in the making of snuffers without any spring or lever.—12th Oct.—2 months.

To Sir William Congreve, of Cecil-street, in the city of Westminster, baronet, for his discovered and invented certain new methods of constructing steam-engines.—19th Oct.—6 months.

Meteoro-

Meteorological Journal kept at Walthamstow, Essex, from September 15 *to October* 15, 1818.

[Usually between the Hours of Seven and Nine A.M. and the Thermometer (a second time) between Twelve and Two P.M.]

Date.	Therm.	Barom.	Wind.
September			
15	59 68	29·90	SW.—Gray and windy; some clouds; fine day till 2 P.M. then rain till after 5 P.M., and then a hard shower; cloudy night.
16	45 61	29·65	SW.—*Cirrus* and clear; very fine morn; fine day; clouds and sun, and very slight showers; showers of rain and wind at night.
17	43 58	29·91	N.—Very clear and windy; clouds and clear; moon-light and *cumuli.*
18	48 59	30·10	S—SE.—Hazy; slight rain at 9 A.M. till after 2 P.M.; cloudy at 9; stars and *cumuli* at 11 P.M.
19	54 67	29·90	SE.—Clear, and fine *cirrocumuli*; fine day; and some very slight showers; a remarkably *black* small *cumulus* passed over the moon about 11½ P.M.
20	59 68	29·60	SE. — Fine morn; *cirrostratus* and clear; some slight showers; fine day; cloudy night.
21	59 67	29·40	SE.—Cloudy; rain from about 9 A.M. to 1 P.M.; sun and clouds; a great shower about 4 P.M.; clear night.
22	52 67	29·60	SE.—Clear and *cirrostratus*; very fine day; bright star-light. Moon last quarter.
23	53 67	29·60	SE.—Clear morning; fine day; some showers after noon; dark and rainy.
24	54 67	29·60	SE.—Rain and hazy; fine day; sun and *cumuli;* dark night.
25	52 68	29·60	N.—Gray morn; fine day; very rainy since 5 P.M.
26	51 63	29·40	W—SW.—Rain till about 9 A.M.; showers and sun; fine day; dark and rainy.
27	51 64	29·60	SE.—Fine morn; sunshine; a slight shower; rain after 3 P.M.; star-light.
28	59 70	29·60	SE.—Fine sun and *cirrostratus;* very fine hot day; fine red sunset; star-light.
29	59 67	29·60	SE—E.— Clear and *cumuli;* fine hot day; calm; stars, but not very bright.
30	59 64	29·40	E—SE.—Rain and wind early; fine hot day; star-light, very hot. New moon.

October

Date.	Therm.	Barom.	Wind.
October			
1	56 67	29.40	SE.—Clear and *cirrostratus;* a small shower about 10 A.M.; clouds, wind, and sun; showers; clear and star-light.
2	49 64	29.55	E—SE.—Sun; clear high; *stratus* low; white dew; fine day; very bright star-light, and *aurora borealis.*
3	57 64		SE—SW.— Orange red-sun-rise; clouds and wind; rain began before 8 A.M. and lasted almost incessantly till near 9 P.M.; star-light.
4	54 60		SW—NW.—Sun and hazy; very fine morning; a great shower and thunder and lightning about noon; some showers after; star-light and windy.
5	49 57	29.45	NW—S.—Clear, *cirrostratus,* and windy; sun, and very slight showers; at 4 P.M. hazy and damp; some rain; dark night early; bright star-light at midnight.
6	41	29.29	W—NW.—Clear, & some *cirrostratus;* wind; very fine day; at 3 P.M. Ther. 56; a shower after 5 P.M.; bright star-light.
7	37 55	29.50	NW—N.—Sun; hazy; strong dew; very fine day; star and moon-light bright.—Moon first quarter.
8	38 56	29.70	NW—N.—Foggy morning; very fine day; sun and wind; fine moon and star-light.
9	38 57	29.80	SE—W—SW.—Very foggy early; very calm; very fine day; dark night.
10	55 66	29.65	SW.—Hazy morning; damp hazy day; neither moon nor stars visible, but not very dark.
11	58 62	29.50	SW.—Hazy; slight rain early or in the night; gray; showers and some sun; rain.
12	45 57	29.70	SW.—Fine clear sun-rise; hazy at 8 A.M.; very fine hot day; bright moon-light.
13	49 59	29.80	E—S.— Gray morning; fine sunny day; bright moon and star-light.
14	52 66	29.91	E—SE.—Fine morning; white dew and sun; very fine hot day; bright moon-light. Full moon.
15	54 66	29.91	SE—Red sun-rise; fine morning, and some slight showers after 8 A.M.; clouds and sun; fine day; very warm; slight rain and *cirrostratus.*

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

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

1818	Age of the Moon.	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Sept. 15	15	54·	29.71	Rain
16	16	55·	29·67	Cloudy—rain in the evening
17	17	59·	30·13	Ditto
18	18	61·5	30·10	Ditto
19	19	66·5	29·87	Ditto
20	20	63·	29·71	Ditto
21	21	61·	29.47	Rain
22	22	63·	29·76	Cloudy
23	23	64·	29·75	Ditto—heavy rain at night
24	24	64·	29·69	Fine—do. A. M.
25	25	60·	29·75	Cloudy—do. at night
26	26	58·	29·65	Ditto—rain A.M.
27	27	59·	29·71	Fine
28	28	69·5	29·79	Very fine
29	29	65·5	29·81	Ditto
30	new	60·	29·63	Cloudy
Oct. 1	1	66·	29·60	Fine
2	2	64.5	29·73	Very fine
3	3	63·	29·57	Rain
4	4	56·5	29·56	Ditto
5	5	57·	29·40	Cloudy
6	6	56·	29·39	Fine
7	7	55·	29·66	Ditto
8	8	55·	29·90	Ditto
9	9	59·	29·89	Cloudy
10	10	61·5	29·65	Ditto—rain in the evening
11	11	64·5	29·60	Fine—heavy rain, storm at night
12	12	59·	29·82	Ditto
13	13	60·	22·91	Ditto—shower A. M.
14	full	67·	30·07	Ditto

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For October 1818.

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	55	64	60	29·70	27	Showery
28	60	66	60	·75	36	Fair
29	61	69	57	·63	40	Showery
30	60	66	59	·50	40	Showery
Oct. 1	60	68	57	·53	29	Showery
2	60	66	57	·63	41	Fair
3	59	64	59	·47	0	Rain
4	57	65	56	·50	0	Stormy
5	56	60	50	·50	36	Showery
6	45	56	48	·35	30	Showery
7	44	58	47	·55	35	Fair
8	44	55	47	·69	30	Fair
9	42	60	55	·82	32	Fair
10	55	62	57	·60	29	Cloudy
11	58	63	50	·54	0	Rain
12	57	64	50	·90	36	Fair
13	56	65	57	·86	30	Cloudy
14	57	68	59	·97	39	Fair
15	60	67	57	·95	31	Cloudy
16	60	69	60	30.02	36	Fair
17	55	64	55	·02	35	Fair
18	55	62	55	29.93	39	Fair
19	55	63	56	30·04	40	Fair
20	54	61	50	·16	36	Fair
21	45	56	52	·09	42	Fair
22	49	55	48	29·97	30	Fair
23	49	50	49	.99	29	Cloudy
24	50	51	50	30·03	28	Cloudy
25	52	59	55	·05	30	Fair
26	55	62	54	·12	36	Fair

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

LIII. *On Measuring the Depths of Cavities seen on the Surface of the Moon.* *By* A Correspondent.

To Mr. Tilloch.

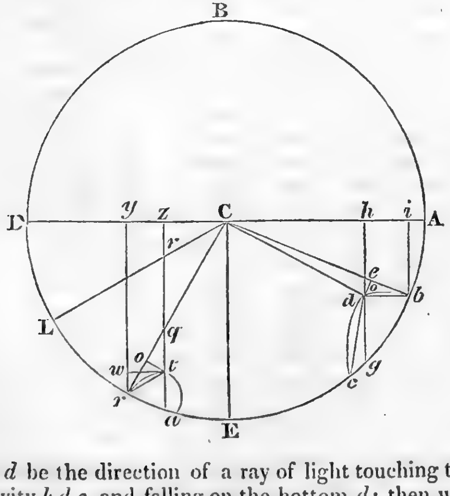
Sir,—The attention of astronomers having for years past been directed to measuring the altitudes of lunar mountains, I have frequently been surprised that no attempts have yet been made to ascertain the depths of those cavities which are so conspicuous on the surface of the moon.

Under a conviction that determining the depths of such cavities will not be considered unworthy of notice, after the first astronomers of Europe have endeavoured to ascertain the heights of mountains on the moon's surface, I beg leave to submit to the examination of your readers the inclosed method of measuring the depths of lunar cavities.

I remain, sir, yours, &c.

Islington, Sept. 26, 1818. H. M.

"Having given the apparent length of a shadow projected by the side of a lunar cavity on the bottom of it, and the angle between the edge of the cavity and the boundary of vision or moon's limb, to find the depth from the edge to the bottom of the cavity.



Let $b\,d$ be the direction of a ray of light touching the edge b of the cavity $b\,d\,c$ and falling on the bottom d; then will $b\,d$ represent the shadow projected by the side *b o d*. Let *b i* and *g h* represent lines drawn from the observer to the extremities *b* and *d* of the shadow; and ABDE the circular plane in which are found the straight lines *g h* and the point *b*. Let EC be the direction of a line joining the centres of the earth and this plane; then *g h* and *b i* may be considered parallel and in the same plane, that is the plane ABDE.

Draw the diameter AD at right angles to EC; AD then is the boundary of vision. First let the moon be in quadratures; *b d* will then be perpendicular to *g h*, that is to EC, and EC will be the boundary of illumination; therefore $bd = hi$, that is the apparent = to the true length of the shadow.

Hence, in the right-angled $\triangle\, b d e$ are given, and the $\angle\, b d e = (\angle\, \mathrm{AC}\, b$ the angle made between the edge of the cavity and the ☾'s limb) to find *b e*, the depth required.

If the moon be not in quadratures, *b d* is not perpendicular to *l i*, and consequently the apparent not equal to the true length of the shadow.

In this case let *r t* be supposed to be the direction of a ray touching the edge of the cavity *r t a*.

Let fall the perpendiculars *t z* and *r y*, join *r* C, and draw *w t* parallel to DC, and from *t* let fall *t o* perpendicular to *r q*; then will *w t* be the apparent length of the shadow, *r t* the true length, and *r o* the depth of the cavity.

In the triangle *w t r* are given the side *w t* and the angle $w\,t\,r =$ (the elongation if the moon be in her first or last quarters, or to its supplement if in the second or third quarters) to find *r t*. Then in the $\triangle r.o.t.$ the $\angle r.t.o. =$ the $\angle r t z - \angle o.t.r.$ But $\angle r.t.z = \angle l.r.z. = \angle z.\mathrm{C.B} =$ the ☾'s elongation and the ($\triangle$s $o.t.r.$ and $z.c.q.$ being similar) $\angle o.t.r. = \mathrm{D.C.}r.$ the angle between the edge of the cavity and ☾'s limb ∴ $\angle r.t.o. = \angle r t z - \angle o.t.z.$ and *r t* are given to find *r.o.* the depth required.

If (*a*) be put = the angle between the edge of the cavity and ☾'s limb, (*l*) = the apparent length of the shadow, (*e*) = the elongation, and (*d*) = the depth of the cavity;—the following formulæ are deducible:

When the moon is in quadratures $d = \dfrac{l \times \text{co-si.}\ a}{\text{radius}}$

When not in quadratures . . . . . . $d = \dfrac{\text{sine}(e \backsim a) \times l}{\text{sine}\ e}$.

The angle (*a*) between the edge of the cavity and the moon's limb is taken by placing one wire of a micrometer so as to join the cusps, and moving the other from the edge of the cavity till it becomes a tangent to the disk: the measure thus taken : radius : : ☾'s semidiameter : to the versed sine of (*a*).

The

The angle under which the shadow appears is taken in the same manner; and the moon's semidiameter : this measure : : semidiameter in miles : the length of the shadow.

As the result deduced from the above operation will always be the depth of that part of the cavity on which the extremity of the shadow falls, the difficulty of ascertaining when the shadow falls *on the bottom* may be objected to it:—this, however, may always be obviated by continuing the observations until their result becomes a maximum, which will evidently be the depth of the deepest part; or in those cavities in which prominences or bright spots appear, it may be more easily done by observing when one of these prominences (which are no doubt situated at the bottoms of their respective cavities) is in the line which forms the boundary of the shadow.

By repeated observations some idea may be formed of the interior shapes of these cavities. If from a few continued measurements the same depths are deduced, it may be concluded that the bottom is a plane surface: if they are found gradually to increase and afterwards decrease in the same proportion, that part of the interior surface will be shown either to be formed by the inclination of two planes meeting at the bottom, or to be a curve: to which of these classes it belongs may be ascertained from the nature of this increase. Any considerable irregularities in the bottom or sides will be marked by corresponding diminutions in the depth of the cavity, or length of the shadow. Whether these speculations are carried further than is sanctioned by the present state of our best instruments, remains for the determination of those who are possessed of them, and are accustomed to use them."

LIV. *Conjectures concerning the Cause, and Observations upon the Phænomena, of Earthquakes; particularly of that great Earthquake of the first of November* 1755, *which proved so fatal to the City of Lisbon, and whose Effects were felt as far as Africa, and more or less throughout almost all Europe; by the Rev.* John Michell, *M.A. Fellow of Queen's College, Cambridge.*

[Concluded from p. 270.]

Part III.—Section I.

65. In the former part of this tract, I supposed a part of the roof over some subterraneous fire to fall in: this is an event that cannot happen merely accidentally; for so long as the roof rests on the matter on fire, no part of it can fall in, unless the matter below could rise and take its place: now, it is very difficult to conceive how this should happen, unless it was to rise by

some larger passages than the ordinary fissures of the earth, which seem much too narrow for that purpose; for, besides that the melted matter cannot be supposed to have any very great degree of fluidity, it must necessarily have a hard crust formed upon it, at all the fissures, by the long continued contact of the water contained in them: these impediments seem too great to be overcome by the difference of the specific gravities of the part that is to fall in, and the melted matter, which is the only cause that can tend to make it descend; the manner therefore, in which, I suppose, this event may be brought about, is as follows:

66. The matter of which any subterraneous fire is composed, must be greatly extended* beyond its original dimensions by the heat. As this will be brought about gradually, whilst the matter spreads itself, or grows hotter, the parts over the fire will be gradually raised and bent; and this bending will, for some time, go on without any other consequence; but, as the fire continues to increase, the earth will at last begin to be raised somewhat beyond the limits of it. By this means, an annular space will be formed at the edges next to the fire, and surrounding it, a vertical section of which space, through a diameter of the fire, will be two long triangles, the shortest side or base of each lying next the fire, and the two longer sides being formed by the upper and lower strata, which will be separated for a considerable extent, proportionably to the distance through which they are raised from each other†. This space will be gradually filled with water, as it is formed, the melted matter being prevented from filling it, by its want of fluidity, as well as on account of the other circumstances, under which it is to spread itself; for the lentor and sluggishness of this kind of matter is such, that, when somewhat

* As all bodies we are acquainted with are liable to be extended by heat, there can be no doubt of its being so in this case likewise; but the matter of subterraneous fires is yet much more extended, than those bodies which are only capable of being melted into a solid glass, if we may judge of it from what we see of volcanos; for the lavas, sciari, and pumice stones, thrown out from thence, even after they are cold, are commonly of much less specific gravity, on account of their porous spongy texture, than the generality of earth, stones, &c. and they frequently are even lighter than water, which is itself lighter than any known fossil bodies, that compose strata in their natural state.

† In fig. 4. A is supposed to represent a vertical section of the matter on fire; BB, parts of the same stratum yet unkindled; CC, the two sections of the annular space, (surrounding the fire) which is supposed to be filled with water, as far as the strata are separated; D, the several sets of earth, stones, &c. lying over the fire, which are raised a little, and bent, by the expansion of the matter at A. As it is not easy to represent the things above described in their due proportions, it may not be amiss, in order to prevent the figure here given from misleading the reader, to give some random measures of the several parts, such as may probably approach towards

what cooled on the surface by the contact of the air only, it will not flow, perhaps, ten feet in a month, though in a very large body; instances of which we have in the lavas of Ætna, Vesuvius, &c. It is not to be expected then, that it should spread far, when it comes in contact with water at its edges, as soon as it is formed, and when it is, perhaps, several months in acquiring a thickness of a few inches; but it must, by degrees, form a kind of wall between the fire and the opening into the annular space before described. This wall will gradually increase in height, till it becomes too tall in proportion to its thickness, to bear any longer the pressure of the melted matter; which must necessarily happen at last, because the thickness of it will not exceed a certain limit*.

67. Besides the giving way of this wall, the fire may undermine the space containing the water, and, by that means, open a communication between them. Let us suppose one of these come to pass, and the time arrived when the partition begins to yield. If then the water had any way to escape readily, the breach would be made, and the melted matter would burst forth immediately, and flow out in large quantities at once amongst it; but as this is not the case, and it can only escape by oozing slowly between the strata, and through the fissures, the way that it came, the breach will be made gradually, from whence we may account for some appearances that have preceded great earthquakes.

towards those which are sometimes found in nature: we may suppose then the stratum B to be, perhaps, from ten or twenty to a hundred yards in thickness; the greatest height of the annular space C, next the fire, to be from four or five to ten or fifteen feet, and its greatest extent, horizontally, from ten or twenty to fifty or sixty feet; the horizontal extent of the fire at A, may be from half a mile to ten or twenty miles; [see art. 29, and the note to art. 53.] and the thickness of the superincumbent matter at D, may be from a quarter or half a mile to two or three miles; the number of the laminæ also, into which it is divided, may be many times more than those in the figure. As to the perpendicular fissures, they must be so numerous, and so small, in proportion to the other parts, that I chose rather to leave them, to be supplied by the imagination of the reader, than attempt to express them in a manner, that could give no adequate idea of them at all.

* This limit will depend upon the thickness of the matter necessary to prevent so quick a communication of the heat or cold through it, as that the water should be able to diminish the heat of the fire considerably. The thickness requisite to do this, is very different in different kinds of bodies. Metals of all kinds transmit heat and cold extremely readily; but bricks and vitrified substances (with which last we may class the matter under our present consideration) transmit them very slowly: the walls of the hottest of our furnaces, when built of bricks, and eighteen inches thick, will not transmit more heat than a living animal can bear without injury, though the fires are continued in them for ever so long a tima; probably, therefore, if we allow two feet for the thickness of the matter, cooled and rendered hard by the contact of the water, we shall not underdo it.

 68. We

68. We are told, that two or three days before an earthquake* in New England, the waters of some wells were rendered muddy, and stank intolerably: why might not this be occasioned by the waters contained in the spaces before described, which, being impregnated with sulphureous steams, were driven up, and mixed with the waters of the springs? At least, there can be no doubt, by whatsoever means it was brought about, that this phænomenon was owing to the same cause, already beginning to exert itself, which afterwards gave rise to the succeeding earthquake.

69. Something like this happened before the great Lisbon earthquake † of 1755. We are told, that at Colares, about twenty miles from thence, "in the afternoon preceding the 1st of November, the water of a fountain was greatly decreased: on the morning of the 1st of November, it ran very muddy, and after the earthquake, it returned to its usual state, both in quantity and clearness." The same author says, a little lower, "in the afternoon of the 24th, I was much apprehensive, that the following days we should have another great earthquake; for I observed the same prognostics as in the afternoon of the 31st of October; that is," &c. "And I further observed, that the water of a fountain began to be disturbed to such a degree, that in the night it ran of a yellow clay colour; and from midnight to the morning of the 25th, I felt five shocks, one of which seemed to me as violent as that of the 11th of December."

70. But the most extraordinary appearance of any that preceded this earthquake, was that of the agitation of the waters of Lochness ‡, and some others of the lochs in Scotland, about half an hour before any motion was felt at Lisbon, notwithstanding the cause of all these great effects could not lie far from thence, and, I think, certainly lay to the south of Oporto. Nor is it probable, that there should be any mistake in the time, not only because the difference is too great, as well as the concurrent testimonies too many, to admit of such a solution; but because they mention another greater agitation, that happened

* See Philos. Trans. No. 437, or Martyn's Abridgem. vol. viii. p. 689.

† See Philos. Trans. vol. xlix. p. 416 and 417; or Hist. and Philos. of Earthq. p. 313.

‡ See Philos. Trans. vol. xlix.—or Hist. and Philos. of Earthq. art. *Lochness, Lochlomond*, &c. The same thing also seems to have taken place in Switzerland; for Mons. Bertrand says, that all the agitations of the waters in the lakes there, which were observed on the 1st of November 1755, happened between nine and ten in the morning; and particularly at lake Leman, he says, the agitation happened just before ten; which, allowing for the difference of longitude, must have been just before nine at Lisbon; and, consequently, if there is no mistake in the times, all these agitations preceded the earthquake, at this last place, by near three quarters of an hour. [See *Memoires sur les Tremblemens de Terre*, p. 107 & 105.]

about

about an hour and half after the former; which latter agrees with the times when the agitations of the waters were observed in England, if we allow only a proper interval for the motion to be propagated so far northward, proportionably to the time it took up in travelling from its original source near Lisbon.

71. These appearances seem to be connected with that mentioned in the preceding article, and they may both, I think, be accounted for, by supposing a considerable quantity of vapour to be raised, whilst the partition before-mentioned was beginning to give way; during which time a partial communication between the water and fire would be brought on, and that by degrees only. Hence the vapour, not being produced at once but gradually, might creep silently between the strata*, towards that quarter where the superincumbent mass of earth was lightest; and, by this means, some places very near the source of the vapour might be little, or not at all, affected by it, whilst others might be greatly affected, though they lay at a great distance; and even those places, which lay immediately over the part where the vapour was passing, might not perceive any effect, on account of the gentleness of the motion, occasioned by the small quantity of it. This might continue to be the case, till it came to some country where, the set of strata above being much thinner, the vapour would not only be hurried forward, but collected also into a much narrower compass; and therefore, raising the earth more, would produce more sensible effects; and this we ought chiefly to expect in the most mountainous countries, according to the idea before given of them†.

72. To make this something clearer, let us suppose, in fig. 1. the vapour to be passing between the strata in the dotted line C, and to go forwards, till it arrives at A: whilst, then, it passes under the deeper parts at E, it will raise the earth over it but little, as well because it will be spread broader and thinner, as because it will be more compressed by the weight of the superincumbent matter; but as it arrives towards A, not only the lat-

* Some appearances that have been observed in New England seem to confirm this, and make it probable, that a small quantity of vapour is often found to creep silently between the strata, before a general communication between the water and the fire gives rise to the greater and more sensible effects of earthquakes. See Philos. Trans. No. 462; or Martyn's Abr. vol. viii. p. 693, where we are told, that, at Newbury, a little before any noise or shock was perceived, the bricks of an hearth were observed to rise, and, falling down again, to lean another way. In the same account, it is also said, that "a few minutes before any shock came, many people could foretell it by an alteration in their stomachs:" an effect, which seems to be of the same kind with sea-sickness, and which always accompanies the wavelike motion of earthquakes, when it is so weak, as to be uncertainly distinguishable.

† See art. 43.

ter part will be driven forwards with greater velocity, but the foremost will travel slower, on account of its travelling under a thinner set of strata*; and, besides this, the load being much less, it will greatly expand itself. From all these causes taken together, the wave at the surface of the earth, occasioned by the passing of the vapour under it, will not only be much higher, but also much shorter, and, consequently, the sides of it, on both these accounts, will be much more inclined to the horizon: and, moreover, because the progress of the wave will be slower, it will give more time to any waters situated on one side of it, to flow one way; and on this account also, the apparent agitation of them will be increased.

Section II.—73. We are told, that, in the Lisbon earthquake of 1755, "the bar [at the mouth of the Tagus] was seen dry from shore to shore; then suddenly the sea, like a mountain, came rolling in; and about Bellem castle, the water rose fifty feet almost in an instant; and, had it not been for the great bay opposite to the city, which received and spread the great flux, the low part of it must have been under water †." The same phænomena were observed to accompany the same earthquake at the island of Madeira; where we are told, that, at the city of Funchal, "the sea, which was quite calm, was observed to retire suddenly some paces; then rising with a great swell, without the least noise, and as suddenly advancing, it overflowed the shore, and entered the city. It rose full fifteen feet perpendicular above high-water mark, although the tide, which ebbs and flows there seven feet, was then at half ebb. In the northern part of the island, the inundation was more violent, the sea retiring there above one hundred paces at first, and suddenly returning, overflowed the shore, forcing open doors, breaking down the walls of several magazines and storehouses, and carrying away in its recess, a considerable quantity of grain, and some hundred pipes of wine ‡."

74. Both these appearances (which have been observed to attend several other earthquakes, as well as this) seem to admit of an easy solution, supposing the cause of them to lie under the bed of the ocean; for, in the further progress of the communication between the fire and water, the vapour, that is gradually raised at first, will at last begin to raise the roof over the fire, which being supported by so light a vapour, there will now be no want of fluidity in the matter it rests upon, and the difference of specific gravity between the two, instead of being small, will be

* See art. 63, the note.

† See Hist. and Philos. of Earthq. p. 316.

‡ See Philos. Trans. vol. xlix. p. 432, &c. or Hist. and Philos. of Earthq. p. 329.

very

very great: hence, if any part of the roof gives way, it must immediately fall in, the vapour readily rising, and taking its place; and a beginning being once made, a communication will be opened with numberless clefts and fissures, that must occasion the falling in of vast quantities of matter, which, as soon as the vapour can pass round them, will want their support; then will follow the great effects* already described.

75. Now, whilst the roof is raising, the waters of the ocean, lying over it, must retreat, and flow from thence every way; this however, being brought about slowly, they will have time to retreat so gently, as to occasion no great disturbance: but as soon as some part of the roof falls in, the cold water contained in the fissures of it, mixing with the steam, will immediately produce a vacuum, in the same manner as the water injected into the cylinder of a steam-engine, and the earth subsiding, and leaving a hollow place above, the waters will flow every way towards it, and cause a retreat of the sea on all the shores round about: then presently, the waters being again converted by the contact of the fire into vapour, together with all the additional quantity, which has now an open communication with it, the earth will be raised, and the waters over it will be made to flow every way, and produce a great wave immediately succeeding the previous retreat†.

SECTION III.—76. That great quantity of water, which we have supposed to be let out upon subterraneous fires, and, by that means, to produce earthquakes, will supply us with a reason, why they observe a sort of periodical return. This water must extinguish a great portion of the burning matter, in consequence of which, it will be contracted within much narrower bounds; and though the effects before described could not take place at

* See art. 56 to 60 inclusive.

† It may, perhaps, be objected, that these phænomena may as easily be occasioned by a vapour generated under the dry land, which, by first raising the earth upon the sea-shore, would make the waters retreat; and that the return of them again, upon its subsiding into its place, might cause the subsequent wave. That this may be the case, in some instances, is not impossible, but, I believe, upon examining the particular circumstances, it will generally be found to be otherwise; and there cannot be any doubt about it, in the case of the Lisbon earthquake; for the retreat was observed to precede the wave, not only on the coast of Portugal, but also at the island of Madeira, and several other places: now, if the retreat had been caused by the raising of the earth on the coast of Portugal, the motion of the waters occasioned by this means, when propagated to Madeira, must have produced a wave there previous to the retreat, contrary to what happened; nor could the motion of the waters at Madeira be caused by the earthquake at that place, because it did not happen till above two hours after; whence it is manifest, that it must have been owing to the continuation of a motion propagated from the place, where the earthquake exerted its first efforts. And we may observe, in general, that this must always be the case, whenever the retreat does not happen till some considerable time after the earthquake.

first,

first, but by the great extension of the heated matter, yet, after they have once taken place, they may well continue to do so for some time; for the great disturbance in the first instance, by the falling in of a great part of the roof, must render the frequent communication between the fire and water not only very easy, but almost unavoidable: and this will continue to be so, till the roof is well settled, and the surface of the melted matter sufficiently cooled, after which, it may require a long time for the fire to heat it again so much, as will be necessary to make it produce the former effects. Now, as the matter has been more or less cooled, or as the combustible materials are with more or less difficulty set on fire again, as well as on account of other circumstances, the returns of these effects will be later or earlier; but though they will not, for this reason, observe any exact period, yet they will generally fall within some sort of limits, till either the matter that occasions them is consumed, (which, probably, will seldom happen in less than many ages,) or till the fires open themselves a passage, and become volcanos.

Section IV.—77. I have already intimated, that the most extensive earthquakes frequently take their rise from the sea. According to the description of the structure * of the earth before given, any combustible stratum must lie at greater depths in places under the ocean, than elsewhere; hence far more extensive fires may subsist there, than where the quantity of matter over them is less; for any vapour raised from such fires, having both a stronger roof over it, and being pressed by a greater weight, (beside the additional weight of the water) will not only be less at liberty to expand itself, and consequently of less bulk, but it will also be easily driven away towards the parts round about, where the superincumbent matter is less, and therefore lighter. On the other hand, any vapour raised from fires, where the superincumbent matter is lighter, finding a weaker roof over it, and being not so easily driven away under strata, that are thicker and heavier, will be very apt to break through, and open a mouth to a volcano; and it must necessarily do this long before the fires can have spread themselves sufficiently, to be near equal to those which may subsist in places that lie deeper. All this seems to be greatly confirmed by the situation of volcanos, which are almost always found on the tops of mountains †, and those often some of the highest in the world.

78. If,

* See art. 43.

† Perhaps this may supply us with a hint (if the conjecture is not thought extravagant) concerning the manner in which these mountains have been raised, and why the strata lie generally more inclining from the mountainous countries, than those countries themselves; an appearance not easily to be accounted for, but upon the supposition, that the upper parts of the earth rest

78. If, then, the largest fires are to be supposed to subsist under the ocean, it is no wonder that the most extensive earthquakes should take their rise from thence: the great earthquake of Lisbon has been shown to have done so*; and that the cause of it was also at a greater depth, than that of many others, appears from the greater velocity with which it was propagated †.

79. The great earthquake that destroyed Lima and Callao in 1746, seems also to have come from the sea; for several of the ports upon the coast were overwhelmed by a great wave, which did not arrive till four or five minutes after the earthquake began, and which was preceded by a retreat of the waters ‡, as well as that at Lisbon. Against this, it may, perhaps, be alleged, that there were four volcanos broke out suddenly§, in the neighbouring mountains, when this earthquake happened, and that the fires of these might be the occasion of it. This however, I think, is not very probable; for, to omit the argument of the wave, and previous retreat of the waters, already mentioned, it is not very likely, that more than one fire was concerned: besides, the vapour, opening itself a passage at these places, could not well be supposed, if it took its rise from thence, to spread itself far; especially towards the sea, where it is manifest, that the strata over it were of great thickness, as appears from the great velocity with which the earthquake was propagated there: the shocks also continued with equal, or nearly equal violence, for some months after the openings were made; whereas, if these fires had been the cause of them, they must immediately have ceased, upon the fires finding a vent, as it has happened in other cases ||. It is therefore much more probable, that a very large quantity of vapour, taking its rise from some far more extensive fire under the sea, spread itself from thence; and as it passed in places, where the roof over it was naturally much thinner, as well as greatly weakened by the undermining of these fires, it opened itself a passage, and burst forth.

rest upon matter, in some degree, though not perfectly fluid, and that this matter is lighter than the earth that rests upon it. This conjecture, however, will probably be thought less strange, if it be considered, that the new islands, formed about Santerini and the Azores, have some of them been raised from 200 to 300 yards, and upwards; a height which might well enough intitle them to the denomination of mountains, if they had been raised from lands not lying under the ocean. [See fig. 3.]

* See art. 54. See also art. 94 to 97 inclusive.

† See the note to art. 63.

‡ Both the wave and previous retreat have been observed in the other great earthquakes, which have happened at Lima, and in the neighbouring country. See d'Ulloa's Voyage to Peru, part ii. book i. chap. 7.

§ If these volcanos were not new ones, but only old ones which broke out afresh, [see the note to art. 34.] the argument will come with still greater force.

|| See art. 28.

80. As

80. As the most extensive earthquakes generally proceed from the lowest countries, but especially from the sea, so those of a smaller extent are generally found amongst the mountains: hence it almost always happens, that earthquakes, which are felt near the sea, if at all violent, are felt also in the higher lands; whereas there are many amongst the hills, and those very violent ones, which never extend themselves to the lower countries. Thus we are told, that, at Jamaica, "shakes* often happen in the country, not felt at Port-Royal; and sometimes are felt by those that live in and at the foot of the mountains, and by no body else." On the other hand, the earthquake that destroyed Port-Royal extended itself all over the island: and the same was observed of a smaller earthquake, that happened there in 1687–8; which latter undoubtedly came from the sea, as appears by Sir Hans Sloane's account of it †.

81. Earthquakes of small extent are also very common amongst the mountains of Peru and Chili. Antonio d'Ulloa says, "Whilst we were preparing for our departure from the mountain Chichi-Choco, there was an earthquake which was felt four leagues round about: our field tent was tossed to and fro by it, and the earth had a motion like that of waves; this earthquake, however, was one of the smallest, that commonly happen in that country." The same author tells us, in another place, that, "during his stay at the city of Quito, or in the neighbourhood of it, there were two earthquakes, violent enough to overturn some houses in the country, which buried several persons under their ruins."

Section V.—82. It is generally found, that earthquakes in hilly countries, are much more violent than those which happen elsewhere; and this is observed to be the case, as well when they take their rise from the lower countries, as amongst the hills themselves. This appearance being so easily to be accounted for, from the structure of the earth already described, I shall content myself with establishing the certainty of a fact, which tends so greatly to confirm it.

83. The earthquakes that have infested some of the towns in the neighbourhood of Quito, have not only been incomparably more violent than that which destroyed Lisbon, but they seem to have exceeded that also which destroyed Lima and Callao. In Lisbon,

* This is taken from an account of the earthquake that happened at Jamaica in the year 1692, which, as well as some others before mentioned, was attended with the wave and previous retreat. See Philos. Trans. No. 209, or Lowthorp's Abr. vol. ii. p. 417 and 418.

† See Phil. Trans. No. 209, or Lowthorp's Abr. vol. ii. p. 410.

Lisbon*, many of the houses were left standing, although few of them were less than four or five stories high. At Lima also, it is only said, that "all the buildings, great and small, or at least the greatest part of them, were destroyed." Callao likewise, as it appears from the accounts we have of it, had many houses left unhurt by the earthquake, till the wave came, which overwhelmed the whole town, and threw down every thing that lay in its way. All these effects seem to be greatly short of those produced by an earthquake that happened at Latacunga, in the year 1698, when the whole town, consisting of more than six hundred houses, was entirely destroyed in less than three minutes time, a part of one only escaping; notwithstanding that the houses there are never built more than one story high, in order, if possible, to avoid these dangers. Ambato, a village about the same size as Latacunga, together with a great part of Riobamba, another town in the same neighbourhood, were also entirely destroyed by the same earthquake, and some others were either destroyed, or received considerable damage from it. At the same time, a volcano burst out suddenly in the neighbouring mountain of Carguayraso, as before-mentioned; and, "near Ambato, the earth opened itself in several places, and there yet remains, to the south of that town, a cleft of four or five feet broad, and about a league in length, lying north and south; there are also several other like clefts on the other side of the river." The city of Quito † was affected at the same time, but received no damage, though it is no more than forty-two geographical miles from Latacunga, not far from whence the greatest violence of the shock seems to have exerted itself. These towns are supposed to stand by far the highest of any in the world, being as high above the level of the sea, as the tops of some of the highest mountains in Europe; and the ground upon which Riobamba stands, wants but ninety yards ‡ of being three times as high as Snowdon, the highest mountain in Wales.

84. The country upon which these towns stand, serves as a base, from whence arise another set of high lands and mountains, which are much the highest in the known world. Amongst these mountains there are no less than six volcanos, if not more, within an extent of 120 miles long, and less than thirty broad, the

* See Phil. Trans. vol. xlix. p. 403, where it is said, "of the dwelling-houses, there might be about one-fourth of them that tumbled."

† The city of Quito stands lower than the level of Riobamba, by about 500 yards perpendicular. Though it escaped this, it has lately, however, been destroyed by another violent earthquake, that happened on the 28th April 1756, of which I have not yet seen any other particulars worth notice.

‡ This is according to Antonio d'Ulloa's account; but Mons. Condamine makes it exactly three times the height of Snowdon, computing it at 1770 toises. [See his measure of a degree of the meridian.]

lowest

lowest of which exceeds the height of Riobamba by above two-thirds of a mile, and the highest by more than twice that quantity. Now, as the earthquakes have been more violent at the foot of these mountains, than in the lower lands, so they have been still more violent towards the tops of them: this is sufficiently manifest, from the many rents made in them and the rocks *, that have been broken off from them, upon such occasions: but it appears still more manifestly, and beyond all dispute, in the bursting forth of volcanos, which are almost always at the very summit of the mountains †, where they are found. In these instances, the earth, stones, &c. which lay over the fire, are generally scattered by the violence of the vapour, that breaks its way through, to the distance of some miles round about.

85. The great earthquake of the 1st of November 1755, was also more violent amongst the mountains, than at the city of Lisbon. We are told, that " the mountains of Arrabida, Estrella, Julio, Marvan, and Cintra, being some of the largest in Portugal, were impetuously shaken, as it were, from their very foundations; and most of them opened at their summits, split and rent in a wonderful manner, and huge masses of them were thrown down into the subjacent valleys ‡."

86. The same was observed at Jamaica likewise. In the earthquake that destroyed Port-Royal in 1692,we are told, that " more houses were left standing at that town than in all the island besides. It was so violent in other places, that people were violently thrown down on the ground, where they lay with their legs and arms spread out, to prevent being tumbled about by the incredible motion of the earth. It scarce left a planter's house or sugar-work standing all over the island: I think it left not a house standing at Passage Fort, and but one in all Liganee, and none in St. Iago, except a few low houses, built by the wary Spaniards. In Clarendon precinct, the earth gaped, and spouted up, with a prodigious force, great quantities of water into the air, twelve miles from the sea; and all over the island, there were abundance of openings of the earth, many thousands. But in the mountains, are said to be the most violent shakes of all; and it is a generally received opinion, that the nearer to the mountains, the greater the shake; and that the cause thereof, whatever it is, lies there. Indeed they are strangely torn and rent, especially the blue, and other highest mountains, which seem to be the greatest sufferers, and which, during the time that the

* See d'Ulloa's Voyage to Peru, part i. book vi. chap. 2.

† The only exceptions that I know of to this rule, are in those cases, where the highest part having an opening already, some fresh mouth opens itself in the side of the mountain.

‡ See Hist. and Philos. of Earthq. p. 317.

great

great shakes continued, bellowed out prodigious loud noises and echoings.

87. "Not far from Yallowes, a mountain, after having made several moves, overwhelmed a whole family, and a great part of a plantation, lying a mile off; and a large high mountain near Portmorant, near a day's journey over, is said to be quite swallowed up.

88. "In the blue mountains, from whence came those dreadful roarings, may reasonably be supposed to be many strange alterations of the like nature; but those wild desert places being very rarely, or never visited by any body, we are yet ignorant of what happened there; but whereas they used to afford a fine green prospect, now one half part of them, at least, seem to be wholly deprived of their natural verdure*."

SECTION VI.—89. I have supposed, that fires lying at the greatest depths generally produce the most extensive earthquakes. We must, however, except from this rule those cases where the depths are very great: for, as the weight of three miles perpendicular of common earth is capable of absolutely repressing the vapour of inflamed gunpowder, so we may well suppose, that there may be a quantity of earth sufficient to repress the vapour of water, and keep it within its original limits, though ever so much heated. Now, whenever this is the case, it is manifest, that it can produce no effect: or, it may happen, that though the quantity of earth may not be sufficient absolutely to repress the vapour, yet it may be so great, as to suffer it to expand but very little: in this case, an earthquake arising from it would be but of small extent; the wave-like motion would be little or none; the vibratory motion would be felt every where; and the propagation of the motion would be very quick. This last circumstance being almost the only one, by which these earthquakes can be known from those which owe their origin to shallower fires, it must be very difficult to distinguish them with certainty, as it is almost impossible to distinguish the difference of the time of their happening in different places, when the whole, perhaps, is com-

* See Philos. Trans. No. 209; or Lowthorp's Abridg. vol. ii. p. 416, &c. where there is a great deal more to the same purpose. See also Hist. and Philos. of Earthq. p. 286 and 287.

From the authorities quoted in this section, it appears, how little reason there is for the notion, that either large cities, or towns situated near the sea-coast, are more subject to violent earthquakes than others: it is not, however, much to be wondered at, that such a notion should have prevailed, after the great destruction that happened in so large and populous a city as Lisbon; since the demolition of a few ruinous houses only, in such a place, would have affected the imaginations of men more, and would have been more talked of, than the subversion of whole mountains in some wild and desert country, where at most half a dozen unknown shepherds might feel the effects of it, or perhaps only see it at a distance.

prehended

prehended within the space of two or three minutes; possibly, however, some of the earthquakes, which we have had in England, may have been of this class.

SECTION VII.—90. If we would inquire into the place of the origin of any particular earthquake, we have the following grounds to go upon.

91. *First*, The different directions, in which it arrives at several distant places: if lines be drawn in these directions, the place of their common intersection must be nearly the place sought: but this is liable to great difficulties; for there must necessarily be great uncertainty in observations, which cannot, at best, be made with any great precision, and which are generally made by minds too little at ease to be nice observers of what passes; moreover, the directions themselves may be somewhat varied, by the inequalities in the weight of the superincumbent matter, under which the vapour passes, as well as by other causes.

92. *Secondly*, We may form some judgement concerning the place of the origin of a particular earthquake, from the time of its arrival at different places; but this also is liable to great difficulties. In both these methods, however, we may come to a much greater degree of exactness, by taking a medium amongst a variety of accounts, as they are related by different observers. But,

93. *Thirdly*, We may come to the greatest degree of exactness in those cases, where earthquakes have their source from under the ocean; for, in these instances, the proportional distance of different places from that source may be very nearly ascertained, by the interval between the earthquake and the succeeding wave: and this is the more to be depended on, as people are much less likely to be mistaken in determining the time between two events, which follow one another at a small interval, than in observing the precise time of the happening of some single event.

94. Let us now, by way of example, endeavour to inquire into the situation of the cause, that gave rise to the earthquake of the 1st of November 1755, the place of which seems to have been under the ocean, somewhere between the latitudes of Lisbon and Oporto, (though probably somewhat nearer to the former) and at the distance, perhaps, of ten or fifteen leagues from the coast. For,

95. *First*, The direction, in which the earthquake arrived at Lisbon, was from the north-west; at Madeira it came from the north-east; and in England it came from the south-west; all of which perfectly agree with the place assumed*.

96. *Se-*

* All these directions together with the times when the earthquake, as well as the succeeding wave, arrived at different places, (two or three only excepted)

96. *Secondly,* The times in which the earthquake arrived at different places, agree perfectly well also with the same point. And,

97. *Thirdly,* The interval between these, and the time of the arrival of the subsequent wave, concur in confirming it. That all this might appear the better, I have subjoined the following table, assuming the point, from whence I compute, at the distance of about a degree of a great circle from Lisbon, and a degree and half from Oporto. In consequence of this supposition, I have added three minutes to the interval between the time when the shock was felt at Lisbon, and at the several other places. The first column in the table contains the names of places; the second, the distances from the assumed point, reckoned in half degrees; the third, the time that the earthquake took up in travelling to each, expressed in minutes; and the fourth contains the time in which the wave was propagated, from its source to the respective places, expressed in minutes likewise.

	Half deg.	Min.	Min.
Lisbon*	2	3	12
Oporto*	3	5	
Ayamonte	6		53
Cadiz	9	12	82
Madrid	9	11	
Gibraltar	11	18	
Madeira	19	25	152
Mountsbay	20		267
Plymouth	21		360
Portsmouth	23	29	
Kingsale	23		290
Swansea	24		530
The Hague	30	32	
Lochness	33	66	
Antigua	98		565
Barbadoes	101		485

98. In

excepted) are taken from the 49th volume of the Philos. Trans. and the Hist. and Philos. of Earthq. To these I must refer the reader for the particular authorities, which, as they are very numerous, I was not willing to quote at length.

* It appears, by all the accounts, that the interval between the earthquake and wave, either at Oporto or Lisbon, was not long: I have met with no account yet, however, which tells us how long it was at the former, and only one which mentions it at the latter, where it is said to have been nine minutes. [See *Memoires sur les Tremblemens de Terre*, p. 245, compared with Hist. of Earthquakes, p. 315.] These intervals, if we knew them exactly,

98. In computing the times in the preceding table, allowance was made for the difference of longitude, as it is laid down in the common maps, which are not always greatly to be depended on. The times themselves also are often so carelessly observed, as well as vaguely related, that they are many of them subject to considerable errors; the concurrent testimonies, however, are so many, that there can be no doubt about the main point; and, that the errors might be as small as possible, I have not only endeavoured to select those accounts that had the greatest appearance of accuracy, but, in all cases where it was to be had, I have always taken a mean amongst them. In many of the accounts, the relaters say only between such hours, or about such an hour: of this kind were the accounts of the times of the agitation of the waters at the Hague and Lochness, which vary the most from a medium of the rest, the former erring about seven minutes in defect, and the latter about twenty minutes in excess: with regard to the latter, however, I must observe, that, from the account itself, it is probable the agitation happened sooner than eleven o'clock, which is the time mentioned. The accounts also of the time of the agitation of the waters in the northern parts of England, seem to confirm the same thing*.

99. It is observable, in the preceding table, that the times, which the wave took up in travelling, are not in the same proportion with the distances of the respective places from the supposed source of the motion: this, however, is no objection against the point assumed, since it is manifest, wherever it was, that it could not be far from Lisbon, as well because the wave arrived there so very soon after the earthquake, as because it was so great, rising, as we are told, at the distance of three miles from Lisbon, to the height of fifty or sixty feet. The true reason of this disproportion seems to be the difference in the depth of the water; for, in every instance in the above table, the time will be found to be proportionably shorter or longer, as the water through

actly, might have served, perhaps, to ascertain the distance of those two places from the original source a little more accurately; but, as the distance of neither from thence could be very great, a small difference in them would hardly sensibly affect any of the others; from which, therefore, we may draw the same general conclusions, as if they were exact.

* As the shortest way that the vapour could pass from near Lisbon to Lochness was under the ocean, possibly it might, on that account, be somewhat retarded; for the water adding to the weight of the superincumbent mass, and not to its elasticity, must produce this effect in some degree: it is probable, however, that this could make no great difference, as the motion seems to have been very little retarded in its passage from the original source to Madeira, to which place, I suppose, it must have passed under deeper seas than would be found in its road to Scotland.

which

which the wave passed was deeper or shallower*. Thus the motion of the wave to Kingsale or Mountsbay (through waters not deeper in general than 200 fathoms) was slower than that to Madeira, (where the waters are much deeper,) in the proportion of about three to five; and it was slower than that to Barbadoes, (where its course lay through the deepest part of the Atlantic ocean) nearly in the proportion of one to three: so likewise the motion of it from the Scilly islands to Swansea in Wales (where the depth gradually diminishes from about sixty or seventy fathoms to a very small matter) was still slower than that to Kingsale, in the proportion of less than one to three: the same thing is observable with regard to Plymouth also, where the wave arrived about ninety minutes later than at Mountsbay, though the difference of their distance from the first source could not, upon any supposition, be more than forty or fifty miles.

SECTION VIII.—100. If we would inquire into the depth, at which the cause lies, that occasions any particular earthquake, I know of no method of determining it, which does not require observations not yet to be had; but if such could he procured, and they were made with sufficient accuracy, I think some kind of guess might be formed concerning it: for,

101. *First*, In those instances, where the vapour discharges itself at the mouths of volcanos, (as in the case of the earthquake at Lima,) it might, perhaps, be possible for a careful observer to trace the thickness of the several strata† from thence to the place where the earthquake took its rise, or at least as far as the shore, if it took its rise from under the sea. If this could be once done in any one instance, and the velocity of such an earthquake nicely determined, we might then guess at the depth of the cause in other earthquakes, where we knew their velocity, by taking the depths‡ proportional to those velocities, which probably would answer very nearly.

102. *Secondly*, If, in any instance, it should be possible to know how much the motion of any earthquake was retarded by passing under the ocean, the depth of the ocean being known, the depth at which the vapour passed would be known also; for the velocity under the water would be to the velocity, if there had been no water, in the subduplicate ratio of the weight in

* We have an instance to this purpose in the tides, which, in deep waters, move with a velocity that would carry them round the whole earth in a single day; but as they get into shallower waters, they are greatly retarded: and we are told, that in the river of Amazons, the same tide is found running up to the tenth or twelfth day, before it is entirely spent. [See Condamine's Voyage down the Maranon.]

† This is upon the supposition, that the under strata, in ascending up the hills, come to the day in the manner before described. See art. 43, and *fig.* 3.

‡ See the note to art. 63.

the

the latter case to the weight in the former: hence allowing earth to be about two and half times the weight of water, the depth will be readily found.

103. *Thirdly*, Let us conceive the earth to be formed according to the idea before given of it, and that the same strata are at a medium of the same thickness for a very great extent, as well in those places, where several of the upper ones are wanting, as where they are not. Upon this supposition, we may discover the depth, at which the vapour passes, by comparing the several velocities of the same earthquake in places where the thicknesses* of the superincumbent mass are different. It must be acknowledged, indeed, that such observations with regard to time, as would enable us to determine these velocities, are in general much too nice to be expected: the matter, however, is not altogether desperate, as we may collect them, in some measure perhaps, from other circumstances; such, for instance, as the degree of agitation in different waters †, the proportional suddenness‡ with which the earth is lifted in different places, &c.

104. As the observations relating to the earthquake of the 1st of November 1755 are too gross, it would be in vain to attempt, by any of the foregoing methods, to determine with any certainty the depth at which the cause of it lay; but, if I might be allowed to form a random guess about it, I should suppose, (upon a comparison of all circumstances,) that it could not be much less than a mile, or a mile and half, and I think it is probable, it did not exceed three miles.

Conclusion.—105. Thus have I endeavoured to show how the principal phænomena of earthquakes may be produced, by a cause with which none, that I have seen, appear to me to be incompatible. As I have not knowingly misrepresented any fact, so neither have I designedly omitted any that appeared to affect the main question; but, that I might not unnecessarily swell what had already much exceeded the limits at first intended for it, I have omitted,

106. *First*, Those minuter appearances, which almost every reader would easily account for, from what has been said already, and which did not seem to lead to any thing further: such, for

* In order to know this difference, it will be necessary to trace the thickness of those strata, which are found in some of the places, but are wanting in others. † See art. 71 and 72.

‡ This may be known from the distance to which the mercury subsides in the barometer, upon the first raising of the earth by the vapour. I don't find that this phænomenon, which is a common attendant on earthquakes, was observed any-where, at the time of the earthquake of the 1st of November 1755, except at Amsterdam, where the mercury subsided more than an inch. See Hist. and Philos. of Earthq. p. 309.

instance,

instance, are the sudden stopping and gushing out of fountains, occasioned by the opening or contracting of fissures; the dizziness and sickness people feel, from the almost imperceptible wave-like motion, &c.

107. *Secondly,* Those appearances which seemed to depend upon particular circumstances, and of which therefore, unless we had a more exact knowledge of the countries where they happened, it would have been impossible to give any account, without having recourse to uncertain conjectures; of this kind was the greater agitation of the waters in the lakes of Switzerland, at the time of the earthquake of the 1st of November 1755, than during the earthquake of the 9th of December following*, though the houses upon the borders of them were more violently shaken by the latter. And,

108. *Lastly,* Those appearances, which only seem to have an accidental connection with earthquakes, or the causes of them; of this kind, are the effects which, in some instances perhaps, they produce on the weather; the distempers which are sometimes said to succeed them; the disturbance which, we are told, they have sometimes occasioned, during the shocks, in the direction of the magnetic needle, &c. none of which are observed to be constant attendants on earthquakes, nor do they seem materially to affect the solution given either one way or other.

LV. *Account of certain Improvements in Involution and Evolution.* By Mr. PETER NICHOLSON.

INVOLUTION.

I HAVE not observed in any of our treatises on Algebra, any general form for the expansion of $(a+b+c+d+e+ \&c.)^n$, except that which would result from the theorem of Demoivre: but as this is capable of a more simple form, which does not involve the combinations of the quantities, and on which the extraction of roots in numbers entirely depends, I shall here exhibit it thus:

$$(a+b+c+d+e+\&c.)^n = a^n$$

$$(2), + \left\{ na^{n-1}b + \frac{n.n-1}{1.2}a^{n-2}b^2 + \frac{n.n-1.n-2}{1.2.3}a^{n-3}b^3 + \frac{n.n.-1.n-2.n-3}{1.2.3.4}a^{n-4}b^4 + \&c. \right\}$$

$$(3), + \left\{ n(a+b)^{n-1}c + \frac{n.n-1}{1.2}(a+b)^{n-2}c^2 + \frac{n.n-1.n-2}{1.2.3}(a+b)^{n-3}c^3 + \&c. \right\}$$

* See Monsieur Bertrand's *Memoires sur les Tremblemens de Terre.*

(4),

$(4), + \left\{ n(a+b+c)^{n-1}d + \frac{n.n-1}{1.2}(a+b+c)^{n-2}d^2 + \frac{n.n-1.n-2}{1.2.3} (a+b+c)^{n-3}d^3 + \&c. \right\}$

$(5), + \left\{ n(a+b+c+d)^{n-1}e + \frac{n.n-1}{1.2}(a+b+c+d)^{n-2}e^2 + \frac{n.n-1.n-2}{1.2.3}(a+b+c+d)^{n-3}e^3 + \&c. \right\}$

$(6), + \&c.$

Demonstration.

Calling each series inclosed within each two braces a term, a^n being the first term :—I observe that the first and second terms are equal to the expansion of $(a+b)^n$; that the third term is the expansion of $[(a+b)+c]^n$ considered as a binomial wanting the first term $(a+b)^n$; that the fourth term is the expansion of $[(a+b+c)+d]^n$ wanting the first term $(a+b+c)^n$, and so on : therefore the whole expanded series is equal to

$$(a+b)^n + \left\{ (a+b+c)^n - (a+b)^n \right\} + \left\{ (a+b+c+d)^n - (a+b+c)^n \right\} + \left\{ (a+b+c+d+e)^n - (a+b+c+d)^n \right\} + \&c. = (a+b+c+d+e+\&c.)^n$$

And thus we have another general rule for raising any number to the nth power, besides that of multiplying the number continually by itself $(n-1)$ times.

I shall here present the reader with a numerical example or two in involution, in order to explain the nature of evolution : for this purpose we have

$$(37658)^n = (30000+7000+600+50+8)^n$$

therefore $(37658)^n = (30000)^n$ &c.

$$+ \left\{ n(30000)^{n-1}(7000) + \frac{n.n-1}{1.2}(30000)^{n-2}(7000)^2 + \&c. \right\}$$

$$+ \left\{ n(37000)^{n-1}(600) + \frac{n.n-1}{1.2}(37000)^{n-2}(600)^2 + \&c. \right\}$$

$$+ \left\{ n(37600)^{n-1}(50) + \frac{n.n-1}{1.2}(37600)^{n-2}(50)^2 + \&c. \right\}$$

$$+ \left\{ n(37650)^{n-1}(8) + \frac{n.n-1}{1.2}(37650)^{n-2}(8)^2 + \&c. \right\}$$

Or universally thus :

Since any scale of numbers may be generally represented by $ax^m + bx^{m-1} + cx^{m-2} + \ldots + kx + l$ we shall have

$$(ax^m + bx^{m-1} + cx^{m-2} + \ldots\ldots + kx + l)^n = (ax^m)^n$$

$$+ \left\{ n(ax^m)^{n-1}(bx^{m-1}) + \frac{n.n-1}{1.2}(ax^m)^{n-1}(bx^{m-1})^2 + \&c. \right\}$$

+

$+ \left\{ n\left[(ax^m) + (bx^{m-1})\right]^{n-1}(cx^{m-2}) + \frac{n.n-1}{1.2}\left[(ax^m) + (bx^{m-1})\right]^{n-2}(cx^{m-2})^2 + \&c. \right\}$ + &c. for the expansion of any power of a number, according to any scale of notation.

But perhaps it will be more useful to descend to some particular instances by which the method and principle may be seen to advantage. Therefore let $a+b+c+d+e+$&c. be raised to the cube; then will $(a+b+c+d+e)^3 = a^3 + \left\{3a^2b+3ab^2+b^3\right\}$

$$+ \left\{3(a+b)^2c+3(a+b)c^2+c^3\right\} + \left\{3(a+b+c)^2d+3(a+b+c)d^2+d^3\right\}$$

$$+ \left\{3(a+b+c+d)^2e+3(a+b+c+d)e^2+e^3.\right.$$

Or, because that in any term the members constituting that term have a common factor, the same may be exhibited thus:

$$(a+b+c+d+e)^3 = a^3 + b\left\{3a^2+3ab+b^2\right\} + c\left\{3(a+b)^2 + 3(a+b)c+c^2\right\}$$

$$+d\left\{3(a+b+c)^2+3(a+b+c)d+d^2\right\} + e(3(a+b+c+d)^2 + 3(a+b+c+d)\,e+e^2\}$$

Let it now be required to find the cube of the number 5436. Here $a=5000$, $a+b=5400$, $a+b+c=5430$, and $a+b+c+d=5436$; consequently $b=400$, $c=30$, and $d=6$.

Operation by the method here demonstrated.

	$a^3=$	125000000000
1st,	$3a^2b=$	30000000000
	$3ab^2=$	2400000000
	$..b^3=$	64000000
2d,	$3(a+b)^2c=$	2624400000
	$3(a+b)c^2=$	14580000
	$......c^3=$	27000
3d,	$3(a+b+c)^2d=$	530728200
	$3(a+b+c)d^2=$	586440
	$........d^3=$	216
		160634321856

The number of figures in this operation may be considerably lessened by considering that the first period would be the same as if it consisted only of $54=50+4$; and the second period as if it consisted of $543=540+3$; and the third period as if it consisted of $5436=5430+6$. Therefore the operation in this form is

 $a^3=$

$$
\begin{array}{rr}
a^3 = & 125 \\
\left\{\begin{array}{r} 3a^2b= \\ 3ab^2= \\ ..b^3= \end{array}\right. & \begin{array}{r} 30000 \\ 2400 \\ 64 \end{array} \\
\left\{\begin{array}{r} 3(a+b)^2c= \\ 3(a+b)c^2= \\ ..\quad ..c^3= \end{array}\right. & \begin{array}{r} 2624400 \\ 14580 \\ 27 \end{array} \\
\left\{\begin{array}{r} 3(a+b+c)^2d= \\ 3(a+b\div c)d^2= \\ ..\quad ..\ d^3= \end{array}\right. & \begin{array}{r} 530728200 \\ 586440 \\ 216 \end{array} \\
\hline
 & 160634321856
\end{array}
$$

The foregoing operations now exhibited, require several minor operations which are omitted. However, these operations will not be necessary in the following form, where it may be considered that in the first period $a=50$ and $b=4$; in the second period $(a+b)=540$ and $c=3$; and in the third period $(a+b+c)=5430$ and $d=6$. Here follows the operation:

$$
1st,\left\{\begin{array}{rr} 3a^2 = & 7500 \\ 3ab= & 600 \\ .b^2= & 16 \end{array}\right.
$$
$$
\overline{8116} = \acute{D}
$$

$$
2d,\left\{\begin{array}{rr} 3(a+b)^2 = & 874800 \\ 3(a+b)c= & 4860 \\ ....\ c^2= & 9 \end{array}\right.
$$
$$
\overline{879669} = D''
$$

$$
3d,\left\{\begin{array}{rr} 3(a+b+c)^2 = & 88454700 \\ 3(a+b+c)d= & 97740 \\ ....\ d^2 = & 36 \end{array}\right.
$$
$$
\overline{88552476} = D'''
$$

$$
\begin{array}{ll}
a^3 & = 125\ldots \\
4\times D' & = 32464\ldots \\
3\times D'' & = 2639007\ldots \\
6\times D''' & = 531314856 \\
\hline
 & 160634321856 = (5436)^3
\end{array}
$$

It is evident that, $3(a+b+c+\&c.)^2 = 3(a+b+\&c.)^2 + 2\cdot 3(a+b+\&c.)c + 3c^2$ for the square of $a+b+c$ &c., consists of the squares of each quantity, and twice the sum of the products of every two; this square being multiplied by 3, will give three times the square of each letter, and six times the product of every two. Again: the square of $a+b+$&c. with one letter less, contains the squares of all the letters, except the one omitted; and twice their products, except those of the letter omitted: therefore when multiplied by 3, will contain the square of each letter three times,

times, except the one omitted, and six times the product of every two, except with the one omitted. Again: six times $(a+b+\&c.)c$ contains six times the sum of the products of each letter, with the last letter which was before omitted; and three times the square of the last letter makes the second side equal to the first. Thus suppose only three letters concerned, then $3(a+b+c)^2=3a^2+3b^2+3c^2+6(ab+ac+bc)=3(a+b)^2+2\cdot3(a+b)c+3c^2=3(a^2+2ab+b^2)+6ac+6bc+3c^2=3a^2+3b^2+3c^2+6(ab+ac+bc)$ as before.

Now as the first period consists of three times the square of the first left-hand figure of the number to be squared with a cipher annexed placed in the first row, the product of the left-hand figure and annexed cipher, into the second figure of the number to be squared placed in the second row, and the square of the second figure placed in the third row:

And in general as the nth period consists of three times the square of the first or left-hand n figures to be squared with a cipher annexed placed in the first row; the product of the n figures with the cipher annexed, into the $(n+1)$th figure of the number to be squared placed in the second row, and the square of the said $(n+1)$th figure placed in the third row:

Therefore in the $(n+1)$th period, instead of taking three times the square of the number consisting of $n+1$ of the first figures of the number to be squared with a cipher annexed, for the number to be placed in the first row; we shall find this first row of the $(n+1)$th period, by adding each figure in the lowest or third row of the nth period, three times each figure in the second row of the nth period twice, and each figure in the first row of the nth period once.

The third or last row is only a mental operation; the middle row is easily found by multiplying first by the digit 3, and the new figure.

It is not meant that the method now shown for cubing a number should supersede the common method; but the principal use is to explain the reverse operation of extracting the cube root in numbers. It may also serve as a method of proof to that commonly used for raising powers. For it is of some advantage to have two different methods of performing every arithmetical operation, that the one may furnish a check to the other.

EVOLUTION.

Problem.

To find a number of which its cube shall be the nearest less number to a proposed number;—Or, as commonly expressed, to find the cube root of a proposed number.

Rule.

Rule.

Divide the proposed number into as many periods as possible, consisting of three figures each, proceeding from right to left: subtract the highest cube that can be taken out of the remaining figure or figures on the left, placing that cube under those figures, and the remainder under the cube with a line between them; to the remainder annex the next period, which is called the resolvend, and place the root of the cube taken in the quotient.

Then any resolvend and the quotient being given, a new figure of the quotient will be obtained by annexing a cipher to the quotient figure; and calling the quotient thus increased, the increased quotient. Subtract the sum of three times the square of the increased quotient into the new figure, three times the increased quotient into the square of the new figure, and the cube of the new figure from the resolvend, and place the new figure in the quotient instead of the cipher, and annex the next period to the remainder for the new resolvend.

It is obvious that the new figure must be such that the sum to be subtracted must be as nearly equal as possible, but less than the given resolvend.

This rule as generally given in books of arithmetic is far from being explicit, as they do not show clearly how it is derived from the polynomial. In obtaining a new figure in the root, most authors direct the student to multiply the square of the quotient by 300, and this again by the new figure: then to multiply the quotient by 30, and this product by the square of the new figure; and lastly, to take the cube of the new figure, and add these three products together, and subtract as above: but this does not follow from the principle, though the effect must be the same.

Example.

Find a number whose cube shall be the nearest less number to 160634321856.

160‘634‘321‘856(5436

$a^3 = 125$

35634, 1st resolvend

$a^2b = 30000 = 3(50)^2 \times 4$

$ab^2 = 2400 = 3(50) \times 4^2$

$b^3 = 64 = \ldots 4^3$

32464

3170321, 2d resol.

$(a+$

$$(a+b)^2c=2624400=3(540)^2\times 3$$
$$(a+b)c^2=\quad 14580=3(540)\times 3^2$$
$$c^3=\quad 27 \quad .. \quad 3^3$$
$$\overline{2639007}$$

$$\overline{.531314856}, \text{ 3d resol.}$$
$$(a+b+c)^2d=530728200=3(5430)^2\times 6$$
$$(a+b+c)d^2=\quad 586440=3(5430)\times 6^2$$
$$d^3=\quad 216= \quad .. \quad 6^3$$
$$\overline{531314856}$$
$$0$$

In this operation, in order to obtain a new figure, the parts within the parenthesis always represent a number consisting of one place of figures more than the number of letters; the first letter represents the highest place of figures; the second the next highest; and so on, to the right-hand place, where a cipher is introduced.

This number is therefore the number formed by the figures in the root obtained by the preceding periods with a cipher annexed to the right hand; so that when the cipher is replaced by the new figure, the number formed will be the root of the proposed number as far as the number of periods that are used. Thus $a+b=540$ and $a+b+c=5430$.

But this operation may be simplified, as in the following, which is performed according to the last example in the involution herein.

The method of pointing off is the same as before directed; the only difference in finding a period of the root is expressed in the following short

Rule.—Subtract the product of the new figure into the sum of the square of the increased quotient, the product of the increased quotient into the new figure, and the square of the new figure from the resolvend.

$$a^2=7500$$
$$ab=\ 600$$
$$b^2=\ 16$$
$$\overline{8116}=\acute{D}$$
$$(a+b)^2=874800$$
$$(a+b)c=\ 4860$$
$$c^2=\ 9$$
$$\overline{879669}=\acute{\acute{D}}$$
$$(a+b+c)^2=88454700$$
$$(a+b+c)d=\ 97740$$
$$d^2=\ 36$$
$$\overline{88552476}$$

$$160'634'321'856(5436$$
$$a^3=125$$
$$\overline{\quad 35634}$$
$$4\times\acute{D}=32464$$
$$\overline{\quad 3170321}$$
$$3\times\acute{\acute{D}}=2639007$$
$$\overline{\quad .531314856}$$
$$6\times D'''=531314846$$
$$\overline{\quad 0}$$

The

The method of finding the triple square in the first row of any period independent of an additional operation, wes suggested to me by a Mr. Holdred, who is now about to publish a small tract, in which he has shown by an original and ingenious method, how the roots of equations as well as the roots of numbers may be accurately and easily extracted by one method which is not an approximation, but as direct as the rule for division or those employed in the extraction of roots can be. His principle appears to be new, and it is more general than any thing of the kind that has yet appeared in this country.

LVI. *An alphabetical Arrangement of the* Places *from whence Fossil Shells have been obtained by Mr.* James Sowerby, *and drawn and described in Vol.* II. *of his "Mineral Conchology," with the geographical and stratigraphical Situations of those Places, the Species and Varieties of Fossil Shells, &c. By Mr.* John Farey *Sen., Mineral Surveyor.*

To Mr. Tilloch.

Sir, — Mr. Sowerby in June last completed a second volume of his excellent work, entitled "Mineral Conchology," containing 101 coloured Plates, of the Shells of formerly existing Fish, which have been found imbedded in the British Strata, with one or two exceptions, as to Shells found near the opposite coast of France; and in which volume he has given the names and descriptions, of 184 species of such Fossil Shells, that were widely distributed through the British series of Strata; yet *all of them prove perfectly distinct from any Species of the Shells of analogous living Fish, in any known part of the World!* which last, further confirms, if any confirmation were at this day wanting, what that experienced and enlightened naturalist Sir Joseph Banks has, uniformly and for many years past been often heard to say, as to every Shell found imbedded in the Strata, which he had seen, *being of an extinct species!*

Besides the above number of Fossil Shells described and *named* in this volume, Mr. Sowerby has therein described 5 Varieties, of as many species of these Shells, and has distinguished them by the addition of β, after their specific names: in like manner, I have ventured, in a *Stratigraphical Index,* which I have sent to Mr. S. (to be printed and accompany his 2nd volume) affixed Greek Letters, β, γ, δ, &c. to distinguish 33 other Varieties of these Shells, which, by the Places mentioned, and other circumstances connected therewith, and mostly also, by what is said and shown of the Shells themselves, appear to me to be *different Species, and belonging to different Strata,* from the Shells and Habitats

Habitats of the first 184 species, above mentioned; making in all 222 different Shells, whose places in Mr. Smith's series of the Strata, and their topographical situations, are ascertained in this volume.

While this second volume of Mr. Sowerby's Work has been in progress, Mr. Smith has published the first part of his "Stratigraphical System," (see P.M. vol. 50. p. 271) describing or mentioning 1155 specimens of Fossil Shells, Coralites, &c. found in the Strata which over-lie the Lias, at 263 different Places in England; and he has also published, three numbers of his "Strata Identified," containing figures of the most characteristic Shells, Coralites, &c. in the upper part of the British Series, over-lieing the Cornbrash Limestone: I have lately very carefully collated these four numbers of Mr. Smith's Works, with the first 37 numbers of "Mineral Conchology," and thereby I have been sorry to find, that a good many errors, as to the Strata and the varieties of Shells, have crept into the Index of *Places* (that are mentioned in Mr. Sowerby's 1st volume,) which is inserted in P.M. vol. xlvi. p. 211 to 224; several new Localities of the Shells described in the 1st volume, have also been recorded in vol. 2d.

In order to correct and supply these deficiencies, I have contrived, to introduce all the most material of the corrections and additions, wanting in the former Index, into the Index of *Places**, with their Strata and Shells, for which now I solicit insertion in your Work: nearly all of the remaining errors in the first Index may be corrected, and the Greek Letters added to distinguish the varieties of the Shells named, by means of, a List of the Shells

* I was in hopes, by some delay in setting about this Index, since Mr. Sowerby finished his 2nd Volme, that my labour in hunting through Maps *for the situations* of a large portion of *the Places* mentioned in his and Smith's Works, might ere this have been greatly shortened, by a reference to the Manuscript Index which my valued Friend, the able and indefatigable *Mr. Arrowsmith* of Soho-square, has for near two years been preparing, and which is intended to contain, *every Name*, of Towns, Villages, Farms and Cottages, Mills, Mines, Collieries and Quarries, Rivers, Streams and Waterfalls, Bays, Headlands, Cliffs and Lighthouses, Mountains, Hills and Valleys, Parks, Forests and Woods, &c. &c.; together with the District Names, &c. which are to be found, not only in his own large and unparalleled *Map of England and Wales*; but also, in all the largest County Maps, local Maps of Canals, Roads, Mining-Districts, &c. &c. which either his own large Collection contains, or to which he can have access, through the kindness of the friends of Science: unfortunately however for me, this great *Index to Localities*, although all the names from printed Maps were collected out (and ascertained by Bearings and Distances), and it is now rapidly proceeding towards its final revision and completion, it has not been in a state for me to consult it, as otherwise, the kindness and liberality of Mr. A. would have permitted, prior to its publication; which now will soon take place; with the addition, of the population, and *a blank column*, for future corrections and additions, and to enable this volume to be made, by Scientific, Curious, or Travelling Persons, into *an universal Index to Localities in South Britain!*

Shells and Coralites, which under the *same Name,* either Mr. Sowerby or Mr. Smith, have referred to *more than one Stratum:* which List I shall in a few days transmit to you, for insertion in your Work, accompanied by some Remarks, on the use and importance of the knowledge of Fossil Shells, in conducting Geological investigations*: it may however be proper here to notice the other corrections following, viz. p. 215, *dele* the 3d line, beginning with Ditto:—p. 217, p. 20 and 21, for Blue Marl above the Lias, *read, Green sand:* l. 32, before Terebratula obsoleta, *insert* Ditto, in *Clunch Clay* lower part:—p. 218, l. 7 and 8, after Goat-acre, *insert* 3½ m. NNE of Calne; and *dele,* under the great Bath Oolite:—l. 10 from bottom, after in, *insert* Oaktree Clay, below. p. 220, under Longleat, *insert* Pecten quadricostata, t. 56, f. 1 and 2:—l. 2 and 3 from bottom, *dele,* or Melburg:—and p. 224, l. 11, for great, *read,* under. I am,

Your obedient humble servant,

Howland-street, Oct. 10, 1818. JOHN FAREY Sen.

An alphabetical List of the PLACES *from whence* FOSSIL SHELLS *have been obtained by Mr.* JAMES SOWERBY, *and described in Vol.* II. *of* MIN. CONCH: *each referred to its proper* STRATUM *in Mr.* SMITH's *Series and Map.*

Adlington Hills (N of Romney Marsh), 10 m. W of Folkstone, Kent, in Portland Rock.

Gryphæa dilatata, *var.* β *tab.* 149, f. 2.

Aldborough, see vol. 46, p. 212, 2 species of Shells, in Crag Marl.

Tellina obliqua, t. 161, m. | Voluta Lamberti, t. 129, f. 3.

Amberley-Heath, ½ m. SW of Minchin-Hampton, Glouc. in Forest Marble.

Patella rugosa, t. 139, f. 6.

Aswarby, near, 3½ S of Sleaford, Lincolnshire, in Cornbrash Limestone.

Ammonites Herveyi α, t. 195, u.

Aynhoe, see vol. 46, p. 212, 4 species, in Fullers'-earth Rock.

Ostrea acuminata γ, t. 135, f. 3.

* I was not unmindful of the promise made at the end of my first Index, (P. M. vol. 46, p. 224,) and repeated p. 285, as to preparing a Paper on *Fossil Shells,* intended for your Work: just as I had finished the same, Sir Richard Phillips undertook, at my request, to give some account in his "Monthly Magazine," (see vol. xl. p. 379) of my Friend Mr. Smith's *Map of the Strata, and Memoir,* for which purpose I lent him my Copies of the same: and thinking that the Paper I had drawn up, and addressed but not sent to you, would somewhat explain to Sir R. the nature and objects of those Works on Fossil Shells, which my friend Smith intended publishing, I lent this manuscript (retaining no copy) with Smith's Map, and never got it back again: it was pretended to have been sent to me by the Two-penny Post.

Babling-

Babling-Hill, of Yeovil, Somerset, in Under Oolite.
Astarte elegans, t. 157, f. 3.

Barry-Island, see vol. 46, p. 213, 1 species, in blue Lias.
Plagiostoma punctata, t. 113, f. 2.

Barton Cliff, see vol. 46, p. 213, 25 species, in London Clay, upper part.

Auricula simulata, t. 163, f. 5 to 8.
Cardium semigranulatum, t. 144.
Cerithium geminatum, t. 127, f. 2.
——— pyramydale, t. 127, f. 1.
Murex carinella, t. 187, f. 3 and 4.
——— fistulosus, t. 189, f. 1 and 2.
——— regularis, t. 187, f. 2.
——— tubifer, t. 189, f. 3 to 8.
Nucula minima, t. 192, f. 8 & 9.
——— similis, t. 192, f. 10.
——— trigona. t. 192, f. 5.
Pleurotoma colon, t. 146, f. 7 and 8.
——— exorta, t. 146 f. 2.
——— rostrata α, t. 146, f. 3.
Voluta ambigua (monst.), t. 115, f. 5.
——— luctator, t. 115, f. 1.
——— spinosa α, t. 115, f. 2 and 4.
——— ——— β, t. 115. f. 3.

Ditto, in London Clay, lower part.
Cyclas obovata, t. 162, f. 4 to 6.

Bath, in NE corner of Somersetshire (near), see vol. 46, p. 213, Clay on upper Oolite.
Ostrea acuminata α, t. 135, f. 2.

Ditto, see ditto, 1 species (Terebratula digona γ, t. 96) in upper Oolite.

Ditto, see ditto, 2 species (Mactra gibbosa α, and Terebratula media) in Fullers' Earth Rock.

Ditto, see ditto, 1 species (Nautilus lineatus α) in under Oolite.
Cardita? obtusa α, t. 197, f. 2.
——— producta α, t. 197, f. 1.
Lima gibbosa α, t. 152.
Planorbis euomphalus β, t. 140, f. 8 and 9.

Ditto, in Marlstone.
Ammonites Walcotii α, t. 106.

Ditto, W, see vol. 46, p. 213, 1 species in Blue Lias.
Ammonites Bucklandi, t. 130.
——— Conybeari, t. 131.
——— Greenoughi, t. 132.
Cardita? lirata α, t. 197, f. 3.
Gryphæa incurva α, t. 112, f. 1.
Nautilus truncatus, t. 123.
Unio crassissimus, t. 153 (in Clay).

Ditto, see ditto 1 species (Plagiostoma gigantea α, t. 77) in White (and Blue?) Lias.

Bawdsey

Bawdsey Cliff, 8m. SE of Woodbridge, Suffolk, in Crag, on Blue Clay.

Cassis bicatenatus, t. 151.
Nucula lanceolata, t. 180, f. 1.
Unio crassicusculus, t. 185.
Voluta Lamberti, t. 129.

Bayeux, 11 m. WNW of Caen in Normandy, in under Oolite.

Ammonites Brongniarti, t. A. f. 2, p. 190.
——— Gervillii, t. A f. 3, p. 189.
——— Cardita? producta α, t. 197, f. 1.

Bennington, 4 m. ESE of Stevenage, Herts, in London Clay, lower part.

Gryphæa dilatata α, t. 149, f. 1.

Birdbrook, 9½ m. NW of Halstead, Essex, in Crag, perhaps alluvial?

Gryphæa incurva β, t. 112, f. 2, alluvial?
Nautilus intermedius β, t. 125.

Black-down Hills, see vol. 46. p. 214, 10 species, in Green Sand.

Auricula incrassata, t. 163, f. 1 to 3.
Cardium proboscideum, t. 156, f. 1.
——— umbonatum, t. 156, f. 2 to 4.

Black-Rock, near Cork, see vol. 46, p. 214, 5 species, in Derbyshire-peak Limestone.

Spirifer cuspidatus, t. 120, f. 5.

Bolingbroke, 3¼ WSW of Spilsby, Lincolnshire, in Clunch Clay.

Patella latissima α, t. 139, f. 1.

Boreham, 1 m. ESE of Warminster, Wilts, in Green Sand.

Nautilus simplex, t. 122.

Bourn, E, 7½ W of Spalding, Lincolns. in Clunch Clay.

Gryphæa dilatata γ, t. 149.

Bracklesham Bay, see vol. 46, p. 214, 3 species (Melania sulcata, t. 39, and Turritella conoidea, t. 51, f. 1. being omitted, II. 239) in London Clay, upper part.

Sanguinolaria Hollowaysii, t. 159.

Bradford, E 4½ WSW of Calne, Wilts, see vol. 46, p. 214, 1 species, in Clay on upper Oolite.

Ditto, SW, in under Oolite.

Ammonites Herveyi β, t. 195, lo.

Bramberry-Hill, ½ m. SE of Clyne Church, on SE coast of Sutherland, Scotland, in Mountain Limestone?

Gryphæa dilatata η, t. 149.

Bramerton-Hill, see vol. 46, p. 214, 5 species, in Crag Marl.

Astarte plana, t. 179, f. 2, (perhaps alluvial?)
Mactra cuneata, t. 160, f. 7.
Nucula Cobboldiæ, t. 180, f. 2.
Tellina obtusa, t. 179, f. 4.
——— ovata, t. 161, f. 2.

Bridport,

Bridport, near, $13\frac{1}{2}$ W of Dorchester, Dorset, in the under Oolite ? Marl.

Ammonites Stokesi, t. 191.

Brockenhurst, see vol. 46, p. 215, 1 species, in the London Clay, upper part.

Venus incrassata, t. 155, f. 1 and 2.

Bromham, $3\frac{1}{2}$ m. NW of Devizes, Wilts. in the Portland Rock.

Gryphæa dilatata β, t. 149, f. 2.

Bugthorp, $5\frac{1}{2}$m. NNW of Pocklington, York ER, in the Blue Lias.

Trochus Anglicus, t. 142.

Calne, W, 6 m. N by W of Devizes, Wilts, in the Clunch Clay.

Gryphæa dilatata γ, t. 149.

Cambridge, Castle-hill, see vol. 46, p. 215, 1 species, in the Chalk-marl.

Ditto, N, in the Oak-tree Clay.

Ostrea deltoidea α, t. 148.

Cardiff, Castle-hill, see vol. 46, p. 215, 1 species, in the Blue Lias.

Plagiostoma punctata, t. 113, f. 2.

Carrington, Oxfordshire, in the under Oolite.

Pecten equivalvis, t. 136, f. 1. | Pecten fibrosus η, t. 136, f. 2.

Castleton, see vol. 46, p. 215, 2 species, in the Derbyshire-peak Limestone.

Spirifer cuspidatus, t. 120.

Chapel-House, 1 m. NE of Chipping-Norton, Oxfordsh. in the under Oolite.

Cardita? producta α, t. 197, f. 1.

Charlton, 1 m. SW of Woolwich, Kent, in the London Clay, lower part.

Cerithium intermedium, t. 147, f. 3 and 4.
——— melanioides α, t. 147, f. 6.
Cyclas cuneiformis, t. 162, f. 2 and 3.
——- deperdita? t. 162, f. 1, (with Chert nodules).
——- obovata, t. 162, f. 4 to 6.

Chatley, $4\frac{1}{2}$ m. N by W of Frome, Somerset, see vol. 46, p. 215, 4 species (see below), in the Cornbrash Limestone.

Pecten fibrosus α, t. 136, f. 2.

Ditto, 1 species (Terebratula ornithocephala, t. 101, f. 1 and 2, being omitted) in the Kelloway Stone.

Gryphæa incurvata γ, t. 112, f. 2?

Chicksgrove Quarry (of which the sinking is particularized, vol. 2. p. 58) $4\frac{1}{4}$ m. SE of Hindon, Wilts, in the Portland Rock.

Ammonites gigantea α, t. 126.
——————— β, t. 126.
Astarte cuneata, t. 137, f. 2.

Childrey, 2 m. WNW of Wantage, Berks, in the Chalk-marl.

Pecten Beaveri, t. 158.

Chilmark, $3\frac{3}{4}$ m. E of Hindon, Wilts, in the Portland Rock.
Astarte cuneata, t. 137, f. 2.

Chute Farm, see vol. 46, p. 216, 8 species, in the Green Sand.
Terebratula Lyra, t. 138, f. 2.

Colebrook-Dale, see vol. 46, p. 216, 3 species, in the Derbyshire-peak Lime-stone.
Orthocera annulata, t. 133.

Ditto, in the Coal-measures.
Ammonites Walcotii δ, t. 106.

Colomby (St.), 4 m. SSW of Volagne, in the Cotentin, the Department of the Channel, or Lower Normandy, France, in Limestone, with quartz grains.
Ammonites constrictus, t. A. f. 1, p. 189.
Cerithium Cornucopiæ β, t. 188, f. 3 and 4.

Comb-Pyne, 10 m. ENE of Sidmouth, Devon, in the lower Chalk.
Ammonites rusticus, t. 177.

Coney-Weston, $5\frac{1}{2}$ m. WNW of Bottesdale, Suffolk, in the London Clay, lower part.
Gryphæa dilatata α, t. 149.

Cork, see *Black Rock.*

Cotentin district, see *Colomby.*

Cotswold-Hills, see vol. 46, p. 216, 1 species, in the upper Oolite.

Ditto, in the under Oolite.
Lima gibbosa α, t. 152.

Cowes, East, (near) at N end of Isle of Wight, Hants, 1 species, (Natica depressa, t. 5, being omitted, ii. 239) in the Cowes Rock, of Limestone.
Lymnæa fusiformis, t. 169, f. 2 and 3.
——— minima, t. 169, f. 1.
Planorbis cylindricus, t. 140, f. 2.
Planorbis euomphalus α, t. 140, f. 7.
——— Lens, t. 140, f. 4.
——— obtusus, t. 140, f. 3.

Culford-Hall, $4\frac{1}{2}$ m. NNW of Bury St. Edmunds, Suffolk, in the Crag Marl?
Nautilus intermedius β, t. 125.

Derbyshire, see vol. 46, p. 216, 2 species, in the Coal-measures.

Ditto, 1 species, in the Derbyshire-peak Limestone.
Cirrus acutus, t. 141, f. 1.
Helix? cirriformis, t. 177, f. 2.
——- striatus, t. 171, f. 1.

Devizes, NE, see vol. 46, p. 216, 1 species, in the upper Chalk.

Ditto, N, in the Canal, 4 species, in the Green Sand.
Ammonites auritus, t. 134.
Cardita? tuberculata, t. 143.
Helix Gentii, t. 145.
Ostrea gregarea α, t. 111, f. 1.
Pecten orbicularis, t. 186.
Pleurotoma rostrata β, t. 146, f. 3.

Devon-

Devonshire, in the Derbyshire-peak Limestone.
Ammonites Walcotii ε, t. 106.

Donat's Castle, 2½ m. ENE of Cowbridge, Glamorganshire, in the Blue Lias.
Gryphæa obliquata, t. 112, f. 3.
Plagiostoma punctata, t. 113, f. 1.

Dover, SW, under Cliff, 7 m. NE of Folkstone, Kent, in the Chalk-marl, pyritic.
Nucula pectinata, t. 192, f. 6 and 7.

Dry-Sandford, 2½ m. NW of Abingdon, Berks, in the Portland Rock.
Ammonites excavatus, t. 105.
——— plicatilis, t. 166.
Ammonites vertebralis, t. 165.

Dundry-Hill, 3½ m. SSW of Bristol, Somerset, in the under Oolite.
Ammonites Braikenridgii, t. 184.
——— Brocchii, t. 202.
Cardita? obtusa α, t. 197, f. 2.
Trochus abbreviatus, t. 193, f. 5.
——— elongatus, t. 193, f. 2 to 4.
——— punctatus, t. 193, f. 1.

Durham County, in the blue beds of the buff or yellow Limestone?
Unio Listeri β, t. 154, f. 1, 3 and 4.

Dursley, 4 m. W of Berkeley, Gloucesters. in the under Oolite.
Pecten equivalvis, t. 136, f. 1.

Earl-Stoke, 7 m. NE of Warminster, Wilts, 1 species (Turrilites costata α, t. 36, being omitted, ii. 45) in the Chalk-marl
Nautilus Comptoni, t. 121.

Emsworth, N, 1½ m. NE of Havant, Hants, 1 species (Pecten quinquecostata, t. 56, being omitted, ii. 239) in Flint in the upper Chalk.

Ditto, NW, on common, Sand-pit, 1 species (Dentalium cylindricum, t. 79, being omitted, ii. 239) in alluvia of the Green Sand?

Farley-Gate, Gloucestershire, in the under Oolite.
Gryphæa dilatata δ, t. 149, f. 1.
Pecten equivalvis, t. 136, f. 1.

Filliagh, 3 m. WNW of South Moulton, Devon, 1 species (Ammonites striatus β, t. 53, f. 1, being omitted, ii. 69) in the Coarse Slate, or Killas.

Folkstone, NE, see vol. 46, p. 217, 11 species, in the Chalk-marl.
Ammonites splendens α, t. 103, f. 1 and 2.
Cirrus plicatus, t. 141, f. 3.
Nucula pectinata, t. 192, f. 6 and 7.
Patella lævis, t. 139, f. 3.

Fonthill, see vol. 46, p. 218, 1 species, in the Green Sand.

Ditto, SE, (Chicksgrove)? in the Portland Rock.
Ammonites giganteus α, t. 126.

Fox-hill Quarries, Gloucestershire, in the under Oolite.

Astarte lurida, t. 137, f. 1.

Framilode, near, 7 m. SW of Gloucester, in the Blue Lias.

Gryphæa incurva α, t. 112, f. 1, (Pak. iii. 209).

Framlingham, 3½ m. SE of Norwich, in the Crag-marl.

Tellina ovata, t. 161, f. 2.

France, in the under Oolite.

Terebratula acuta, t. 150, f. 1.

Frethern (or Freborne) 5½ m. NNE of Berkeley, Gloucest. in the Blue Lias.

Gryphæa incurvata α, t. 112, f. 1.

Giles's, St., Gate, of Norwich, 1 species (Terebratula subundata α, t. 15, f. 7, being omitted) in the upper Chalk.

Grignon Quarries, 19 m. W of Paris, in "Coarse Limestone," &c. (P. M. 35, p. 118) answering to the London Clay, upper part? (see P. M. 35, p. 132).

Cerithium giganteum, t. 188, f. 2.
Murex tubifer, t. 189, f. 3 to 8.

Haldon Hills, see vol. 46, p. 218, 4 species, in the Green Sand.

Planorbis euomphalus γ, t. 140, f. 8.
——— radiatus, t. 140, f. 5.

Hampton-common, 1 m. WNW of Minchin Hampton, Gloucestersh. in the Forest Marble.

Patella rugosa, t. 139, f. 6.

Hamsey, see vol. 46, p. 218, 7 species, in the Chalk-marl.

Ammonites varians, t. 176.
Cerithium melanioides β, t. 147.
Pecten Beaveri, t. 158, u.

Harwich, SSE, see vol. 46, p. 218, 2 species, in the Crag-marl.

Voluta Lamberti, t. 129.

Headington, 1½ m. ENE of Oxford, in the Oaktree Clay, pyritic.

Astarte lineata, t. 179, f. 1.

Ditto, Common (not Heddington, Wilts !), see *Shotover Hill.*

Headon-Hill, SW of West Cowes, see *Cowes.*

Highgate, SE, see vol. 46, p. 218, 22 species, in the London Clay, upper part.

Auricula simulata, t. 163, f. 5 to 8.
——— turgida, t. 163, f. 4.
Murex coniferus, t. 187, f. 1.
——— curtus, t. 199, f. 5.
Murex tubifer, t. 189, f. 6 to 8.
Nucula minima, t. 192, f. 8 & 9.
——— similis, t. 192, f. 3 & 4.
Pleurotoma acuminata, t. 146, f. 4.

Hilary, St., 1½ m. SE of Cowbridge, Glamorganshire, in the Derbyshire-peak Limestone.

Spirifer cuspidatus, t. 120, f. 1 to 4.

Hollesley, 5 m. SW of Orford, Suffolk, in the Crag Marl.

Venus rustica, t. 196.

Holywell,

Holywell, see vol. 46, p. 219, 14 species, in the Crag Marl.

Astarte obliquata, t. 179, f. 3.
Buccinum granulatum, t. 110, f. 4.
——— reticosum, t. 110, f. 2.
——— rugosum, t. 110, f. 3.
Mactra arcuata, t. 160, f. 1 and 6.
——— dubia, t. 160, f. 2 to 4.
Murex striatus β, t. 109.
Nucula Cobboldiæ, t. 180, f. 2.
——— lævigata, t. 192, f. 1 and 2.
Patella equalis, t. 139, f. 2.
——— unguis, t. 139, f. 7 & 8.
Tellina obliqua, t. 161, f. 1 and m.
Trochus lævigatus, t. 181, f. 1.
——— similis, t. 181, f. 2.
Voluta Lamberti, t. 129.

Hordle-Cliff, see vol. 46, p. 219, 7 species, in the London Clay, upper part.

Cerithium funatum, t. 128, f. 2.
——— pyramydale, t. 127, f. 1.

Horningsham, see vol. 46, p. 219, 1 species, in the lower Chalk.
Ditto, 1 species, in the Green Sand.

Terebratula pectita, t. 138, f. 1.

Huntcliffe, 6 m. NE of Gisborough, York NR, in the Kelloway Stone?, ii. 239. (see P. M. xlix. p. 251, Note †.)

Chama digitata β, t. 174.

Hythe, N, 4½ m. WSW of Folkstone, Kent, in the Green Sand, marly. Ammonites Nutfieldiensis α, t. 108.

Ilminster, E, see vol. 46, p. 220, 2 species, in the under Oolite.

Pecten equivalvis, t. 136, f. 1.
Terebratula acuta, t. 150, f. 2.
——— resupinata, t. 15, f. 3 and 4.

Ditto, S, in the Clunch Clay.

Gryphæa dilatata γ, t. 149.

Kelloways-Bridge, see vol. 46, p. 220, 1 species, in the Kelloway Stone. Ammonites Calloviensis α, t. 104.

Cardita? deltoidea β, t. 197, f. 4.
Pecten fibrosus γ, t. 136,.
Plagiostoma obscura, t. 114, f. 2.

Kendal (near), see vol. 46, p. 220, 2 species, in the Derbyshire-peak Limestone.

Planorbis equalis, t. 140, f. 1.

Keynsham, 4 m. SE of Bristol, Somersetshire, in the Blue Lias.

Nautilus intermedius α, t. 125.
——— truncatus, t. 123.

Knowles-Hill, 2 m. SSE of Bruton, Somerset, in the under Oolite.

Ammonites Herveyi β, t. 195, lo.

Lechlade, N, 3 m. ESE of Fairford, Gloucestersh. in the Cornbrash Limestone.

Cardita? deltoidea α, t. 197, f. 4.

Lewes, N, see vol. 46, p. 220, 2 species, in the lower Chalk?
Ditto, E, 1 species, in the upper Chalk?
Terebratula octo-plicata, t. 118, f. 2.

Little Sodbury, see vol. 46, p. 220, 2 species, in the under Oolite.
Trochus concavus, t. 181, f. 3.
——— dimidiatus, t. 181, f. 4.
——— duplicatus, t. 181, f. 5.

Llantrissent, S, 9 m. NW of Cardiff, Glamorganshire, in the Derbyshire-peak Limestone.
Ammonites Walcotii ε, t. 106.

Long-Comb (or Lincomb) Girts, $4\frac{1}{2}$ m. N of Sidmouth, Devon, in the Kelloway Stone, flinty chert.
Chama digitata α, t. 174.

Lopham, 3 m. S of East Harling, Norfolk, in the London Clay, lower part.
Ostrea deltoidea β, t. 148.

Lyme-Regis, see vol. 46, p. 22, 2 species, in the Marlstone.
Ditto, NE, in the Blue Lias.

Ammonites Brooki, t. 190.	Ammonites Loscombi, t. 183.
——— fimbriatus, t. 164.	——— obtusus, t. 167.
——— Henleyi, t. 172.	Nautilus striatus, t. 182.

Lyth, 3 m. NW of Whitby, Yorks. see *Whitby*.

Malden, near, $7\frac{1}{2}$ m. E of Chelmsford, Essex, in the Crag Marl.
Murex costellifer, t. 199, f. 3.
——— echinatus, t. 199, f. 4.
——— rugosus β, t. 199, f. 1.

Marcham, $2\frac{1}{4}$ m. W of Abingdon, Berks, in the Portland Rock.
Ammonites excavatus, t. 105.
——— plicatilis, t. 166.
——— vertebralis, t. 165.

Margate, 4 m. NNW of Ramsgate, Kent, in the upper Chalk.
Terebratula plicatilis, t. 118, f. 1.

Marston-field, $1\frac{1}{2}$ m. NE of Oxford, in the Clunch Clay.
Ostrea palmetta, t. 111, f. 2.

Mitford (or Midford), see vol. 46, p. 221, 1 species, in the under Oolite.
Ammonites Walcotii γ, t. 106.

Mordiford, $3\frac{1}{2}$ m. ESE of Hereford, in the Derbyshire-peak? Limestone.
Terebratula Wilsoni, t. 118, f. 3.

Mundesley, 6 m. SE of Cromer, Norfolk, 1 species, (Terebratula carnea, t. 115, f. 5 and 6, being omitted, ii. 77) in the upper Chalk.

Magas pumilus, t. 119.	Ostrea canaliculata, t. 135, f. 1.

Neots, St., $7\frac{1}{2}$ m. SW of Huntingdon, in the Clunch Clay.
Ammonites Duncani, t. 157.

New-

New-cross, in Canal, $1\frac{1}{4}$ m. E of Peckham, Surrey, in the London Clay, lower part, with chert nodules.

Cerithium melanioides α, t. 147, f. 7.
Cyclas cuneiformis, t. 162, f. 2 and 3.
—— obovata, t. 162, f. 4 to 6.

Newhaven, SW, Castle-hill, $6\frac{1}{2}$ m. S by E of Lewes, Sussex, in the London Clay, upper part.

Cerithium funatum, t. 128, f. 1.

Ditto, in the London Clay, lower part.

Cerithium melaniodes α, t. 147, f. 7.

New Malton, N, 17 m. NE of York, in the Portland Rock.

Unio Listeri α, t. 154, f. 3 and 4.

Norfolk, County, see vol. 46, p. 221, 1 species, in the Crag Marl.

Tellina obliqua, t. 161, f. 1.

Northfleet, see vol. 46, p. 221, 1 species, in the upper Chalk.

Terebratula plicatilis, t. 118, f. 1.

Northleach, 10 m. NNE of Cirencester, Gloucestershire, in the upper Oolite.

Pecten fibrosus β, t. 136, f. 2.

Norton-Bavant, see vol. 46, p. 221, 2 species, (Hamites intermedius, t. 62, in Down Quarry? being omitted) in the lower Chalk.

Nautilus elegans, t. 116.

Norton, under Hamdon, $6\frac{1}{2}$ m. E by N of Ilminster, Somerset. in the under Oolite.

Nautilus obesus, t. 124.

Nottinghamshire, in the blue beds of Yellow Limestone ?.

Unio hybridus, t. 154, f. 2.

Nutfield, see vol. 46, p. 221, 1 species, in the Green Sand, brown.

Ammonites Nutfieldiensis α, t. 108.

Oxfordshire (Oxford E ?, and ENE ?) 2 species (Trigonia clavellata β, and costata β, t. 87 85, being omitted) in the Oak-tree Clay.

Pakefield, see vol. 46, p. 221, 1 species, in an alluvial lump of Limestone.

Gryphæa dilatata ε, t. 149. | Patella latissima β, t. 139, f. 5.

Paris, near, in France, see P. M. vol. 35, p. 116 to 124, 1 species (Venericardia planicosta, t. 50, being omitted) in the London Clay, upper part ?.

Cerithium pyramydale, t. 127, f. 1.
Nucula similis, t. 192, f. 3, 4, and 10.
Voluta Luctator, t. 115, f. 1.
—— spinosa β, t. 115, f. 3.

Paris, near, in France (see P.M. vol. 35, p. 116 to 124) in the London Clay, lower part ?.

Cyclas deperdita ? t. 162, f. 1.

Ostrea deltoidea β, t. 148.

Peterborough, 11 m. NE of Oundle, Northamptonshire, in the Cornbrash Limestone.

Cardita ? deltoidea α, t. 197, f. 4.

——— ? producta β, t. 197, f. 1.

Petty France, 4 m. NE of Sodbury, Gloucestersh. in the upper Oolite.

Plagiostoma cardiiformis, t. 113, f. 3.

Pickeridge-Hill, $4\frac{1}{2}$ m. SSE of Taunton, Somerset, see vol. 46, p. 221, 3 species, in the Blue Lias, and its Clay beds.

Plagiostoma pectinoides, t. 114, f. 4, in Clay.

——— punctata, t. 113, f. 1.

Plumstead, see vol. 46, p. 221, 5 species, in the London Clay, lower part, with Cherts, Sand, Loam, &c.

Cerithium funiculatum, t. 147, f. 1 and 2.

Cyclas cuneiformis, t. 162, f. 2 and 3.

—— deperdita ? t. 162, f. 1.

Cyclas obovata, t. 162, f. 4 to 6.

Murex graduatus, t. 199, f. 6.

—— rugosus γ, t. 199, f. 2.

Planorbis hemistoma, t. 140, f. 6.

Plumpton, $3\frac{1}{4}$ m. NW of Lewes, Sussex, in the Chalk Marl.

Ammonites varians, t. 176.

Portland Isle, see vol. 46, p. 221, 3 species, in the Portland Rock.

Gryphæa dilatata β, t. 149, f. 2.

Purbeck Isle, or Peninsula, S of Corfe-Castle, Dorsetshire, in the Portland Rock.

Ammonites gigantea α, t. 126.

Radipole, see vol. 46, p. 222, 1 species, in the Portland Rock.

Gryphæa dilatata β, t. 149, f. 2.

Regent's-Park, in Canal, 1 m. N of London, in the London Clay, upper part.

Cardium semigranulatum, t. 144.

Richmond-Park Well, see vol. 46, p. 222, 3 species, in the London Clay, upper part.

Voluta Luctator, t. 115, f. 1.

Ringmer, see vol. 46, p. 222, 2 species, in the Chalk Marl.

Nautilus elegans, t. 116.

Roak, 4 m. W of Watlington, Oxfordshire, in the Chalk Marl.

Ammonites rostratus, t. 173.

Hamites armatus, t. 168.

Romney-Marsh, bounding Hills, see *Adlington.*

Roydon-Green, 1¾ m. NW of Diss, Norfolk, in the Crag Marl, perhaps alluvial ?.

Nucula Cobboldiæ, t. 180, f. 2.
Tellina obtusa, t. 179, f. 4.
Unio Listeri γ, t. 154, f. 1.

Rude-Cliff, 3 m. NE of Weymouth, Dorset, in the Portland Rock.

Gryphæa dilatata β, t. 149, f. 2.

Sandfoot-Castle, 1 m. SSW of Weymouth, Dorset, in the Clunch Clay.

Gryphæa dilatata γ, t. 149. | Ostrea deltoidea γ, t. 148.

Sandgate, 7½ m. SE of Dover, Kent, in the Green Sand.

Ammonites monile, t. 117.

Seamer, 4 m. SW of Scarborough, York NR, in the Portland Rock.

Unio Listeri α, t. 154, f. 3 and 4.

Shalcombe, 3¼ m. SE of Yarmouth, in the Isle of Wight, Hants, in the Cowes Rock ?

Helix globosus, t. 170.
Phasianella angulosa, t. 172, f. 2.
Phasianella minuta, t. 175, f. 3.
———— orbicularis, t. 175, f. 1.

Sherborn, E, in Park-Well, 6 m. W by S of Stalbridge, Dorset, see vol. 46, p. 222, 1 species, in the under Oolite.

Ammonites Banksii, t. 200.
———— Blagdoni, t. 201.
Ammonites Brocchii, t. 202.

Shotover-Hill, see vol. 46, p. 222, 1 species, in the Brick Earth on Portland or Aylesbury, Limestone.

Ditto, 2 species (omitting Melan. Hed. in the Portland Rock.

Ammonites excavatus, t. 105. | Plagiostoma rigida, t. 114, f. 1.

Ditto, in the Oak-tree Clay.

Ostrea deltoidea α, t. 148.

Ditto, see vol. 46, p. 222, 1 species (Melania Heddingtonensis, t. 39, r. le, being wrong placed) in the Coral Rag.

Small-Cossal, of Bath, in the Fullers' Earth Rock.

Plagiostoma ovalis, t. 114, f. 3.

Southfleet, 3 m. SW of Gravesend, Kent, in the London Clay, lower part.

Cerithium melanioides α, t. 147, f. 7.

Spalden, an error, see *Aswarby*.

Stanton-Hill, 4 m. NE of Winchcombe, Gloucestershire, in the under Oolite.

Terebratula acuta, t. 150, f. 2.

Stubbington Cliff, see vol. 46, p. 223, 11 species (9 of them being omitted, see ii. 230, as below*) in the London Clay, upper part. Ceri-

* viz. Cardium Plumsteadiense, t. 14, r. l.
Cassis carinatus, t. 6 m.
Dentalium entalis, t. 70.

Fusus

Cerithium Cornucopiæ α, t. 188, f. 1, 3 and 4.
——— dubium, t. 147, f. 5.
——— giganteum, t. 188, f. 2.
Pleurotoma attenuata, t. 146, f. 1.
——— comma, t. 146, f. 5.
——— semicolon, t. 146, f. 6.
Voluta spinosa β, t. 115, f. 3: ii. 239.

Suffolk, County, NW, see vol. 46, p. 223, 1 species, in the lower Chalk.

Ditto, in the London Clay, lower part.
Gryphæa dilatata α, t. 149, f. 1.

Ditto, in the Crag Marl.
Mactra ovalis, t. 160, f. 5.
Tellina obliqua, t. 161, f. 1 and m.
——— ovata, t. 161, f. 2.
Unio Listeri γ, t. 154, f. 1, 3, or 4?
Venus gibbosa, t. 155, f. 3.
——— lentiformis, t. 203.

Sussex, County, see vol. 46, p. 223, 1 spe. in the Chalk Marl.
Hamites armatus, t. 168. | Nucula pectinata, t. 192, f. 6 & 7.

Taunton, 9 m. NW of Ilminster, Somerset, in the under Oolite.
Astarte lurida, t. 137, f. 1. | Lima gibbosa α, t. 152.

Trent River (upper part, NE of Newcastle-under-Line, Staffordshire?) in the Coal-measures? *
Ammonites Walcotii δ, t. 106.

Under-cliff, in S part of Isle of Wight, Hants, in the Green Sand.
Ammonites inflatus, t. 178.

Vincent's, St., Rock, 1 m. W of Bristol, Gloucestershire, in the Derbyshire-peak Limestone.
Spirifer cuspidatus, t. 120, f. 1 to 4.

Walton le Soken (Nase), see vol. 46, p. 223, 2 species, in the Crag Marl
Buccinum elongatum, t. 110, f. 1.
Pholas cylindricus, t. 198.
Venus lentiformis, t. 203 (Essex Cliff).

Westbrook, 6 m. SE of Melksham, Wilts, in the Coral Rag, and Clay.
Ammonites splendens, t. 103, f. 3.
Ostrea gregarea β, t. 111, f. 3.

Weston, 1 m. NW of Bath, Somerset, in the Blue Lias.
Trochus anglicus, t. 142.

Fusus longævus, t. 63.
Pectunculus costatus, t. 27.
Rostellaria? lucida, t. 91.
Scalaria acuta, t. 16.
Trochus Benettiæ, t. 98, le.
Venericardia planicosta, t. 50.

* The lower part of the Trent River (although running upon Red Marl) in a remarkable manner skirts the foot of the *Lias* Strata, for 165 miles, in Nottinghamshire and Yorkshire: and perhaps, therefore, a reference to Lias was intended, instead of Coal-measures?.

Whitby,

Whitby, see vol. 46, p. 224, 3 species, in the Alum Shale, or lower Clunch Clay.

Ammonites angulatus, t. 107, f. 1.
——— communis, t. 107, f. 2 and 3.
——— Walcotii β, t. 106.
Patella lævis, t. 139, f. 4.

White-conduit House, 1 m. N of London, in the London Clay, upper part.

Cardium semigranulatum, t. 144.

White-Lackington, see vol. 46, p. 224, 1 species, in the under Oolite.

Ammonites Walcotii γ, t. 106. | Pecten equivalvis, t. 136, f. 1.

Ditto, NW, in the Blue Lias.

Trochus anglicus, t. 142.

Wight, Isle of, on S coast of Hampshire, in the London Clay, lower part, pyritic.

Cyclas cuneiformis, t. 162, f. 2 and 3.

Wiltshire, see vol. 46, p. 224, 1 species, in the Clunch Clay, lower part.

Ditto, in the Chalk Marl.

Ammonites varians, t. 176.

Windcombe, see *Donhead St. Mary.*

Withyham, 6 m. ESE of East-Grinstead, Sussex, in the Brick-Earth ?

Ostrea acuminata β, t. 135, f. 2.

Woburn, N, 6 m. SW of Ampthill, Beds, in the Clunch Clay, upper part.

Gryphæa dilatata γ, t. 149.

Woodbridge, see vol. 46, p. 224, 2 species, in the Crag Marl.

Mactra dubia, t. 160, f. 2 to 4. | Nucula lævigata, t. 192, f. 1 and 2. | Tellina obtusa, t. 179, f. 4.

Woolwich, SW, 7 m. NW of Dartford, Kent, in the London Clay, lower part.

Cyclas deperdita ? t. 162, f. 1.

Yeovil, NE, see vol. 46, p. 224, 2 species, in the Marlstone.

Ditto, in the under Oolite.

Ammonites Brongniarti, t. A. f. 2, p. 190. | Cirrus nodosus, t. 141, f. 2. | Nautilus sinuatus, t. 194.

Ditto, N, in the Blue Lias.

Trochus anglicus, t. 142.

LVII. *An*

LVII. *An Account of Experiments for determining the Length of the Pendulum vibrating Seconds in the Latitude of London. By Capt.* HENRY KATER, *F.R.S.*

[Continued from p. 182.]

Detail of the Experiments.

IN the first experiments which were made with the pendulum, it has been already observed that the knife edges rested on plates of hard steel; but as these at the conclusion were found to have suffered penetration in no slight degree, planes of agate were substituted for them, and the results having thus been rendered doubtful, were deemed inadmissible. It may not however be irrelevant to remark, that the distances of the knife edges obtained by the two methods which have been before described, did not differ quite one ten-thousandth of an inch; and, that on remeasurement after the knife edges had been used a very considerable time, their distance was found to be increased, by wear, four divisions only of the micrometer, or not quite two ten-thousandths of an inch. The length of the seconds pendulum deduced from these first experiments, differed from the result of the observations about to be detailed, only two ten-thousandths of an inch *in defect*. I nevertheless think it useless to insert these first experiments, as the near approximation of the result cannot but be deemed to have been in some degree accidental.

In repairing the knife edges after the termination of the first series of experiments, one of them was broken, and when it was replaced by another, the distance between them was increased about one-hundredth of an inch; a circumstance which proved rather gratifying than otherwise, as it afforded a pendulum differing in length from the former one, and which yet gave nearly the same result.

June 9th, 1817, the knife edges being adjusted parallel to each other, and the scale and pendulum having remained together for several preceding days, the pieces A, *a*, and B, *b*, were applied to the knife edges in the manner described in the former part of this paper, and the following measurements were taken.

Distance from A to a, 329·06 divisions.
B to b, 366·97.

Date.	Readings of the Micrometer.			Divisions +39·4 in.
	A to a.	B to b.	Scale.	
June 9th.	27·0	62·0	653·0	956·51
	21·0	52·0	642·7	954·21
	13·0	52·0	642·5	958·01
	12·0	48·0	638·5	956·51
	18·0	50·0	643·0	957·01
	18·0	50·0	642·0	956·01

TABLE continued.

Table continued.				
The Pieces changed.				
June 10th.	65·5	112·7	698·0	956·91
	64·0	112·5	696·0	955·76
	61·0	106·2	693·2	957·61
	64·5	108·0	693·5	955·26
	65·0	106·2	694·5	956·91
	67·2	107·0	696·0	956·91
			Mean of the whole	956·47

Hence the distance between the knife edges is 39·4 inches +956·47 divisions of the micrometer.

June 12th, the knife edges having been adjusted parallel to each other, the following measurements were taken, the knife edges being viewed as *dark* objects on a *white* ground.

Dark on a White Ground.					
	Readings of the Micrometer.				
Date.	Near side of the bar.	Further side of the bar.	Mean.	Scale.	Divisions +39·4 in.
June 12	50·0	50·0	50·00	1006·5	956·50
	50·0	50·0	50·00	—	956·50
	50·0	50·0	50·00	—	956·50
	49·0	50·0	49·50	—	956·50
12	46·5	44·0	42·25	1001·0	955·75
	44·5	44·5	45·50	1001·0	955 50
	42·0	43·0	42·50	1003·0	960·50
	43·0	43·0	43·00	1003·0	960·00
13	37·5	38·0	37·75	994·0	956·25
	35·0	39·0	37·00	993·5	956·50
	38·0	35·0	36 50	1001·0	964·50
	38·0	38·0	38·00	—	963·00
14	25·5	27·5	26·50	987·0	960·50
	25·0	26·5	25·75	987·5	961·75
	24 0	25·2	24·60	—	962·90
	25·0	25·7	25·25	—	962·25
14	79·5	78·0	78·75	1042·0	963·25
	76·0	75·0	75·50	—	966·50
	72·0	71·5	71·75	1035·2	963·45
	74·0	73·5	73·75	—	961·45
	Mean of the whole				960·00
	Correction for irradiation (see page 179)				—5·51
					954·49

By the foregoing measurements, the distance between the knife edges appears to be 39·4 inches + 954·49 divisions.

The

The pendulum was now placed on its support, and the following experiments made for equalising the number of vibrations.

Slider 18 divisions. Clock losing 0″·33 on mean time.		Great weight *above*.							Barometer 29·7.
	Temp.	Time of coincidence.	Arc of vibration.	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
	°	m. s.	°	°				s	
June 19th.	66·8	29 12	1·29						
		37 50	1·07	1·18	518	516	86066·40	2·28	86068·68
	66·8	46 30	0·93	1·00	520	518	86067·70	1·63	86069·33
								Mean	86069·00
								Clock	— 0·33
	66·8	mean							86068·67
		Great weight *below*.							
	66·6	5 59	1·25						
		14 32	1·13	1·19	513	511	86063·16	2·32	86065 48
	66·7	23 4	1·01	1·06	512	510	86062·50	1·84	86064·34
								Mean	86064·91
								Clock	— 0·33
								Temp.	— 0·04
	66·7	mean							86064·54

Here the vibrations were in excess; the slider was therefore placed at 29 divisions, and the second weight moved nearer to its knife edge.

Slider 29 divisions. Clock losing 0″·33 on mean time		Great weight *above*. Second weight moved.							Barometer 29·7.
	°	m. s.	°	°				s	
June 19th.	67·0	57 42	1·23						
		6 3	1.04	1·13	506	504	86058·49	2·08	86060·57
	66·9	14 37	0·88	0·96	509	507	86060·50	1·51	86062·01
								Mean	86061·29
								Clock	— 0·33
	66·9	mean							86060·96
		Great weight *below*.							
	67·1	24 39	1·14						
		33 5	1 01	1·07	506	504	86058 49	1·87	86060·36
	67·1	41 32	0·93	0·97	507	505	86059·16	1·51	86060·67
								Mean	86060·51
								Clock	— 0·33
								Temp	+ 0 09
	67·1	mean							86060·27

The number of vibrations being still in excess, the second weight was moved again, the slider remaining as before.

Slider 29 divisions. Clock losing 0″·33 on mean time. — Great weight *above*. Second weight moved. — Barometer 29·7.

	Temp.	Time of coincidence. m.	s.	Arc of vibration.	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours	Corr. for arc.	Vibrations in 24 hours.
June 19th.	° 67·3	51	7	° 1·21	° 1·11	503	501	86056·47	s. 2·02	86058·49
		59	30	1·02	0·95	504	502	86057·16	1·48	86058 64
		7	54	0·88	0·80	504	504	86058·49	5·05	86059·54
		16	20	0 73	0·68	507	505	86059·16	0·75	86059 91
	67·3	24	47	0·63						
									Mean	86059·14
									Clock	— 0·33
	67·3	mean								86058·81
		Great weight *below*.								
	67·4	30	8	1·14	1·09	504	502	86057·16	1·94	86059·10
		38	32	1·05	0·99	504	502	86057·16	1·60	86058·76
		46	56	0 94	0 89	506	504	86058·49	1·30	86059·79
		55	22	0·84	0·79	506	504	86058·49	1·02	86059·51
	67·4	3	48	0·75						
									Mean	86059·29
									Clock	— 0·33
									Temp.	+ 0·04
	67·4	mean								86059 00

Slider 29 divisions. Clock losing 0″·26 on mean time. — Great weight *above*. — Barometer 29 76.

	Temp.	Time of coincidence. m.	s.	Arc of vibration.	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours	Corr. for arc.	Vibrations in 24 hours.
June 20th.	° 68·7	1	17	° 1·22	° 1·13	503	501	86056·47	s. 2 08	86058·55
		9	40	1·04	0·96	503	501	86056 47	1·51	86057 98
		18	3	0·88	0·80	505	503	86057·82	1·05	86058·87
		26	28	0·73	0·68	504	502	86057·16	0 75	86057·91
	68 7	34	52	0·63						
									Mean	86058·33
									Clock	— 0·26
	68·7	mean								86058·07
		Great weight *below*.								
	68:4	22	4	1·19	1·13	504	502	86057·16	2 08	86059·24
		30	28	1 07	1·00	504	502	86057·16	1·63	86058·79
		38	52	0·94	0 99	504	502	86057·16	1 38	86058·54
		47	16	0 86	0·82	505	503	86057·82	1·10	86058·92
	68·5	55	41	0·78						
									Mean	86058·87
									Clock	— 0·26
									Temp.	— 0 09
	68 5	mean								86058 52

The number of vibrations being now in both positions sufficiently near each other, and in *defect*, the second weight was secured in its place.

Slider 23 divisions. Clock losing 0″·30 on mean time.				Great weight *above.*						Barometer 29·76
	Temp.	Time of coincidence. m.	s.	Arc of vibration.	Mean arc.	Interv in seconds.	No of vibrats	Vibrations in 24 hours.	Corr. for arc. s.	Vibrations in 24 hours.
June 20th.	68·7	48	25	1·23	1·13	504	502	86057·16	2·08	86059·24
		56	49	1·03	0·97	506	504	86058·49	1·54	86060·03
		5	15	0·91	0·82	507	505	86059·16	1·10	86060·26
		13	42	0·73	0·68	506	504	86058·49	0·75	86059·24
	68·7	22	08	0·64						
									Mean	86059·69
									Clock	— 0·30
	68.7	mean								86059·39
A				Great weight *below.*						
	68·8	31	17	1·22	1·16	504	502	86057·16	2·20	86059·36
		39	41	1·10	1·04	505	503	86057·82	1·76	86059·58
		48	6	0·99	0·94	506	504	86058·49	1·44	86059·93
		56	32	0·90	0·84	506	504	86058·49	1·15	86059·64
	68·9	4	58	0·79						
									Mean	86059·63
									Clock	— 0·30
									Temp.	+ 0·09
	68·9	mean								86059·42

Slider 23 divisions. Clock losing 0″·20 on mean time.				Great weight *above.*						Barometer 29·86
June 21st.	71·2	54	8	1·23	1·13	503	501	86056·47	2·08	86058·55
		2	31	1·03	0·96	502	500	86055·78	1·15	86056·93
		10	53	0·89	0·81	504	502	86057·16	1·07	86058·23
		19	17	0·73	0·68	504	502	86057·16	0·75	86057·91
	71·3	27	41	0·63						
									Mean	86057·90
									Clock	— 0·20
	71·3	mean								8605·70
B				Great weight *below.*						
	71·0	12	52	1·20	1·14	503	501	86056·47	2·12	86058·59
		21	15	1·08	1·02	503	501	86056·47	1·70	86058·17
		29	38	0·97	0·92	503	501	86056·47	1·38	86057·85
		31	1	0·88	0·83	504	502	86057·16	1·13	86058·29
	71·1	46	25	0·79						
									Mean	86058·22
									Clock	— 0·20
									Temp.	— 0·09
	71·1	mean								86057·93

Slider 23 divisions. Clock losing 0″·20 on mean time. — Great weight *above*. — Barometer 29·86.

	Temp.	Time of coincidence. m.	s.	Arc of vibration.	Mean arc.	Interv. in seconds.	No. of vibrats	Variations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
June 21st.	° 71·4	36	6	° 1·22	° 1·12	502	500	86055·78	s. 2·05	86057·83
		44	28	1·03	0·95	503	501	86056·47	1·48	86057·95
		52	51	0·87	0·80	503	501	86056·47	1·04	86057·51
		1	14	0·73	0·67	506	504	86058·49	0·73	86059·22
	71·4	9	40	0·62						
									Mean	86058·13
									Clock	— 0·20
	71·4	mean								86057·93
C	Great weight *below*.									
	71·5	20	24	1·19	1·13	502	500	86055·78	2·08	86057·86
		28	46	1·07	1·00	503	501	86056·47	1·63	86058·10
		37	9	0·94	0·89	503	501	86056·47	1·29	86057·76
		45	32	0·85	0·81	503	501	86056·47	1·07	86057·54
	71·6	53	55	0·78						
									Mean	86057·81
									Clock	— 0·20
									Temp	+ 0·09
	71·6	mean								86057·70

Slider 23 divisions. Clock gaining 0″ 30 on mean time. — Great weight *above*. — Barometer 29·95.

	Temp.	m.	s.	Arc of vibration.	Mean arc.	Interv. in seconds.	No. of vibrats	Variations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
June 23rd	73·0	9	8	1·22	1·12	500	498	86054·40	2·05	86056·45
		17	28	1·02	0·94	501	499	86055·09	1·44	86056·53
		25	49	0·87	0·80	501	499	86055·09	1·04	86056·13
		34	10	0·74	0·68	501	499	86055·09	0·75	86055·55
	73·1	42	31	0·63				86055·09		
									Mean	86056·24
									Clock	+ 0·30
	73·1	mean.								86056·54
D	Great weight *below*.									
	72·4	27	33	1·21	1·15	501	499	86055·09	2·16	86057·25
		35	54	1·09	1·04	501	499	86055·09	1·76	86056·85
		44	15	0·99	0·94	501	499	86055·09	1·44	86056·53
		52	36	0·89	0·84	502	500	86055·78	1·15	86056·93
	72·8	0	58	0·79						
									Mean	86056·89
									Clock	— 0·30
									Temp.	— 0·22
	72·6	mean								89056·97

The

The pendulum was now taken down to remeasure the distance between the knife edges, in order to ascertain whether or not they had suffered from use.

The pieces A, a, B and b, being applied as before, the following measurements were taken.

Distance from A to a, 329·06 divisions.
B to b, 366·97.

Date.	Readings of the Micrometer.			Divisions +39·4 in.
	A to a.	B to b.	Scale.	
June 25th.	9·7	39·0	630·0	953·66
	7·0	37·3	630·0	955·86
	10·0	36·5	630·7	955·46
The pieces made to change places.				
26th.	59·0	87·0	680·0	955·01
	59·0	84·0	680·3	956·81
	51·0	75·0	671·0	955·51
	43·0	67·7	664·5	957·16
	41·5	68·0	662·5	955·76
			Mean of the whole	955·65

Hence the distance between the knife edges is 39·4 inches +955·65 divisions of the micrometer.

[To be continued.]

LVIII. *Description of a new Apparatus for impregnating Liquids with Gases.* By A Correspondent.

To Mr. Tilloch.

Sir,—Most chemists have to lament the extreme fragility of Wolfe's apparatus; and setting aside its expense, it appears to me to be very defective, as the surface of the gas which is exposed to the absorbing fluid is very small, on account of its being suffered to pass through so small a depth of fluid in large bubbles; when, on the contrary, it ought to be divided, and to pass through as great a depth of fluid as possible. To effect this, the ends of the retort, *a* fig. 6, (Plate III.) of the long conducting tube *b*, should be closed, and perforated with a number of very small holes *. By this means almost every atom of gas is brought into contact with the fluid in ascending the long cylindrical vessel *c*; which might easily be made to turn on its axis with a small degree of eccentricity, which upon the whole would produce a much greater effect than if it had passed through half a dozen of Wolfe's bottles.

* Or perforated platina caps might be fixed on.

Might

Might not the tedious process of forming the red oxide of mercury be considerably accelerated, by sending a minutely divided stream of air through it in the same manner from a gasometer of condensation? The same holds good with cupellation, and many other chemical processes.

I would recommend to those who are near a gas establishment, to provide themselves with what may be called a gas furnace. It consists of a thin metallic tube *d*, open at the top but closed at both ends, and bent in the form of a *cornu ammonis*, with about half an inch distance between the whirls to admit a free access of air from beneath. By this means a degree of heat may be produced, which will combine the power of a table furnace with the uniformity and elegance of an Argand lamp, but without the incumbrance or the trouble of either. I am

Your humble servant,

G. D.

LIX. *On purifying Coal-Gas, and increasing the Quantity produced from a given Weight of Coals.* By Mr. G. Lowe.

To Mr. Tilloch.

Sir, — The Number for this month of your excellent Magazine having just arrived, I have read with pleasure Mr. Parker's letter on the subject of purifying coal-gas. At a time like the present, when the sources of real light are so highly taxed, and the materials of artificial light so dearly bought, we cannot be surprised that so many should be turning their attention to this excellent and too long neglected medium, in order to render the procuring of it so cheap that the humble cottage may not be debarred of its benefits; and so pure, that even the palace may show forth its excellence. It is under this consideration that I feel less surprise at finding my own plans of purifying gas anticipated in the experience of Mr. Parker: with a difference only in the method of application; for although we both agree in passing the gas through a high temperature, yet the very different methods which we have hit upon of so presenting it, will fully satisfy us both as to our originality of the same idea. Mr. Parker uses three ignited tubes without any other oxidizeable surface. I use only one, into which various oxidizeable surfaçes are introduced. The experiments which led me to this method were made in March last, in which experiments the hope of so constructing a stove as to give off both light and heat, of simplicity of construction and with purity of gas, was the desirable object which stimulated my proceedings; in the gaining of which, I am happy to say, I fully succeeded.

To give your readers a perfect idea of my stove, which might not unaptly be called a Thermophotogen, would be impossible without a plate, for which there is not now time, as I could wish this notice of Mr. Parker's paper to appear in your next number. —Suffice it to say, that the body of the stove is an upright cylinder of cast iron, standing four feet high, rather conical, being ten inches diameter inside at the top, and twelve inches at the bottom. For the sake of portableness, and to ensure against expansion, it is divided into three separate pieces. First, an ashpit eight inches deep, having a door in its side to regulate the draught: second, the part one foot eight inches high containing the door through which fuel is introduced, and opposite to which is an aperture to receive an iron cylinder or gun-barrel: and thirdly, the part in which at its side is placed the flue. The parts fit in proper order one upon another, having the joints covered by a small plinth; the top is open, through which is placed a cylindrical retort of two feet six inches long, and seven inches diameter, its flanch forming a top to the stove and covering the flue, which the difference of the diameter of the retort and that of the interior of the stove allows. It is evident, that if the body of the stove be now inclosed with a light sheet iron carcase, leaving a hot air flue all round except at the doors, the heat given off by the stove may be conveyed into apartments; at the same time that its internal heat is liberating the gas in the retort. The cap of the retort is on the simplest construction, like that of the common culinary digester, only fitted with a plug and socket by which the gas is conveyed through a cylinder containing iron turnings, &c. after which it passes through lime-water or not at pleasure. This method requires very little fuel, serves two purposes, and makes very pure gas. The scale upon which the foregoing apparatus is constructed may be said to be but small, though amply large enough for the majority of families; yet it proves sufficiently that its principle is calculated to obtain every advantage reasonably to be hoped for. First, a very great increase of gas is obtained from a given quantity of coal, in comparison to the old method, in which the essential oil, the tar, and the water of crystallization are all condensed prior to washing: but by passing through iron turnings, or any oxidizeable surfaces, the two first are nearly all converted into gas, and come even with the hydrogen of the latter, which has been liberated at the expense of the iron turnings.—It is evident, the great increase arises infinitely more from the decomposition of *the water* than of *the tar*, which I am afraid Mr. Parker's tubes alone after a short time will fail to do. Secondly, the nuisance creating sulphuretted hydrogen is perfectly decomposed, as well as the carbonic acid and ammoniacal gases. And last, though not least

in

in consideration, it materially lessens the expense in setting up, as well as in wear and tear, for it does not require the retort to be heated any thing near so hot. We must all agree with Mr. Parker, that the subject of these decompositions is worthy a strict examination, and which indeed they have had in their uncombined state by many of the first chemists of the day, but not in combination as in coal-gas. That the sulphuretted hydrogen may be decomposed by the mere matter of heat, and converted into carburetted hydrogen by passing it over ignited charcoal, is well known; and that the carbonic acid is converted into carbonic oxide by giving a portion of its oxygen to the iron, one may suppose; but how the ammoniacal gas, which, according to Thenard, is decomposed without the iron receiving *any addition*, or the volume of the gas being in the least *altered*, remains to be explained. In a future Number of your Magazine, if you should think it worthy a place, I perhaps shall be able to send you an account of the same principle of purifying, still further simplified, as applicable to horizontal retorts; in which the tube containing the iron turnings, scraps of tin, charcoal, &c. is placed within the body of the retort. We are now setting one up, but it is not in sufficient progress to describe. Pardon me the length of this hasty letter, and believe me Your well wisher,

Derby, Nov. 14, 1818. G. Lowe.

LX. *Notices respecting New Books.*

In spring last Dr. Watt of Glasgow published a Prospectus, accompanied with a specimen, of a work to be entitled "*Bibliotheca Britannica:* or a General Index to the Literature of Great Britain and Ireland, ancient and modern, with such Foreign Works as have been translated into English, or printed in the British Dominions: including also a copious selection from the writings of the most celebrated authors of all ages and nations. In Two Parts. In the first, the authors are arranged alphabetically, and of each, as far as possible, a short biographical notice is given; to which is subjoined a correct list of his works, their various editions, sizes, prices, &c., and in many instances the character of the work. In the second, the subjects are arranged alphabetically; and, under each, the works, and principal parts of works, treating of that subject are arranged in chronological order. This part also includes all the anonymous works which have appeared in this country, inserted according to their respective subjects and dates." A first part of this work is now in the press, and will be published in February. This, consisting of 35 sheets, or 280 pages, is calculated to be about

one

one-sixth of the whole, which, when completed, will form two quarto volumes nearly of the size of Johnson's Dictionary.

Mr. Joseph Conolly, Author of the Telegraphic Dictionary, and Essay on Universal Telegraphic Communication, for which he received the Gold and Silver Medals from the Society of Arts, has issued the Prospectus of a new work, to be entitled "The Telegraphist's Vade Mecum."

This work is to comprise—the English Language, with sentences alphabetically arranged under their respective final words, thereby obviating the complexity so much complained of by the most experienced officers and telegraphists of the present day; as also the Evolutionary Compass and Telegraphic Codes, calculated for the various symbols used in Europe. Any word or sentence, from an arrangement of twenty-six thousand, is given by the two-armed Semaphore, as over the Admiralty, in two exhibitions, without a stop-signal to divide the words. The new mode of working two numeral tables at every exhibition is fully explained, illustrated with plates of the changes exemplifying the different secret keys for deciphering official messages. The number of flags is twenty-four, and two pendants, being nine under the number used by any ship in the Navy. Any word or sentence, from twenty-six thousand, is given in one exhibition, on one mast, without a class flag; and no signal ever exceeds three flags and a pendant. The spelling power gives a syllable or word at one exhibition. A message, or any subject, can be extracted verbatim from this arrangement, in large portions, without the tedious operation of spelling.

This plan of extracting and deciphering messages will afford a pleasing study to the Telegraphist—a study hitherto rendered difficult through want of simplicity, scope, and method.

The new mode of working two numeral tables at every exhibition of Semaphoric signals, and a new arrangement of words and sentences, are to be also prominent features of this work.

R. Ackermann has in the press "High Quarrel with the Pope." A correspondence between the court of Rome and Baron von Wessenberg, Bishop of Constance, in which the Bishop disputes the authority of the Pope in Germany; with an account of his endeavours, and every probability of success, to effect a general reformation in the German Catholic Church. Demy 8vo.

Observations on Ackermann's Patent Moveable Axles to Four-wheeled Carriages, containing an engraved elevation of the carriage, with plans and sections, conveying accurate ideas of this superior improvement.

A com-

A complete History of Lithography, from its Origin down to the present Time, by the Inventor, Alois Senefelder; containing clear and explicit instructions in all its branches; accompanied by illustrative specimens of this art. Demy 4to. hot-pressed.

The Cabinet of Arts, being a new and universal Drawing-book, forming a complete system of drawing and painting in all its branches, etching, engraving, perspective, projection, and surveying, with all their various and appendent parts, containing the whole theory and practice of the fine arts in general, from the first elements to the most finished principles; displaying in the most familiar manner the whole rudiments of imitation, design, disposition and invention. Illustrated with upwards of 130 elegant engravings: to which is added an Appendix, containing several curious and useful miscellaneous articles. By T. Hudson (Author of the Accomplished Tutor) and J. Dougall.——This valuable work re-appears as a second edition, with additions, in which many new plates will be introduced. It will be comprised in 30 monthly numbers, each containing four plates, three plain and one in colours, and 12 pages of letter-press. No. I. will be published on the 1st of January next, and be continued monthly until completed. The whole will form two handsome quarto volumes. Directions for order and arrangement will be given in the last number.

Preparing for publication,—Observations on Inflammation of the mucous Membrane of the Respiration Organs; illustrative of the pathology and treatment of bronchial inflammation, croup, hooping-cough, measles, catarrh, and those affections resembling pulmonary consumption, &c. exemplified by cases, dissections, and coloured engravings of the morbid appearances. By Thomas Alcock, Surgeon.

LXI. *Intelligence and Miscellaneous Articles.*

PURIFYING OF COAL-GAS.

In our last we inserted a communication from Mr. S. Parker of Liverpool on this subject, and in our present number is one from Mr. G. Lowe of Derby in reference to Mr. Parker's. We now subjoin the method of purifying coal-gas, for which Mr. G. H. Palmer of Regent-street in the city of Westminster lately obtained a patent; and which in principle seems to coincide with Mr. Parker's; for we conceive the effect in Mr. Parker's process to be produced by the oxidation of the ignited iron tubes,

 through

through which he passes the gas. We have, however, no idea that the one was copied from the other.

Extract from Mr. G. Palmer's Specification.

"The gas may be made by any of the usual processes, and is to be conveyed in pipes to a condenser or refrigeratory, to deprive it of its tar, ammoniacal liquor, and condensible ingredients. From thence it is to be conveyed to one of my purifiers, which consists of a vessel of any form, and made of cast iron or any other material which will stand the action of heat. This purifier is to be kept moderately red hot while in action; to accomplish which, it may be set in the same furnace as the retorts, or heated by a separate fire (which will be governed by the nature and extent of the concern) so as to be visibly red by day-light. It must be understood that I mention this temperature as being sufficient, although a higher one will not be detrimental to the process, but will destroy the purifying vessel more rapidly.

"This purifying vessel is to be nearly filled with the fragments or refuse clippings of sheet iron, tinned iron plates, or any oxide of iron at a minimum of oxidation, such as common clay or argillaceous iron ore, or finery cinders, or black oxide of iron; and when so filled and heated the gas must pass through it, which will effect a partial decomposition of the sulphuretted hydrogen, to complete which it must pass into a box or cistern of cold water. The pipe which conveys the gas into the box or cistern should just dip into the water, and a pipe at the top of the cistern must communicate with the gasometer, into which the gas will flow perfectly pure, and can then be distributed and burnt as usual. The operation of this method of purification must be obvious to those who are acquainted with chemistry; for it will be readily observed, that the sulphuretted hydrogen contained in the gas will be decomposed, by the action of heat and the substances used, into hydrogen and sulphuric acid, whilst at the same time no sulphureous acid gas can escape the agents to which the crude gas is exposed."

ELECTRICAL EXPERIMENTS.

Bronzed tube. Take a glass tube of the height of the conductor and fix it on and in a stand. Coat about three-fourths of the upper part of the inside of it with metal (the easiest method is by inserting pewter shavings), and fix a cap and ball on the top. Then varnish one half of the outside from the top to the bottom, and, when nearly dry, apply, with a pencil, a coat of bronze (not the sulphuretted oxide of tin). Place it near the conductor, and a beautiful ramified spark will pass the whole length of the tube.

Bronzed plate. On a plate of glass twelve inches in diameter

paste

paste a circular piece of tin foil about ten inches in diameter. On the other side fix a narrow circle of tin foil to correspond with the outside of the opposite coating: cover the intermediate space with a regular coating of bronze, which is not required to be very thickly laid on; place it on a pedestal, and connect the tin foil coating with the ground and the insulated ball. Now charge it by means of a bent wire fixed on the end of the conductor and touching the centre of the bronzed coating: then move the ball to the conductor, and the whole surface will be covered with the most beautiful ramifications diverging from the centre to the circumference.

Lichtenberg figures may be formed on a paper tea-tray. They take quite a different character, but more beautiful. Perhaps they might be fixed by warming the japan. PROTEUS.

LIVE LIZARD FOUND IMBEDDED IN A SEAM OF COAL.—FOSSIL TREE.—FOSSIL PLANTS.

Wakefield, Nov. 7, 1818.

SIR,—The following particulars respecting a live lizard found imbedded in a seam of coal at Mr. Fenton's colliery about two miles from this town, may be interesting to yourself and readers.

This animal, preserved in spirits, is now in the possession of Mr. James Scholes, engineer to that colliery. It is about five inches long; its back of a drawn brown color, and appears rough and scaly; its sides of a lighter colour, and spotted with yellow; the belly yellow streaked with bands of the same colour as the back. Mr. S. related to me the following circumstances of its being found.—In August last they were sinking a new pit or shaft, and after passing through measures of stone, grey bind, blue stone, and some thin beds of coal, to the depth of 150 yards, they came upon that intended to be worked, which is about four feet thick. When they had excavated about three inches of it, one of the miners (as he supposed) struck his pick or mattock into a crevice, and shattered the coal around into small pieces: he then discovered the animal in question, and immediately carried it up to Mr. S.—It continued very brisk and lively for about ten minutes, then drooped and died. Mr. S. went down the pit to examine the part where it might be supposed to be lodged, but he could not collect any fragments to enable him to ascertain its precise bed.

It may be proper to mention, that in sinking these pits they find, in particular strata, impressions of what Mr. S. calls ferns and other vegetables; and at upwards of 100 yards from the surface, they meet with a black shale one foot thick, full of muscle-shells compressed and flattened by the superincumbent pressure.

About

About four inches above the coal in which the animal was found, numbers of muscle-shells in a fossil state lie scattered in a loose grey earth.

At a considerably higher level, and in an alluvial soil, near this town, a mass of these shells is found at twelve yards from the surface, imbedded in a stratum of black limestone about twelve inches thick, which takes a good polish. This bed or band of fossil muscles is found in all the coal mines in this neighbourhood, but generally connected with iron ore.

Mr. Scholes mentioned another circumstance worthy of record, which occurred some years ago under his own eye at the Stanley colliery two miles NE of this town. In sinking a pit to the depth of 86 yards, they came to a bed of coal 2 feet 6 inches thick, beneath which, in their further progress, they found what they supposed to be a petrified tree, or rather plant, having no branches, standing upright, but rather inclining to the east. It was six inches diameter at the top; but as they sunk down, it increased to twelve inches, and at the depth of 42 feet seemed to branch out roots to another bed of coal six feet thick. The body was a grey sandstone, coated round with a black carbonized matter one-tenth of an inch, supposed to be its bark.

Before concluding, I will take this opportunity of communicating to you another remarkable phænomenon in some measure connected with this at Stanley colliery, which is on the NW side of the river Calder. On the SE side of this river, and nearly in a parallel line, there is a hill about 200 feet above the level of the river. It consists of an argillaceous sandstone, and a few feet from the surface there are strong appearances of its having once been on fire. For many years a quarry has been worked there for procuring materials for the repair of the roads, for which purpose this burnt stone is well adapted. In this quarry many gigantic fossil plants have been found standing upright, as well as casts and impressions of vegetables unknown to these climates lying horizontally. Many specimens of these fossil reliquiæ are in the possession of Mr. Parkinson, Hoxton Square; of Mr. Watson, of Bakewell, Derbyshire; and I believe in the collection of the Geological Society. I am, Sir,

Your constant reader,

To Mr. Tilloch. W. S.

SOLUTION OF BIQUADRATIC EQUATIONS.

The following solution of the Biquadratic is founded upon Des Cartes's method of multiplying together two quadratic factors with indeterminate coefficients. A modification of his assumption presents us with two new and simple formulæ of solution, and places the true principle of his method in a clear light.

By

By exterminating the second term, every biquadratic may be reduced to the form

$$x^4 - qx^2 + rx - s.$$

This we assume equal to the product of two quadratic factors with indeterminate coefficients, α, β, γ,

$$(x^2 + \alpha x + \beta - \gamma) \times (x^2 - \alpha x + \beta + \gamma).$$

This product becomes by actual multiplication

$$x^4 - (\alpha^2 - 2\beta)x^2 + 2\alpha\gamma x + \beta^2 - \gamma^2.$$

By equating the terms of this expression with those of the original equation, we obtain three equations for ascertaining the indeterminates,

$$\alpha^2 - 2\beta = q$$
$$2\alpha\gamma = r$$
$$\beta^2 - \gamma^2 = - s$$

If we find these by means of α, we have Des Cartes's solution. If we find them by means of β, we have

$$\beta^3 + \frac{q}{2}\beta^2 + s\beta + \frac{4qs - r^2}{8} = 0$$

$$x^2 +- \sqrt{q + 2\beta}\ x + \beta -+ \frac{r}{2\sqrt{q+2\beta}} = 0$$

$$x = -+ \frac{\sqrt{q+2\beta}}{2} \pm \sqrt{\frac{q - 2\beta}{4} +- \frac{r}{2\sqrt{q+2\beta}}}.$$

Here we may observe that the quadratic formula for x, though in *appearance* a quadratic, is in reality and algebraically an equation of higher dimensions. This method of exhibiting the two quadratic factors under one form with a mere diversity of signs, shows the true principle of Des Cartes's solution, which consists in preserving the real *dimensions* of the biquadratic, while it is reduced in *form* to a quadratic. The value of x expresses all the four roots, $\pm$ being used as usual to denote that either $+$ or $-$ may be arbitrarily taken, while $+-$ and $-+$ denote that if $+$ be used in the first case, $-$ must be used in the second, and conversely.

If we find the roots by means of γ, we have

$$\gamma^6 - \frac{q^2 + 4s}{4}\gamma^4 + \frac{qr^2}{8}\gamma^2 - \frac{r^4}{8^2} = 0$$

$$x^2 +- \frac{r}{2\gamma}x + \frac{r^2 - 4q\gamma^2 -+ 8\gamma^3}{8\gamma^2} = 0$$

$$x = \frac{-+ r \pm \sqrt{+- 4\gamma^3 + 2q\gamma^2 + \frac{1}{2}r^2}}{2\gamma}.$$

which expresses the four roots in a very simple manner.

ARSENIC TAKEN WITHOUT INJURY.

The object of this communication is, to diffuse more generally the opinion, that charcoal is eminently an antidote to arsenic: from

from our knowledge of chemistry we have reason to expect it should be, but we ought not to trust to theory without some experience.

Mr. R—— took last evening, through mistake, a considerable quantity of arseniate of potash; he had previously been visited with a severe pain in the head, from uncommon exertion during the day, and had taken his supper immediately upon the top of the dose of arsenic: some suspicions were now excited, and assistance sent for, which, fortunately, was near.

Found him with a quick pulse, considerable prostration of strength, a sense of heat over the whole body, pricking in the limbs, the head-ache gone, a disagreeable dry sensation in the throat, and some degree of anxiety, as might be expected.

Gave twenty-five grains of sulphate of zinc, which produced a very little sickness; after waiting fifteen minutes, gave, at short intervals, twelve grains more, together with half an ounce of pulverized charcoal, suspended in a teacup of water: no sickness produced, but the heat and pricking were no longer felt, and the pulse became moderate.

Ordered half an ounce of charcoal and water as before; a table spoonful of which to be given every fifteen minutes: an ounce of ol. ricini, to be repeated at an interval of four hours, should not the first quantity operate; and left him for the night.

Found, this morning, that he has slept comfortably most of the night, has taken two ounces of oil, which has operated profusely and frequently; has no thirst or sickness at stomach; pulse slow and regular; tongue swoln and pale, but lively at the margin; countenance good, and he will be able to attend to his ordinary business shortly.

Conclusions.—That the charcoal was the only agent in counteracting the effects of the poison, and was the cause, together with the torpor of the stomach, of his not puking from 37 grains of white vitriol.

That the dose of vitriol retained in the system must have produced an uncommon paroxysm of thirst, had it not been for the exhibition of carbon; and therefore that all metallic oxides must be inert, when given with the medicine.

That with a view of inverting the action of the stomach, vegetable emetics, and not mineral, should be resorted to, such as oxymel of squills, ipecacuanha, apocynum androsæmifolium, lycopodium, selago, and, above all, the distilled water of ranunculus flammula, the operation of which is said by Dr. Withering (a respectable writer) to be immediate.

Note.—There are two varieties of r. flammula, but both frequent the same soil, and consequently possess the same properties. The virtues of this plant (r. f.) ought to be investigated; the

the sensible qualities are such as to deserve attention; and the name of Dr. Withering ought to be sufficient to give it a place in the materia medica.

NEW RESEARCHES ON HEAT.

MM. Dulong and Petit have lately given to the world a Memoir on Heat, which gained the prize medal for 1818, of the Academy of Sciences. The title of the paper is, "On the Measure of Temperatures, and on the Laws of the Communication of Heat."

Law 1. If the cooling of a body placed in a vacuum terminated by a medium absolutely deprived of heat, or of the power of radiating, could be observed, the velocity of cooling would decrease in a geometrical progression, whilst the temperature diminished in an arithmetical progression.

2. For the same temperature of the boundary of the vacuum in which a body is placed, the velocity of cooling for the excess of temperature, in arithmetical progression, will decrease, as the terms of geometrical progression diminished by a constant number. The ratio of this geometrical progression is the same for all bodies, and equal to 1·0077.

3. The velocity of cooling in a vacuum for the same excess of temperature increases in a geometrical progression, the temperature of the surrounding body increasing in an arithmetical progression. The ratio of the progression is also 1·0077 for all bodies.

4. The velocity of cooling due to the contact of a gas is entirely independent of the nature of the surface of bodies.

5. The velocity of cooling due to the contact of a fluid (gas) varies in a geometrical progression, the excess of temperature varying also in a geometrical progression. If the ratio of the last progression be 2, that of the first is 2·35; whatever the nature of the gas, or whatever its force of elasticity. This law may also be expressed by saying, that the quantity of heat abstracted by a gas is in all cases proportional to the excess of the temperature the body raised to the power of 1.233.

6. The cooling power of a fluid (gas) diminishes in a geometrical progression, when its tention or elasticity diminishes also in a geometrical progression. If the ratio of this second progression be 2, the ratio of the first will be for air 1·366; for hydrogen 1·301; for carbonic acid 1·341; for olefiant gas 1·415. This law may be expressed in the following manner:

The cooling power of gas is, other things being equal, proportionate to a certain power of the pressure. The exponent of this power, which depends on the nature of the gas, is for air 0·45; for hydrogen 0·315; for carbonic acid 0·517; for olefiant gas 0·501.

7. The

7. The cooling power of a gas varies with its temperature; so that, if the gas can dilate so as to preserve the same degree of elasticity, the cooling power will be found diminished by the rarefaction of the gas, just as much as it is increased by its being heated; so that ultimately it depends upon its tension alone.

It may be perceived, from the above propositions, that the law of cooling, composed of all the preceding laws, must be very complicated; it is not therefore given in common language, but may be found in a mathematical form in the body of the memoir.

PLATINA.

A very singular mass of platinum has lately been found in South America, and is now deposited in the Royal Museum at Madrid. Dn. Ignacio Hurtado is the proprietor of certain lands in the Quebrada de Apotó, in the province of Notiva, in the government of Chocó. In this Quebrada is situated his gold mine, called Condoto. One of his Negro slaves, named Justo, found this mass of platina in the year 1814 near the gold mine. Dn. Ignacio, most generously, and full of ardour for the sciences, presented this unequalled specimen to His Most Catholic Majesty, through his excellency S[or] Dn. Pablo Morillo, commander-in-chief of the Royal Spanish armies in the province of Venezuela, who transmitted the same, together with other objects of natural history, belonging to the botanical department, under the Spanish naturalist Dn. José Mutis, to Europe, through General Pascual Enrile, who brought it safely to Spain, and forwarded it to the hands of the King himself by Captain Antonio Van Halen. Being an unique specimen, his majesty gave it to the Museum. Its figure is oval, and inclining to convex. The Spaniards term it "*Pepita*," which signifies water-worn, and not *in situ*.

Its large diameter is two inches four lines and a half, and its small diameter two inches. Its height is four inches and four lines. Its weight is one pound nine ounces and a drachm. Its colour is that of native silver. Its surface is rough, and here and there spotted with yellow iron ochre. The Negro who found it suspected that it contained gold: he tried to fracture it; but he was only able to make a dent in the metal, which is, however, sufficient to show its character.

To avoid every possible doubt about the mass of platina, it should perhaps have been mentioned, that the Spanish Secretary of State, his excellency Dn. José García de Leon and Pizarro, had taken all the measures to ascertain the fact of its being genuine native platina.

COW-TREE.

M. Humboldt and his companions, in the course of their travels, heard an account of a tree which grows in the valleys of Aragua,

Aragua, the juice of which is a nourishing milk, and which, from that circumstance, has received the name of *the cow-tree.* The tree in its general aspect resembles the chrysophyllum cainito; its leaves are oblong, pointed, leathery, and alternate, marked with lateral veins projecting downwards; they are parallel, and are ten inches long. When incisions are made into the trunk, it discharges abundantly a glutinous milk, moderately thick, without any acridness, and exhaling an agreeable balsamic odour. The travellers drank considerable quantities of it without experiencing any injurious effects; its viscidity only rendering it rather unpleasant. The superintendant of the plantation assured them that the Negroes acquire flesh during the season in which the cow-tree yields the greatest quantity of milk. When this fluid is exposed to the air, perhaps in consequence of the absorption of the oxygen of the atmosphere, its surface becomes covered with membranes of a substance that appears to be of a decided animal nature, yellowish, thready, and of a cheesy consistence. These membranes, when separated from the more aqueous part of the fluid, are almost as elastic as caoutchouc; but at the same time they are as much disposed to become putrid as gelatine. The natives give the name of cheese to the coagulum, which is separated by the contact of the air; in the course of five or six days it becomes sour. The milk, kept for some time in a corked phial, had deposited a little coagulum, and still exhaled its balsamic odour. If the recent juice be mixed with cold water, the coagulum is formed in small quantity only; but the separation of the viscid membranes occurs when it is placed in contact with nitric acid. This remarkable tree seems to be peculiar to the Cordilliere du Littoral, especially from Barbula to the lake of Maracabo. There are likewise some traces of it near the village of San Mateo; and, according to the account of M. Bredmeyer, in the valley of Caucagua, three days journey to the east of the Caraccas. This naturalist has likewise described the vegetable milk of the cow-tree as possessing an agreeable flavour and an aromatic odour; the natives of Caucagua call it the milk-tree.

METHOD OF MAKING SALT IN THE GREAT LOO-CHOO ISLAND*.

Near the sea, large level fields are rolled or beat so as to have a hard surface. Over this is strewn a sort of sandy black earth, forming a coat about a quarter of an inch thick. Rakes and other implements are used to make it of a uniform thickness, but it is not pressed down. During the heat of the day, men are employed to bring water in tubs from the sea, which is sprinkled over these fields by means of a short scoop. The heat of the sun

* Extracted from Captain Hall's "Account of a Voyage of Discovery to the West Coast of Corea, and the Great Loo-choo Island."

in

in a short time evaporates the water, and the salt is left in the sand, which is scraped up and put into raised reservoirs of masonry about six feet by four, and five deep. When the receiver is full of the sand, sea water is poured on the top; and this, in its way down, carries with it the salt left by the evaporation. When it runs out below at a small hole, it is a very strong brine; this is reduced to salt by being boiled in vessels about three feet wide and one deep. The cakes resulting from this operation are an inch and a half in thickness.

SUGAR OF THE BEET-ROOT.

The endeavours that were made in France, during the war, to produce sugar from the beet-root in sufficient quantity to satisfy the demands of the population, were very successful, and it was procured of excellent quality. The peace however, by reopening the ports, and allowing the introduction of the cane-sugar, tended to paralyse that branch of agricultural industry, for which, however, some strong exertions have since been made by the philosophers of France.

The following is given as the statement of the expense and returns of the manufactory of M. Chaptal; and if there are no unstated objections to its introduction, it is difficult to account for the preference given to cane-sugar.

Forty-five French acres were sown with beet-root; the produce equalled 700,000lbs.

Charges.	*francs.*
Sowing, pulling, carriage, and expenses of the manufactory for seventy-nine days of actual work	7000
Workmen	2075
Fuel	4500
Animal charcoal	1100
Repairs, interest of capital, &c.	4000
francs	18,675
Produce.	*lbs.*
Rough sugar of the first crystallization	29,132
Sugar obtained by further processes from the molasses	10,960
Total of rough sugar	40,092

Besides which, there were 158,000 lbs. of refuse, which was excellent food for cattle, and a large quantity of exhausted molasses, which might be converted into spirit.

DISCOVERY OF HAÜYNE IN THE ISLAND OF TIREE.

Hitherto in Scotland the attention of mineralogists has been principally directed to the investigation of the structure, relative position, and mode of formation of mountain rocks. This branch of mineralogy, it must be confessed, is more generally interesting than

than any of the others. The mind delights more in tracing out those grander features and relations in the mineral kingdom, exhibited in the structure and arrangement of mountainous and alpine country, than in decyphering the minute, although very interesting, connexions observable among simple minerals. We have accurate geognostical descriptions of many extensive tracts of country in Great Britain, but the history of the simple minerals contained in the rocks of these districts is but imperfectly known. It is therefore with pleasure that we communicate to our readers the following notice, by the celebrated Professor Pictet of Geneva.

Description of a Mineral nearly resembling Haüyne, found in primitive Limestone, in the Island of Tiree, one of the Hebrides. By Professor L. A. NECKER :

Colour—Pure sky-blue, sometimes slightly greenish.
Lustre—Shining and vitreous.
Transparency—Translucent.
Fracture—Conchoidal.
Hardness—Scratches glass.
Form—Massive and in roundish grains.

Chemical Characters.—Before the blow-pipe it becomes white and opake, but does not melt. It dissolves in acids; but we could not, from the smallness of the quantity, determine if it formed with them a jelly.

Geognostic Situation.—It occurs in very minute grains in the contemporaneous masses of felspar, mica, sahlite, and augite, which are imbedded in a primitive limestone contained in gneiss.

Geographic Situation.—It occurs in the limestone or marble rocks at the farm of Balephetrich in Tiree.

Observations.—If this mineral, as we suppose to be the case, should prove to be the true Haüyne, it will be the first instance of its occurring in a primitive district, the varieties hitherto described having been met with in lavas, basaltes, and rocks thrown out by volcanoes.

AMERICAN WATER-BURNER.

An apparatus called the American Water Burner has been invented by Mr. Morcy, of New Hampshire, who, after making many experiments, and employing various combustible substances, as tar, rosin, oil, &c. to mix with the steam, has brought his apparatus to perfection. The construction is very simple: Tar is intimately mixed with steam or vapour of water, and made to issue, with a force proportional to the pressure of the steam, from a small orifice, like that in the jet of a blow-pipe,

pipe, and is there fired. The flame, although the combustible substances issue from so small an orifice, is as large as that of a common smith's forge, and is accompanied with smoke: when this flame is directed against the bricks in the back of a fire-place, they soon become heated to redness: if iron or steel filings be thrown into the flame, they burn with a sparkling brilliancy, similar to iron wire in oxygen gas.

A few experiments have been made to ascertain the effect of steam on burning bodies, and to learn whether it probably suffered decomposition when issuing, mixed with tar, from the jet of the water-burner.

If a jet of steam, issuing from a small aperture, be thrown upon burning coal, its brightness is increased, if it be held at the distance of four or five inches from the pipe through which the steam passes; but if it be held nearer, the coal is extinguished, a circular black spot first appearing where the steam is thrown upon it. The steam does not appear to be decomposed in this experiment: the increased brightness of the coal is probably occasioned by a current of atmospheric air produced by the steam.

If the wick of a common oil lamp be raised so as to give off large columns of smoke, and a jet of steam be thrown into the flame, its brightness is a little increased, and no smoke is thrown off

If spirits of turpentine be made to burn on a wick, the light produced is dull and reddish, and a large quantity of thick smoke is given off; but, if a jet of steam be thrown into the flame, its brightness is much increased; and if the experiment be carefully conducted, the smoke entirely disappears.

If vapour of spirits of turpentine be made to issue from a small orifice, and inflamed, it burns, giving off large quantities of smoke; but if a jet of steam be made to unite with the vapour, the smoke entirely disappears. The same effect takes place if the vapour of spirits of turpentine and of water be made to issue together from the same orifice: hence the disappearing of the smoke cannot be supposed to depend on a current of atmospheric air.

If the flame of a spirit-lamp be brought in contact with a jet of steam, it disappears, and is extinguished at the points of contact, precisely as when exposed to strong blasts of air.

Masses of iron of various sizes, and heated to various degrees from redness to bright whiteness, were exposed to a jet of steam: no flame appeared, as was expected, but the iron was more rapidly oxidated where the steam came in contact with it than in other parts. It is probable, if the water suffered decomposition in in this experiment, and if the hydrogen was inflamed, its flame might not be observed when contrasted with the heated iron, a body so much more luminous.

The operation of the water-burner, then, appears to be simply this:—Tar, minutely divided, and intimately mixed with steam, is inflamed; the heat of the flame, aided by the affinity for oxygen of that portion of carbon which would otherwise pass off in smoke, decomposes the water, and the carbon and oxygen unite; the hydrogen of the water, and probably of the tar, expand on all sides (and hence the flame is very large) to meet the atmospheric oxygen; water is recomposed, and passes off in steam; a degree of heat is produced, no doubt, greater than that which is produced by the combustion of the tar alone; and this heat is equal to that evolved by the combustion of a quantity of carbon which would otherwise form smoke.

The invention is ingenious, and may be found very useful in steam-boat navigation, where it has already been applied. Probably a saving of heat would be produced by condensing the products of this combustion, which might be effected to a certain degree by an apparatus of simple construction.

POLAR EXPEDITION.

We stated in our last the return of one branch of this expedition, under Captain Buchan. It has been since followed by the return also of the other ships, the *Isabella* and *Alexander*, under Captain Sir John Ross, after a fruitless attempt to penetrate through Baffin's Bay into the Northern Pacific Ocean. Captain Ross is said to have completely succeeded in exploring every part of Baffin's Bay, and, with the exception of errors in the latitudes and longitudes, of verifying the statements of that old and able navigator whose name it bears, and of ascertaining that no passage exists between the Atlantic and Pacific Oceans through Davis's Straits and Baffin's Bay, the whole being found to be surrounded by high land, extending to the north as far as lat. 77 deg. 55 min. and long. 76 deg. W.: and in the 74th degree of latitude stretching westward as far as 84 deg. W. longitude. They traced the same the whole way down to the Cape Walsingham of Davis, which they ascertained to lie in lat. 66 deg. and long. 60 deg. From hence they steered for Resolution Island, and then stood homeward. They have made many curious observations and discoveries, of which, perhaps, will not be considered as the least interesting, that of a nation being found to inhabit the Arctic regions, between the latitudes of 76 and 78 degrees, who thought that the world to the south was all ice; that generation had succeeded generation of a people who had never tasted the fruits of the earth, had no idea of a Supreme

 Being,

Being, who never had an enemy, and whose chiefs had hitherto supposed themselves monarchs of the universe. There now only remains to be discovered the termination, if it has one, of Middleton's Repulse Bay, and, a few degrees to the northward of it, to determine whether Greenland be an island, or joins America; and this might with the greatest ease be done from the northernmost station of the Hudson's Bay Company in any one season.

The following are some additional particulars, which have appeared in the public journals, respecting the discoveries made by this branch of the expedition:

The Jane, Capt. Young, of Montrose, sailed in company with the Isabella and Alexander from Lerwick, and learnt from Capt. Ross the following interesting particulars: "After the last accounts from the Expedition, up to the 25th of July, when they had reached lat. 75 deg. 21 min. and long. 60 deg. 30 min. the weather cleared, and the variation of the compass increased so fast, that it became difficult to find out exactly how the ship was steering. The sea, with the exception of some ice-bergs, being completely clear of other ice, they reached lat. 76½ deg. when they were unexpectedly opposed in their northern progress by terra firma. Here they met with a new race of Esquimaux, who, by their astonishment, appeared never to have seen a ship before. At first they were much afraid, and made signs for the vessel to fly away, thinking them huge birds of prey that had descended from the moon to destroy them. A few of the natives, however, were soon enticed on board, when they expressed their awe and wonder by hugging the masts, and other extravagant manifestations of imploration, as if to superior beings; at other times, on attentively surveying the ships, they laughed most immoderately. They were entirely unintelligible to the Esquimaux whom Capt. Ross took out with him, although they seem to be of the same origin, their physiognomy being similar, but of rather a darker complexion—in their general appearance, language, and manners approaching nearer to the natives of Kamschatka, or the north-eastern extremity of Asia. Their mode of travelling is on sledges, drawn by dogs, and some of them were seen in this way going northward. They were in possession of knives, which, it is conjectured, they must have formed from the iron in its natural state, and which may, perhaps, at some future period, become an object of commerce with the natives of these hitherto unknown regions. The weapons they used for killing the smaller species of whales were the horns of the sea unicorn. Here then, at the termination of this immense bay, which, till now, has been supposed to communicate with the Polar Basin, an entire new race of human beings has been discovered; and the idea of of reaching the Pole, or penetrating into the North Pacific Ocean by Behring's Straits, through this supposed passage, is for ever at rest. The Isabella and Alexander traversed the whole bay, having sailed up the eastern, and returned by the western, shore—thus proving that the whole of that vast and unknown country, from Cape Farewell to the Cape Walsingham of Davis, is attached to the continent of America. After all, the traditionary story, handed down among the southern Esquimaux, that there is a rapid and narrow river to the northward, which may communicate to the Polar Basin, is perhaps founded on reality. Our navigators, in their progress round this dreary bay, saw numerous whales; and due advantage will, no doubt, be taken of this valuable discovery by the fishers next season."

Extract of a letter from an officer on board the Alexander, dated Lerwick, November 3, 1818:—"We got up at the head of Baffin's bay on the 20th of August, and found it nothing but a bay, and that we were not further north at that time than 76.45. lat. and lon. 74. W. and found land all round, and quite narrow from land to land: and on the 21st of August we began to come to the southward, along the west land, which all the way down we found quite clear of ice. We found many large inlets, which we were in with the ships, but always found land all round, at the head of them. One of these sounds that we entered was in 74 deg. of N. lat. and were up till our longitude was above 80 W. and we found at the head of the bay the variation as high as 110, which is about 10 points of the compass."

The following particulars which we subjoin are from a Hull paper, but bear evident marks of fabrication about them: for instance, a savage, who had arrived at the skill and combination necessary to make a sledge, and be drawn about in it by dogs, would very easily be able to conceive that a ship need not necessarily be an animal *because* it *moves*; for his reason, however limited, must at least have told him, in the process of contriving his sledge-vehicles, that what contains a human being need not, as a matter of course, be a *stationary* machine.

"The subjoined account of the newly-discovered race of Esquimaux has been communicated by Sir John Ross. The discovery ships had been lying adjoining the land, between lat. 76. and 77. fastened to an iceberg, for two or three days, and had just shoved off, when to their great surprise they saw some persons coming down from the interior, towards the shore, in sledges drawn by dogs. Our countrymen immediately put back; but on landing, the natives fled towards the interior. In order, if possible, to open a communication with them, the Esquimaux on board the expedition set off after them; and about three miles over the ice succeeded in this object, when he found he was able to make himself imperfectly understood by them, and also to comprehend their meaning. On the first introduction taking place, the natives inquired whether their visitors came from the Sun or the Moon? The Esquimaux told them neither; but from a large country a great distance—from the south. They said this was impossible, as there was nothing to the southward but uninhabitable ice. It was with great difficulty that they could be convinced of their error, or led to regard our countrymen in any other light than as beings from some other planet. On being taken on board the vessels, they manifested the utmost surprise at every thing they saw. They could not for some time be persuaded that the ships were not animals, and possessed the power of speaking; and when told that they were of the nature of houses, intimated that could not be, since the former went backwards and forwards, while houses were stationary. They repeatedly handled the clothes of the crews, and could not conceive what sort of skins they were made of; their own covering being wholly of that description. Of bread or grain they knew not the use; and on being induced to put some of the former into their mouths, after masticating it for some time, spat it out again as tasteless. Their own food, it appears, was chiefly fish and blubber. They had never seen any timber, and were quite ignorant of its properties: so that one of them on going aboard, and seeing a mast laid on the deck, attempted to take it up in his hands, as if he conceived it to be devoid of weight. Another of them, on being taken into the cabin and shown his image in a mirror, started back with surprise, and could not, until after repeated assurances and experiments, be convinced that there was not some person behind the mirror. They appeared to have no idea of a God, or of a future state; nor do they seem, from what we can learn, to have any enemies, but suppose themselves sole monarchs of the universe." ———

STEAM ENGINES IN CORNWALL.

From Messrs. Leans' Report for October 1818, it appears that the following was the work performed during that month, by the engines reported, with each bushel of coals.

	Pounds of water lifted 1 foot high with each bushel.	*Load per square inch in cylinder.*
25 common engines averaged	22,345,707	various.
Woolf's at Wheal Vor ..	31,685,292	17·8 lib.
Ditto Wh. Abraham ..	40,975,703	16·8
Ditto ditto	21,647,880	6·7
Wheal Abraham engine ..	32,265,906	10 9
United Mines ditto ..	34,283,211	17·9
Treskirby ditto	38,059,312	11·3
Wheal Chance ditto ..	31,369,231	12·2

ROYAL

ROYAL SCOTCH THISTLE.

To Mr. Tilloch. October 6, 1818.

Dear Sir,—Visiting Dumbarton Castle in the year 1801, on taking leave of Capt. R., he presented me with a few seeds of the Royal Scotch Thistle, which grows spontaneously in the scanty soil that covers or fills the various crevices of that rock. This seed had been collected from thistle-tops of the preceding year.

After distributing a portion to some friends, I carefully reserved the remainder, folded up in the original covering, labelled and dated, till an opportunity of sowing them to advantage might occur. In the abundance of my solicitude, however, to preserve this small packet, I had laid it too carefully up, and it never again came into my hands till the 4th of June last; when searching for another object amongst the relics of some other deposits, I was agreeably surprised to find my long-lost Scotch thistle-seed make its appearance. I now set an additional value on this recovery, as I conceived it a fair opportunity of ascertaining if seeds of this genus of plants might be kept so long without losing their vegetative principle, and prepared for sowing them in an open spot of the field, rendered bare by a small quantity of manure having lain till the turf had rotted under it. The seed of the Royal Scotch Thistle was accordingly dropt into the earth on the evening of the royal birth-day. The spot was soon covered with new vegetation; notwithstanding of which, without further culture of either weeding or hoeing, one plant of this royal thistle, from seed 18 years old, in the short space of four months from the time the dry seed was put into the ground, has attained a size and luxuriance perhaps altogether unprecedented in these latitudes. It presents a foliage resplendently beautiful, and so close and rich in the defensive, as most aptly to remind all who come near it of the very appropriate motto, "*Nemo me impune lacessit.*" It completely covers an area in circumference exceeding 18 feet, rising in the centre to 30 inches without a flower shoot, and then spreading in a regular circle round the bottom, extending its gosky leaves in close order along the ground, bearing down every other inferior weed, and protruding its thorny points beyond three feet in every direction from the parent stem. Should the weather continue mild, and this plant survive the rude fury of the stormy winter's blast, if we may judge from its present vigour and strength of vegetation, the deepness of its beautiful green, intermixed with its shining silvery veins, it may be expected, when in flower, to be one of the greatest and most exuberant native botanical curiosities ever seen in this country. This shows that seeds kept dry, and free from

 the

the entrenching tusk and destructive grinders of the *mite*, will long retain their living principle, and propagate their kind.

I am, &c. GAVIN INGLIS.

Extract from another letter from Mr. Inglis, dated 30th October: "The thistle still continues to thrive, and has attained a most incredible size, such as to draw expressions of admiration from all (gardeners included) who see it. I certainly never saw any thing like it of its kind for size, independent of its nonage. I will do all in my power to preserve it during the winter."

NEW VEGETABLE ALKALI.

MM. Pelletier and Caventou have inserted the following note in the Annales de Chimie for July. (The note was read to the Academy on the 10th August.)

Whilst analysing the vomica nut, and the bean of St. Ignace, they extracted from these two seeds the substance of which they owe their action on the animal economy.

This substance is white, crystalline, and very bitter. It crystallizes in the form of quadrangular plates, or in four-sided prisms, terminated by an obtuse quadrangular pyramid. It is very slightly soluble in water, but very soluble in alcohol. It is formed, like most vegetable substances, of oxygen, hydrogen, and charcoal. It is most distinguished by its alkaline properties; and though like morphium, is essentially different from it. It restores a reddened blue colour, and with acids forms neutral salts, soluble in water, and crystallizable. With weak nitric acid it forms a nitrate, but the concentrated acid acts on and decomposes it; and forms a solution, at first red, but becoming yellow, and yielding oxalic acid. Its acetate is very soluble, the sulphate less so, and crystallizable in rhomboidal plates.

This substance acts on animals in a similar manner to the alcoholic infusion of the nux vomica, but more energetically.

The class of acid vegetable substances is numerous; on the contrary, that of alkaline vegetable substances is confined to morphium. Nevertheless, M. Vauquelin has noticed the alkaline properties of a substance obtained by him whilst analysing the *daphne alpina*. The new body will form another genus in the class, which may become numerous, and which has first been observed by M. Vauquelin. To recall these facts, and designate the substances by a name which will avoid circumlocution, they have called it *vauqueline*. This name is better than one entirely insignificant, or that indicates properties which may be found in other bodies.

EFFECTS OF HOT WATER ON FLOWERS.

The following fact is deserving of record, as an interesting contribution to what has hitherto been discovered on the subject of vegetable

vegetable physiology, and as enabling the lovers of flowers to prolong, for a day, the enjoyment of their short-lived beauty. Most flowers begin to droop and fade, after being kept during twenty-four hours in water; a few may be revived by substituting fresh water; but all (the most fugacious, such as the poppy, and perhaps one or two others, excepted) may be completely restored by the use of *hot* water. For this purpose place the flowers in scalding water, deep enough to cover about one-third of the length of the stem; by the time the water has become cold, the flowers will have become erect and fresh; then cut off the coddled end of the stems, and put them into cold water.

MAGNETISM APPLIED AS A TEST FOR IRON.

The third Number of the New Series of the Annales des Mines contains a paper of M. Haüy on the means of detecting iron in mineral or other substances by magnetism. Its presence is ascertained by the attraction of the substance, either immediately or after having been heated in the flame of a taper, on the magnetic needle; but in order to make the effect more sensible and evident, M. Haüy has taken advantage of the combined forces of the magnetism of the earth and of a bar magnet acting simultaneously on the needle.

The needle should be of excellent steel, and highly magnetic, its cap should be made of agate or rock crystal, and the point on which it moves very fine. If such a needle be left to itself it will stand parallel to the magnetic meridian, in consequence of the forces exerted on it by the magnetic poles of the earth; and if from any cause the needle is deflected from this line, the force exerted upon it to bring it back to its first position will be as the sine of the angle which the needle makes with the magnetic meridian. The power, therefore, which tends to return the needle to its first position, increases until it has passed through a quarter of a circle, and then decreases again; but the increase is in a decreasing ratio, and the decrease is in the increasing ratio; and supposing the needle to be moved through 90° by nine successive additions of force, it would require the greatest addition to move it from 0 to 10°, and the least to move it from 80 to 90°; and then if the power which moved it from 80 to 90° carry it over that point, it would be more than sufficient to make it traverse the next quadrant, because the forces exerted by the earth's magnetism continually decrease.

In order to take advantage of this circumstance, let the needle be influenced only by the earth's magnetism, it will stand parallel to the magnetic meridian; then if the south pole of a bar magnet be approached towards the similar pole of the needle, the bar being in a line with the needle, a repulsion will take place,

place, and the needle will deviate until the repelling power of the bar, and the attractice force of the earth on it, are equal to each other. This may have set the needle at an angle of 30° with its meridian; then, by approximating the bar, the effect belonging to it will be increased, and the direction of the needle will become more oblique. By adjusting the bar in this way, the needle may be placed and retained in a position very nearly at right angles with its first direction; and then, as the power of the earth on the needle increases but very little from the 80° or 85° to 90°, and afterwards diminishes, a small force will make it pass the 90°, and once beyond that point it will continue to move until its position is completely reversed.

The time of applying a mineral or other substance supposed to contain iron to the needle, is when it is nearly at right angles with the magnetic meridian; because an effect will be produced there by a force many times smaller than that necessary to produce a similar effect on a needle uninfluenced, except by the earth's magnetism.

M. Haüy found that in this way effects were produced on the needle by bodies that, in common circumstances, appeared to have no action, as hæmatite, the carbonate, phosphate, chromate, and arseniate of iron, ferriferous carbonate of lime, garnet, peridot, &c.; and he observes that this extension of character, by means of *double magnetism,* may be usefully employed in the description of ferriferous minerals.

EARTHQUAKE.

Inverness, Nov. 12t.

A smart shock of an earthquake was felt here, and to a considerable distance round the town, at about twenty minutes pa twelve on Tuesday night. It was felt with peculiar violence along the banks of Loch Ness. The noise seems to have been greater or less, according to local circumstances: in some places it was very loud, in others more gentle, but every where a tremulous motion which was for a few seconds communicated to moveables. The night was uncommonly serene, with clear moonlight; and while not a breath of wind was stirring on the surface of the ground, in the higher regions of the atmosphere the clouds were driving rapidly from south to north. Lighter shocks are said to have been felt at nine in the evening of Tuesday, and at four in the morning of Wednesday.

Extract of a letter, dated November 11: "We were visited this morning, at 25 minutes before one, with a dreadful earthquake: being at the time sitting reading, about twelve o'clock, I felt the house shake slightly, with a little noise below, which put me in a little confusion, as I was well aware that every person in the house was in bed except myself. On feeling this I retired

retired immediately to bed, being well convinced of its cause. I expected another shock, and my expectations were very soon realized; for in half an hour after another terrible shock shifted my head on the pillow several inches. The noise resembled thunder, or the tumbling of all the furniture in the house; all our bells were set a-ringing. As I was perfectly collected at the time, I endeavoured to ascertain its duration, and counted one, two, three deliberately, while I felt the motion, which I suppose would occupy about two or three seconds, during which the atmosphere was overspread with white clouds, all meeting each other in a common centre, southward of this place."

LIST OF PATENTS FOR NEW INVENTIONS.

To Charles Watt, of Ratcliff-Highway, Middlesex, surgeon, for means of gilding and preparing quills and pens by manual labour and chemical operations so as to render them more durable and useful.—Two months allowed for specification.—Dated 31st October 1818.

To Nicholas Desforges, of Bucklersbury, London, merchant, for certain improvements in propelling boats and other vessels. —31st Oct.—6 months.

To John Bogaerts, of Air-street, Piccadilly, in consequence of a communication made to him by John Groetares, now residing at Brussels, for a method or methods for raising and lowering water, or canal locks.—12th Nov.—6 months.

To Edward Woolley, of Bilston, Staffordshire, screw manufacturer, for improvements in the machinery for making wood screw forgings.—12th Nov.—6 months.

To James Ingledew, of Little College-street, Westminster, licensed victualler, for means of effecting a saving in the consumption of the ordinary articles of fuel by the application of certain well known materials hitherto unused for that purpose.— 12th Nov.—6 months.

To Moses Poole, of Lincoln's Inn, in consequence of a communication made to him by Christopher Dihl, a foreigner residing abroad, for an invention of the application of known mastics or cements to various purposes, such as modelling statues, making slabs, raising or impressing figures or other ornamental appearances; also the covering of houses, and in any other manner in which mastic or cement may or can be applied.—12th Nov.— 6 months.

To John Grafton, late of London, and now of Edinburgh, engineer, for an improved process or method of making carburetted hydrogen gas for the purpose of illumination.—12th Nov.—6 mo.

To James Hadden junior of Aberdeen, woollen manufacturer, for improvement in preparing, roving and spinning of wool.— 12th Nov.—6 months.

To

To George James Clark, of Bath, working cutler, for an apparatus for the more easily applying the drag to a carriage wheel. —12th Nov.—6 months.

To William Styles, of Islington, carpenter, for certain improvements in machinery, for sifting cinders and discharging the cinders so sifted into a convenient receptacle, which machinery is applicable to other useful purposes.—12th Nov.—2 months.

To James Fraser, of Long-Acre, county of Middlesex, coppersmith and engineer, for his new and original invention of tunnels in a steam-boiler; also new flues in the said steam-boiler, or the furnace connected with its erection, the said steam-boiler to be for the purposes of lessening the consumption of fuel, the appearance of smoke, and the trouble of attendance.—12th Nov.—2 months.

To Richard Wright, of Tokenhouse-yard, London, engineer, for certain improvements in the construction of steam-engines, and the subsequent use of the steam.—14th Nov.—6 months.

To Henry Matthews, of Gretton-place East, Bethnal Green, Middlesex, for certain improvements applicable to wheeled carriages or vehicles of different descriptions, calculated to render them more safe and commodious.—19th Nov.—2 months.

To George Clymer, late of Philadelphia, now of Cornhill, London, mechanic, for certain improvements on ships' pumps.—21st Nov.—6 months.

To John Chancellor, of Sackville-street, Dublin, watchmaker, for an improvement in turning the leaves of music books in a simple and effectual manner with or without pedal-work attached. —21st Nov.—6 months.

To Elisha Hayden Collier, late of Boston in the State of Massachusetts, now of Charter House-square, London, gentleman, in consequence of a communication made to him by a certain foreigner now residing abroad, together with certain additions of his own invention, for an improvement in fire-arms of various descriptions, which improvements are also applicable to cannon.—24th Nov.—6 months.

Meteorological Journal kept at Walthamstow, Essex, from October 15 *to November* 15, 1818.

[Usually between the Hours of Seven and Nine A.M. and the Thermometer (a second time) between Twelve and Two P.M.]

Date.	Therm.	Barom.	Wind.
October			
15	54 66	29·91	SE.—Red sunrise; fine morn; some slight showers after 8 A.M.; clouds and sun; fine day;

Date.	Therm.	Barom.	Wind.
			day; very warm; slight rain early in the evening; *cirrostratus*, and very hot.
16	53 67	29·90	SE—E—SE.—Clear, and beautiful *cirrostratus*; fine day; hot; at 4½ P.M. remarkable dark *nimbus*; at 9 P.M. moon through clouds; moon in a *cirrus* at 11 P.M. and bright star-light.
17	53 67	30·00	NW—SE.—Foggy morn; fine day; hot; beautiful *cirrus*; moon-light, and *cirrostratus*.
18	56 60	29·90	SE—E—SE.—Fine clear morn; very white dew; after 9 A.M. cloudy and dark; fine day; some sunshine; moon, stars, and *cirrocumuli*.
19	60 61	29·95	NW—E—SE.—Very hazy morn; fine day; dark *nimbus* and moon beams through it.
20	51 60	29·95	SE.—Gray morn; *very* fine day; star-light at 7 P.M.; 9½ P.M. no stars visible.
21	37 54	30·12	E—SE.—Fine sunshine; white dew; *very* fine day; very dark night.
22	48 63	30·00	E.—Very fine morn; very fine day; fine bright star-light. Moon last quarter.
23	42 54	29·90	SE—NE.—Gray morn; fine gray day; dark night.
24	45 50	30·00	NE.—Gray morn, and wind: gray day; and a little sun about 4 P.M.; small rain after dark; very red sunset.
25	49 59	30·00	NW—SE.—Sun, and *cumuli;* fine day; star-light night.
26	49 62	30·00	SE.—Gray morn; fine *hot* day; red sunset; bright star-light night.
27	46 62	30·10	SE.—Fine sunrise, and white dew and *cirrostratus; very* fine day; star-light.
28	45 60	30·10	SE—S—SW.—Hazy and sun, and strong dew; calm; quite foggy about 10 A.M.; fine day, and some wind; star-light.
29	51 60	30·20	NE—SW.—Hazy, and *cirrostratus;* very fine day; dark night. New moon.
30	47 59	30·25	NW—SE.—Fine; clear and *cirrostratus*; fine day; wind and *cirrostratus*; dark night.
31	52 56	30·00	S.—Hazy; rain began about 9 A.M., and continued till near 2 P.M.; sun through clouds at 5 P.M.; dark night.

November

Date.	Therm.	Barom.	Wind.
November			
1	47 57	29·90	SW—NW—SW. — Hazy and *cirrostratus;* fine day; sun and clouds; bright star-light at 8 P.M.; very dark afterwards.
2	49 57	29·85	S—SW.—*Cirrostratus* and clear; hazy low; fine day; some rain after 4 P.M.; star-light.
3	49 59	29·70	SE.— Clear and *cirrostratus;* very fine day; sun and wind; star-light.
4	47 55	29·50	E—SE. — Fine morning; clear and *cirrostratus;* rain from about 11 A.M. to about 2 P.M.; dark night.
5	52 58	29·40	E—SE.—Fine red sun-rise; 7½ A.M. rain; fine day; *cirrostratus* and wind; cloudy night. Moon first quarter.
6	52 55	29·30	E—SE.—Damp morn, and cloudy; fine day; slight rain at 7 P.M.; cloudy night.
7	50 55	29·60	S—SW.—Fine morn; clear and *cirrostratus;* fine day; windy; fine moon and star-light.
8	46 54	29·80	NW.—Sun and hazy; very fine day; moon and hazy.
9	48 52	29·85	NW.—Very hazy morn; fine day; hazy night.
10	45 51	29·90	NW—NE. — Hazy low; clear and *cumuli* high; rain at 8 A.M. till about 9 P.M.; *cirrostratus*; *cirrostratus* and wind.
11	47 53	29·70	E.—Rain and wind; damp, dark day, and frequent rain; *light* but cloudy.
12	42 48	29·62	E.— Red before sun-rise; *cirrocumuli* and wind; *very fine* day; windy; moon and *cirrostratus.* Full moon.
13	47 56	29·50	SE.—Very fine; clear and *cirrostratus;* remarkable fine day; bright moon and star-light.
14	51 58	29·60	SE.—Clear and clouds; sun and showers and wind; rainy and windy.
15	48 50	29·40	SW—NW.— Clear and clouds; rain began after 11 A.M. till about 2 P.M. fine afterwards; moonlight and *cirrostratus.*

*** Omitted in last month's Magazine:

October 3 Barometer 29·55.

——— 4 ——— 29·55.

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

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

1818	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Oct. 15	15	65°	30·00	Cloudy
16	16	62°	30·05	Rain
17	17	63·5	30·17	Cloudy
18	18	62°	30·04	Ditto
19	19	58°	30·05	Ditto
20	20	60°	30·16	Fine
21	21	55°	30·26	Ditto
22	22	52°	30·10	Cloudy
23	23	51°	30·15	Ditto
24	24	50°	30·17	Ditto
25	25	56°	30·20	Ditto—rain A. M.
26	26	58·5	30·20	Fine
27	27	60°	30·25	Ditto
28	28	61°	30·20	Cloudy
29	new	60°	30·37	Ditto
30	30	59°	30·30	Ditto
31	31	56·5	30·00	Rain
Nov. 1	1	54.5	30·04	Cloudy
2	2	57°	29·87	Ditto
3	3	58°	29·76	Ditto
4	4	56°	29·55	Ditto
5	5	56°	29·53	Rain
6	6	57°	29·54	Ditto
7	7	53°	29·78	Cloudy
8	8	48·5	30·00	Ditto—heavy mist
9	9	51°	30·07	Fine
10	10	50°	30·04	Rain
11	11	48·5	29·90	Ditto
12	full	50°	29·75	Fine
13	13	55°	29·72	Cloudy
14	14	57°	29·63	Fine

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For November 1818.

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.
Oct. 27	55	63	52	30·14	39	Fair
28	55	63	55	·17	36	Fair
29	55	60	50	·25	28	Cloudy
30	51	55	50	·15	27	Cloudy
31	55	55	54	·02	0	Rain
Nov. 1	54	56	55	29·98	25	Fair
2	53	56	55	·82	27	Fair
3	55	57	54	·71	27	Fair
4	52	56	55	·45	0	Rain
5	55	60	55	·35	16	Fair
6	55	58	55	·40	17	Cloudy
7	55	62	49	·68	27	Fair
8	45	55	45	·90	25	Cloudy
9	44	54	48	·95	20	Cloudy
10	48	51	50	·90	16	Cloudy
11	49	55	50	·78	0	Rain
12	45	50	52	·61	19	Cloudy
13	50	57	55	·56	26	Fair
14	55	55	54	·45	0	Rain
15	52	55	45	·40	0	Rain
16	50	57	47	·55	0	Rain
17	45	52	45	·88	29	Fair
18	40	50	47	30·05	26	Fair
19	45	54	40	·04	20	Cloudy
20	41	46	39	29·85	25	Fair
21	41	45	39	·76	20	Fair
22	38	44	37	·75	20	Fair
23	44	55	50	·55	20	Fair
24	50	54	44	·77	0	Rain
25	39	50	49	30·14	24	Fair
26	51	53	56	·12	0	Rain

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

LXII. *On the Question "Whether Music is necessary to the Orator,—to what Extent, and how most readily attainable?"* *By* HENRY UPINGTON, *Esq.*

[Continued from p. 250.]

To Mr. Tilloch.

Blair's Hill, Cork, Nov. 10, 1818.

SIR, — MY last letter, which treated partially of *time*, having found insertion in your Magazine for October, the continuation of this topic must necessarily follow:

Examination of THE SPEAKER *continued.*

OF TIME, continued.

Observation 4th.—The average duration of the far greater number of his long-voweled syllables compared with the average duration of his well-articulated short-voweled syllables, appeared in ratio to each other as about *three* to *two;* while the longest class when under peculiar emphasis appeared in *similar* ratio [about three to two] compared with the ordinary long ones, and consequently in ratio somewhat more than as two to one compared with the ordinary short ones. The particles *a, the, of, to,* with other equally inarticulate syllables unfit for oratory, such as the first syllable of *above, approve, oppose,* or the second syllable of *general, peaceable,* &c. were much shorter than all, and may be estimated in ratio to the ordinary short ones, considerably less than as two to three, and in ratio to the longest syllables pretty nearly as two to eight, or about four to one.

The following rude time-table will exhibit the comparative proportions with sufficient accuracy for our purpose.

Longest, under peculiar emphasis,	as *thrones*	= 12
Average long-voweled	as *dame*	= 8
Average short-voweled	as *dam*	= 5
Shortest, *inarticulate,* as the articles *a, the,* &c.		= 3

Or, in musical characters less nearly thus; taking the quaver as the standard for the ordinary short syllable .. ..

= 4
= 3
= 2
= 1

[An accidental extremely prolonged *exclamation* does not come under our cognisance—nor have I intended in this table to represent the comparative length between the longest syllable in slow time and the shortest syllable in *quick.* I mention this circumstance for the guidance of the classical reader who may possibly be unacquainted with the nature of a time-table.]

Observation 5th.—The disproportion of syllables called *long*

when

when compared with each other, as well as the disproportion of syllables called *short*, when similarly compared, was so great, that all our attempts at more minute analysis than that exhibited in the foregoing table terminated in disappointment: Neither could we pretend to lay down any tolerable set of rules for the distinguishment of long syllables from short, every gradation from our inarticulate article *a* to our longest syllable being constantly discoverable in our language. The *doubtful* syllables are incredibly numerous.

Remarks.—Nothing but the preservation of our native language from the incroaching barbarism of the day, could have warranted the over-minute observations on *time* with which I have so long trespassed on the patience of my readers; I must therefore hasten to a conclusion, beginning with the comparative lengths of our syllables.

How widely different in this respect are the taste and judgement of *Handel* from those of Joshua Steele, and the disseminators of his new-fangled prosody! Our immortal composer, for such may Handel without exaggeration be called, has for the most part limited his numerical relations, even in *recitative*, to the ratio of *two* to *one*, the crotchet = 4 being the general standard of his long syllables, and the quaver the standard of his short. The occasional increase and diminution of this ordinary standard were, in compliance with modern usage and the character of modern language, indispensable, and were introduced accordingly—the ratio of *four* to one being, with very few exceptions, the maximum. Let us open his *Messiah;* and following him throughout the whole of that dignified passage "*The voice of him that cryeth in the wilderness,*" we shall find, with the exception of the second syllable of "*wilderness,*" which for musical effect is written with a *semiquaver*, and with the exception of the word "*Lord,*" which for similar effect (although decidedly unwarrantable in speech) is written with a *minim*, that the crotchet and quaver equivalent to four to two, or two to one, are uniformly preserved. Or let us turn to that celebrated passage of his *Athalia*, in which as a highly impassioned and more theatrical subject he has introduced a more diversified series—and we shall discover, even in this instance, a reasonable limitation throughout, *four* to *one* being the utmost extent of his proportions.

Messiah.

2 4 2 4 2 2 2 2 2 3 1 2 2 4 2 2 4
"The voice of him that cryeth in the wilderness—prepare ye the way

2 2 8 4 4 2 2 2 2 2 4 4 2 2 4
of the Lord, make straight in the desert a high way for our God."

[I have passed over without observation the dotted quaver = 3 assigned to the first syllable of the word *wilderness*, this being the

the necessary preparative for the semiquaver assigned to *der*, followed by the quaver assigned to *ness*. Notwithstanding the general slowness of the whole movement, the recitative performer is scarcely enabled to articulate audibly the semiquaver *der:* in the quicker movement of speech it would be almost if not quite impossible.]

Athalia.

2 2 2 2 2 2 2 2 2 4
"But as the young barbarian I caress'd

1 1 1 2 2 3 1 2 2 4
He plung'd a dagger deep within my breast;

2 2 2 4 1 1 1 4
No effort could the blow repel,

2 4 2 2 2 4 2 4
I shriek'd, I fainted, and I fell."

[By an attentive perusal of these latter lines we shall find them much more exceptionable in the relative proportions of the syllables (when applied to *language*) than represented by Dr. Burney. Why should the unimportant particle *the* in the first line be twice as long as the important word *plunged* in the second, or as the equally important word *blow* in the third? These, though the most prominent, are not the only *oratorical* objections to this highly musical passage.]

Is it necessary to add another example? Let us take our own celebrated national air "God save the King," and scepticism itself must acknowledge that the ratio of even *three* to one, much less of *eight* to one as suggested by the originator of our speech-barring system, is amply sufficient for the production of superior melody even in *song*.

God save the King.

4 4 4 6 2 4 4 4 4 6 2 4
"God save great George our king, long live our noble king,

4 4 4 12
God save our king;

4 4 4 6 2 4 4 4 4 6 2 4 4 2,2 2,2 6 2 4
Send him victorious, happy and glorious, long to reign over us

4 4 4 12
God bless our king."

[The dotted minim = 12 assigned to the word "*king*" at the close of the first and second part, though consonant with the usage of song, cannot (as every person is informed) be tolerated in speech; and may it not therefore with justice be asserted that the ratio of *three* to *one*, which otherwise prevails throughout this beautiful production, is even superabundant for the melody of an *articulate language?* The Greek and Roman languages, which acknowledged neither inarticulate particles nor inarticulate syllables, did probably sometimes attain this ratio; for although judiciously founded on the general principle of *two* to *one*, yet the shortest syllable when most unemphatically delivered could not, to all appearance, have exceeded the one-third of the longest syllable when influenced by particular emphasis.]

There is another, and in my opinion a still more accurate method of analysing our song, with reference to the subject of *quantity*—which is, the taking into account not the *variations* which any individual syllable may undergo (such variations being equivalent to the simple slide or circumflex in language, as thus: , thus , or thus ), but the actual duration allotted in the aggregate to every such syllable. For curiosity's sake let us subject that chaste and admirable song "Hope thou nurse" to this mode of analysis, and it will be seen that the ratio of *two* to one, and no more, is simply and uninterruptedly observed.

Hope thou Nurse.

8 4 8 4 8 4 8 4 8 4 8 4 8 4 8
"Hope thou nurse of young desire—fairy promiser of joy |
8 4 8 4 8 4 8 4 8 4 8 4 8 4 8
Painted vapour glow-worm fire—temp'rate sweet that ne'er can cloy." |

What wonderful simplicity, and yet how beautiful! Scanned after the manner of *Dionysius*, it consists of alternate Cretics and Amphibrachs *only*; and is consequently much less diversified in its arrangement than our ancient Hexameter, as will immediately appear by the analysis of the latter. Will our speech-barring reformers at length be satisfied? or must we at their suggestion subvert the decorous usages of our country; and in the delivery of our language surpass even the composers of our recitative and song?

Fortunately for the elocution of these countries, our collegiate education, which embraces the languages of antiquity, inspires the student with deserved veneration for the poetic and oratorical productions of Greece and Rome,—and from these sources he cannot fail, after a little application, to derive the most signal advantage; for by habituating himself to the recitation—first of select *Hexameter* passages, and afterwards of select passages from the orations of Demosthenes*, (a certain rude observance of or rather *attempt at accent*, and a more particular observance of *quantity* being regarded,) he will sensibly and even speedily acquire

* Our present method of reading the *Latin* language would render the orations of *Cicero* ineligible. We uniformily assign to the Latin *position* vowels a short quantity, regardless of their lengths by nature. The same objection indeed will hold good in *Greek* with regard to the ἄλφα and ἰῶτα; but

quire an incredible command of organ, and a justness of taste which will amply repay him for his trouble, by enabling him subsequently to excel in the delivery of his native tongue.

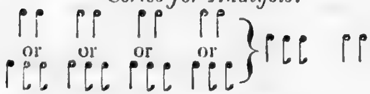
The necessary process for this attainment, or at least that simple process which I successfully adopted in my experiments on the SPEAKER, will appear in my ensuing letter; and in the mean time it may not be impertinent to present the inquiring reader with a statement of those combinations to which the recital—first of Hexameter and afterwards of ancient prose, must as it were intuitively familiarize his ear.

To begin then with *Hexameter*. The reader must not imagine, in common with Mr. Sheridan*, and others who have superficially viewed the subject, that its combinations are simply and solely confined to the Dactyl and Spondee. This *order* of feet has certainly been prescribed; but, as well may they insist that in "God save the King" no other combinations can be acknowledged than those of the Molossus  and imperfect Cretic ; whereas the Trochee , Iambus , Spondee , Amphibrach , and Bacchic , are equally the characteristics of this song. For the more perfect elucidation therefore of our Hexameter, let us analyse its properties in the following series, without any further reference to "Hope thou Nurse" or even "God save the King," which are more limited than Hexameter in their combinations.

HEXAMETER *combinations in dissyllable and trisyllable feet.*

[The *various* combinations of any given series obtrude themselves more constantly and prominently on the ear in speech than in song; every trifling pause which sense or perspicuity requires, producing, by the disjunction of the members of the series, a new and independent effect on the immediately succeeding syllables.]

Series for Analysis.



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but this imperfection appears for the most part too trivial in the delivery of Grecian *prose* (not poetry) to justify, in these instances, any material departure from our habits: By giving such due length as common sense and our newly-acquired taste shall authorize, to the ἦτα, ωμέγα, diphthongs, and circumflexed syllables—and sounding *every* syllable *fully*, which is certainly no violation of our present usage, the orations of Demosthenes will be found, with respect to *quantity*, as perfect as the best-formed ear can reasonably desire.

* Mr. Mason, Mr. Sheridan, and several others who have written on prosody, entertained a most curious notion of time or quantity. Melody and a *thump* of the *bow*, were with these gentlemen synonymous.

Contents.

Pyrrhic ♪♪

Trochee .. .. ♩♪	Iambus .. .. ♪♩
Dactyl .. .. ♩♪♪	Spondee .. .. ♩♩
Anapæst .. .. ♪♪♩	Amphibrach .. ♪♩♪
Molossus .. .. ♩♩♩	Tribrach ♪♪♪ never
Cretic ♩♪♩ .. never	Bacchic .. .. ♪♩♩

Antibacchic ♩♩♪,

to which may be added the following polysyllabical feet:— 2d Pæon ♪♩♪♪; 3d Pæon ♪♪♩♪; Choriambus ♩♪♪♩; Ionicus à majore ♩♩♪♪; Ionicus à minore ♪♪♩♩; 1st Epitrite ♪♩♩♩; 4th Epitrite ♩♩♩♪, and Dispondæus ♩♩♩♩.

Now, of all the trisyllabical feet, as appears by the foregoing table, the Cretic and the Tribrach alone are denied admission;—the latter, besides its *triple-time* character, possessing too much lightness * for the sober dignity of an epic poem, and the former, though in itself perhaps sufficiently dignified, being too irregular, in consequence of its prominently *quintuple* character, for an alliance with the more equable dactyl and the spondee: Nevertheless, in the *recitation* of Hexameter, the unavoidable irregularity of language must occasionally present the Cretic to our ear—not the perfect Cretic indeed, whose proportions are as 4, 2, 4, but the *im*perfect Cretic — ⏓ — the result of an irregular Molossus; or certain imperfect Cretics in some degree analogous to the triple-time movements ♩. ♪ ♩ ♩ ♪ ♩., and which by prosodial notation may be represented thus — ⏑ ⏓ ⏓ ⏑ —, the necessary consequence of irregular Dactyls and irregular Anapæsts.

Having thus rescued the Hexameter from the unfounded accusations of uninformed grammarians, I shall now offer to my reader a complete *musical* analysis of the different combinations which characterize the prose of antiquity when delivered as it ought; considering myself, as I already observed, too incompetent to decide whether and how far these combinations are or may be introduced into our native tongue.

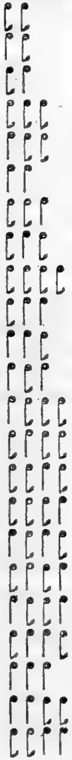
Universal Table of Combinations in the Greek and Roman Languages in various species of Poetry as well as in PROSE.

[The short syllable or *quaver* in the following table is repre-

* In the penultimate bar of "God save the King" the tribrach or triplet ♪♪♪ which by way of *grace* has been substituted in our present copies for the original crotchet, and assigned to the word "*God*," has much contributed to the degradation of the piece. The composer was evidently a chaster musician than his pretended embellisher.

sented,

sented, in conformity with modern usage, by the denominator 8. For simplicity's sake, all the characters are reduced to this term, $\frac{4}{8}$ being considered equivalent to $\frac{2}{4}$, $\frac{6}{8}$ to $\frac{3}{4}$ and $\frac{8}{8}$ to $\frac{4}{4}$. The relative duration of the notes, as already mentioned, was by no means so accurately observed even in poetry as in song.]



Pyrrhic	An imperfect bar.
Trochee	
Iambus	Bars of $\frac{3}{8}$.
Tribrach*	
Dactyl	
Spondee	
Anapæst	Bars of $\frac{4}{8}$.
Amphibrach ..	
Proceleusmaticus ..	
Bacchic	
Antibacchic ..	
Cretic	
Pæon .. first ..	Bars of $\frac{5}{8}$.
second ..	
third ..	
fourth ..	
Dichoræus	
Diiambus	
Choriambus ..	Bars of $\frac{6}{8}$ resolvable into $\frac{3}{8}$.
Antipastus	
Molossus	
Ionicus à majore ..	Bars of $\frac{6}{8}$ not resolvable.
à minore ..	

* The character of the ancient *tribrach* should not be too hastily confounded with that of our modern *triplet*. That species of accentuation (as we style it) which attaches to our usually-executed triplet is rarely discoverable in the judicious delivery of the ancient languages, or indeed in the judicious delivery of our own serious compositions. The audible and independent articulation of *every* syllable so circumstanced will considerably relieve, if not wholly destroy that *jigging* effect which characterizes the triplet, and distinguishes that species of modern poetry whimsically called the *dactylic* or *anapæstic*—the very semblance of which movement the dignified orator should disdain to introduce.

 Epitrite

Epitrite	first ..		Bar of $\frac{7}{8}$ resolvable into $\frac{4}{8}$ and $\frac{3}{8}$.
	second ..		
	third ..		
	fourth ..		
Dochmius ..	..		Bar of $\frac{8}{8}$ resolvable into $\frac{5}{8}$ and $\frac{3}{8}$.
Dispondæus ..	..		Bar of $\frac{8}{8}$ resolvable into $\frac{4}{8}$.

These are the *measures* or combinations of *time* (altogether unconnected with forte, or emphatic syllables,) of which the writers of antiquity have so familiarly spoken; and I sincerely hope that by our *collegiate* efforts they may ultimately tend to the embellishment of that language which has fallen to our inheritance.

OF FORTE*.

This topic has been already so amply treated in my late paper on time and rhythmus, that very little more than the ordinary doctrine of our lexicographers presents itself for discussion. These gentlemen may amuse themselves as they please, by insisting that this or that syllable *only*, shall, in their phraseology, be accented or unaccented; or, in plain common English, be articulated or muttered: Mr. Sheridan, too, in his "Art of Reading," and Dr. Blair in his "Lectures on Rhetoric and the Belles Lettres," may also argue the peculiar propriety of confining the *accent* (as they call it) to an *individual* syllable, while every other syllable is denied even a minor privilege—producing by this wretched maxim a sudden and intolerably offensive burst of forte in the midst of piano†: but these anti-musical as well as anti-oratorical precepts never yet were nor ever will be followed by the cultivated speaker. Hence the energetic custom with several good orators of laying, in certain cases, a considerable stress on the antepenult of such words as *dictator*, *representation*, &c. analogous to the Grecian delivery of *ανεχαίτισε*, *διέλυσεν*, &c. These latter indeed may be so uttered (and with excellent effect) that even the

* The author of *Prosodia Rationalis*, for the mere purpose of coining the obscure and unmeaning term "*poize*," has introduced a most idle distinction between force and loudness. Force may certainly be exerted in whisper or suffocated sounds without the production of loudness—but what relation has this to the musico-oratorical question of forte? "*Pulsation*" and "*remission*" too, terms equally ridiculous with the word *poize*, are likewise employed by this gentleman as a convenient cloak for musico-physical absurdities.

† This is the characteristic sign by which our *Gothic* orators, actors, and reciters may be instantly distinguished from their superiors. The second sign is the striking of an *octave*.

χαι

χαι and ελ shall be louder than the τι and υ—in short, that a perfect *crescendo* and *diminuendo* shall be fully recognised in both; as thus ανεχαιτισε και διελυσεν. The Grecian language exhibits innumerable words of this noble character—and why should not our Colleges follow the example by opening a certain number of the antepenultimate syllables of our own?

To conclude this letter. The latitude of *forte* and *piano*, in the delivery of our impassioned language even as at present constituted, is much greater than ordinary observers would imagine; and to this subject the orator should direct his most serious consideration. The suitable and expressive distribution of *forte*, whether throughout individual or several words; nay, the diversified beauties of the *crescendo* and *diminuendo* are, in cases innumerable, in every man's power*; while cultivated taste governed by common sense cannot fail to apply them. *Rules* are of no utility, nor is there any rational standard by which so changeable a character as that of *forte* can be rendered immutable. [To be continued.]

* Especially when enabled by the *writer*. I have heard the following line delivered with ease and uncommon *military* effect:

Cŏhŏrts chărge hōme—the infantry is broke.

Here, in place of the vulgar alternacy of weak and strong syllables, are three consecutive syllables, without an intervening pause, all in complete *crescendo*. When *poets* (if I may be allowed a well-known hackney'd phrase) shall *sink* the country fiddler, and *readers* shall deliver our language as they ought—then may we aspire to *classical* composition. The foregoing is an example of the *first epitrite*—and so military a foot is not, in my opinion, to be met with in any modern language. For amusement, I have tried some military gentlemen with the delivery of these words, but their ordinary habits did by no means lead them to such *martial* execution. See Magazine of October for the method of expressing the word "cohorts."

LXIII. *Remarks on a Paper by Major-General* Brisbane, *on the Method of determining the Time, the Error, and Rate of a Chronometer, by Altitudes taken with a Sextant from an artificial Horizon.* *By Mr.* Ed. Riddle.

To Mr. Tilloch.

Sir,—I have just seen, in the last volume of the Edinburgh Philosophical Transactions, a paper by Major-general Brisbane on the method of determining the time, the error, and rate of a chronometer, by altitudes taken with a sextant from an artificial horizon. I am agreeably surprised to find that his methods of observing and of making his computations scarcely differ in any respect from those which I have practised for several years; and judging

judging from my own experience, I have no reason to doubt that he has over-rated the degree of exactness which may generally be attained by the method which he recommends.

Some of his altitudes were taken by reflection from quicksilver, but he remarks that he has with equal success employed pure limpid oil for that purpose. I have made many experiments on this subject, and I find I can generally make my observations more satisfactorily from oil than either from quicksilver or any thing else that I have tried. From the slightest motion, the surface of quicksilver is so affected with tremors that it is quite impossible to use it with advantage. The Trinity-House School is on the second floor of a substantially built house, yet I have there observed the surface of a quicksilver horizon sensibly agitated by the tumbling down of an empty ale-barrel in the adjoining yard. Indeed I have frequently taken altitudes with perfect satisfaction from the surface of oil, when, from slight and sometimes imperceptible causes, the surface of quicksilver has appeared so tremulous that no dependence could be placed on a contact observed in it. Water I have found liable to the same objections as quicksilver, though in a less degree. Treacle and other semifluids are subject to have their reflective power destroyed by dust, &c. and when their surface happens to be disturbed, it requires a considerable time to settle. If the reflecting surfaces are uncovered, as mine generally are, and at all exposed to the influence of the wind, oil is still more decidedly preferable. When taking the moon's altitude during the day, I find it advantageous to pour the oil into a *black* receiver:—sometimes, as a convenient method of attaining the same object, I pour a little *ink* into a white plate, and then pour the oil upon it. But when the altitude of a star is wanted, recourse must be had to a quicksilver horizon.

The uniformity of General B's results bears creditable testimony to the care with which he has made his observations; but I may be permitted to doubt whether he was justified in marking the time to *tenths* of a second. The increments of time in the margin correspond, from his observations, to increments of 10′ of double altitude in succession; and though no reasonable doubt can be entertained that the mean of the whole will be exact to a small fraction of a second, it appears sufficiently obvious that the time of no single observation can be depended upon to *less than a second*. I am aware that a part of the trifling discrepances which will be observed among these intervals is to be attributed to the slow motion of the sun in altitude at the season in which the observations were made; as it is

Forenoon. SECONDS.	Afternoon. SECONDS.
44·6	45
43	44
44	45·3
45	43·6
44	43·6
46	47
45	45·3
44	44
44	45
46	45·6
45	45·7

is then a matter of some difficulty to ascertain the time of contact *to a second.*

I have been compelled to pay particular attention to the practice of the method of determining the time, from having often to depend upon it for the altitudes in lunar observations; as it is seldom that the altitudes of the sun and the moon, or the moon and a star, can be got conveniently, at the same time, by reflection.

In taking either altitudes or lunar distances I direct my assistant (who is generally one of the young men attending the school) to mark to the nearest second, the time when I give notice that the contact is perfect; and from the nature of the observations, I see no reason to believe that it can be of any advantage to mark it nearer.

The formula by which General B. has computed the time is that which furnishes the rule given in Prob. VI. Requisite Tables. There is a typographical mistake in the analytical statement of it;—sine (co. lat. + declin.) ought to be sine (co. lat. $\pm$ declin). This formula is not generally esteemed the most convenient one for computing the horary angle; but that is of little importance in the presence case, as it is correct.

In taking altitudes for the time I do not *always* make the increments of altitude equal; but when the whole time of observation is small, and the sun is at a considerable distance from the meridian, the increments of altitude are nearly proportional to the intervals of observation, so that any irregularity is easily detected. I then determine the error of the clock, not from the mean of the altitudes, but from the mean of the errors deduced from each altitude calculated separately, and this is exactly General B.'s method. After I have finished my observations, I generally make my young men take altitudes for the same purpose; and a comparison of their results with my own shows me at once whether their observations have been made with requisite correctness.

I perfectly agree with General B. in the opinion which he expresses of the utility of the sextant, as it is now made by Troughton, Jones, and other respectable workmen. It is a portable handy instrument; it requires few adjustments, and those are easily made. Its state can be ascertained at almost any time; and a little practice renders its use extremely easy. It is, in short, in many cases, a valuable substitute for more expensive and complicated instruments.

But with deference to General B., I would here suggest that the sextant can scarcely be considered as an instrument, the principles and the use of which are not generally well understood. If the

the results commonly obtained by it, are less correct than those obtained by General B., I apprehend the true reason is, that such minute accuracy is seldom thought necessary.

General B.'s observations, however, will show what the instrument is capable of, and the friends of practical science are obliged by their publication. One obvious consequence deducible from them is, that the rate of a chronometer may be determined in a shorter time by this method than by meridian transits, or any other method that is practised. The time deduced from a well observed transit of the sun, or a star whose place is well ascertained, may indeed be depended on as probably nearer the truth than that determined from a single altitude, however carefully taken; but the error determined from a single transit (of a star at any rate) is by no means of equal authority with a mean of the errors deduced from 10 or 12 altitudes of the sun. A comparison of General B.'s results with the errors of the astronomical clock as determined by transits in the Greenwich observations will show this clearly enough. Besides, opportunities for observing the transits of proper stars are few compared with those for taking sets of altitudes, and a transit of the sun can be observed but once a day.

In the method of regulating a chronometer by equal altitudes it is not necessary that the latitude should be known to any very great degree of exactness; but to practise the method of single altitudes successfully, it must be known with considerable accuracy; and as a supplement to his paper, General B. promises to transmit another on the method of determining the latitude by a series of altitudes taken with a sextant near noon.

As his method of finding the time is so similar to mine, I think it probable that the method of finding the latitude which he engages to communicate may also be in principle the same as that which I have practised for a considerable time; and my chief reason for wishing you to insert this letter in your respectable publication is, that our methods may be compared when his paper appears.

I shall explain my method perhaps as well as I can in any other way, by giving you an account of some observations which I made last autumn, for determining the latitude of this place. But before I enter on this account, I must inform you that the Trinty-House School is situated in the lower part of Newcastle, not far from the river; the steep banks of which are covered with houses; and that from the direction of the wind, and other circumstances, the smoke, though at all times considerable, is sometimes much more dense than at other times; and that the effect of this difference in the density of the smoke upon the refraction

I am

I am unacquainted with any means of appreciating. I frequently observe the edge of the sun's disk in a state of apparent undulation, at altitudes at which that appearance is never observed in high and open situations.

The observations of which I shall subjoin an abstract were made, like General B.'s, with a sextant of Troughton's divided on platina to $10''$; and the telescope applied to it magnified eight times. The rate of the clock and its error for apparent time at noon were carefully determined, and the times were noted and written down with the altitudes, by a young gentleman on whom, from experience, I knew I could rely. Several altitudes of the sun's *upper* and *lower* limbs *alternately* were taken near the meridian, and the several altitudes were reduced to the meridian by the following theorem. Let P, S, and Z, be the pole, the sun's centre, and the zenith; then, sine $\frac{\prime}{ZS}$ = vers. P. sine PS. sine PZ. cosect. ZS. This theorem was published two or three years ago by Dr. Evans in a slightly different form, in Leybourn's Mathematical Repository; and as I know not whether the number of that work is published which contains the demonstration, I may as well give it here. By spherics, vers. ZS — vers. $\overline{ZP \backsim PS}$ = vers. P. sine PS. sine PZ. Now $\overline{ZP \backsim PS}$ is the meridian zenith distance; and when P is small, it is evidently nearly = ZS.

Hence $\frac{\prime}{\text{vers. } ZS} = \sin. \frac{\prime}{ZS} \sin. ZS$ = vers. P. sine PS. sine PZ;

or sin. $\frac{\prime}{ZS}$ = vers. P. sine PS. sine PZ. cosect. ZS.

The declination of the sun was reduced to the Greenwich time of each altitude, and the latitude found from each altitude separately. The mean of the latitudes resulting from the altitudes of the sun's lower limb was then taken, and also the mean of those resulting from the altitudes of the upper limb, and half the sum of these two means was taken as the latitude resulting from the observations of that day. By taking nearly an equal number of altitudes of the sun's upper and lower limbs, any error that might have arisen from a faulty habit of estimating the contact was effectually obviated.

This method of observing is also useful in determining the time.

The following will require no further explanation, and will be sufficient to exemplify what I have said:

Trinity-

Trinity-House School, Newcastle, Sept. 24, 1817; clock slow for apparent time $10^m\ 11^s$.

Time per clock. H. M. S.	App. time. H. M. S.		Obser. alt. sun's l. l.	Inct. of alt.	Semidi.	Refr. less par.	Declin. S.	Lat. N.
11 43 40	11 53 51	90° −	34 21 10	+52″	+15′ 59″	−1′ 17″	+0° 24′ 55″	=54° 58′ 21″
44 16	54 27		34 21 16	+42	+15 59	−1 17	+0 24 56	=54 58 24
50 10	12 0 21		34 21 54	+ 0	+15 59	−1 17	+0 25 2	=54 58 22
51 24	1 35		34 21 45	+ 4	+15 59	−1 17	+0 25 3	=54 58 28
52 54	3 5		34 21 29	+13	+15 59	−1 17	+0 25 5	=54 58 31
54 9	4 20		34 21 20	+25	+15 59	−1 17	+0 25 6	=54 58 27
55 24	5 35		34 20 59	+42	+15 59	−1 17	+0 25 7	=54 58 30
							Lat. per sun's l. l.	54 58 26

Time per clock. H. M. S.	App. time. H. M. S.		Obser. alt. sun's u. l.	Inct. of alt.	Semidi.	Refr. less par.	Declin. S.	Lat. N.
11 47 5	11 57 16	90° −	34 53 34	+10	−15 59	−1 15	+0 24 59	=54 58 31
47 50	58 1		34 53 43	+ 6	−15 59	−1 15	+0 24 59	=54 58 26
50 59	12 1 10		34 53 42	+ 2	−15 59	−1 15	+0 25 3	=54 58 27
52 0	2 11		34 53 40	+ 6	−15 59	−1 15	+0 25 4	=54 58 24
53 17	3 28		34 53 25	+17	−15 59	−1 15	+0 25 5	=54 58 27
54 57	5 8		34 53 5	+36	−15 59	−1 15	+0 25 5	=54 58 26
							Lat. per sun's u. l.	54 58 27

This may serve as a specimen of the method, and I shall only extract the final results of the observations of a few days.

1817. August 20th.	54°	58′	30″	
30th.	54	58	30	
Sept. 1st.	54	58	35	
2d.	54	58	28	
8th.	54	58	27	
24th.	54	58	26	(see above.)
Oct. 6th.	54	58	28	
9th.	54	58	32	
Mean	54	58	29	

This exactly agrees with the mean of the results of 16 single meridian altitudes taken with the same instrument, and with the results of several hundreds of observations made with an excellent sextant of Ramsden's which belongs to the Institution.

I am, sir,
With due respect, your obedient servant,
EDWARD RIDDLE,
Master of the Trinity-House School,
Newcastle-upon-Tyne.

Oct. 21, 1818.

LXIV. *An Account of Experiments for determining the Length of the Pendulum vibrating Seconds in the Latitude of London. By Capt.* HENRY KATER, *F.R.S.*

[Concluded from p. 370.]

HAVING thus satisfied myself that no injury to the knife-edges was to be apprehended from moderate use, the pendulum was again suspended, but now, to my surprise, I found the number of vibrations different from what they were before the remeasurement. This difference became still greater on the following day; and it at length occurred to me that the moisture of the atmosphere must have undergone some change, and that an alteration had been thus occasioned in the weight of the wooden extremities of the pendulum. On referring to the register of the hygrometer kept by Mr. Browne, it was found that a considerable change had in fact suddenly taken place from moisture to dryness; and so great was the derangement of the pendulum from this apparently trivial cause, that it became necessary to move the second weight. This was accordingly done, and the following experiments made for again bringing the number of vibrations to an equality.

Slider

Slider 29 divisions. Clock gaining 0″·18 on mean time. — Great weight *above.* Second weight moved. — Barometer 29·70.

	Temp.	Time of coincidence. m.	s.	Arc of vibration.	Mean arc.	Interv. in seconds	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours
July 1st.	° 69·1	10	31	° 1·21	°				s	
		18	52	1·01	1·11	501	499	86055·09	2·01	86057·10
	69·1	27	14	0·83	0·92	502	500	86055·78	1·38	86057·16
									Mean	86057·13
									Clock	+ 0·18
	69·1	mean								86057·31
		Great weight *below.*								
	68·8	47	22	1·23						
		55	44	1·09	1·16	502	500	86055·78	2·20	86057·98
	68·9	4	6	0·99	1·04	502	500	86055·78	1·76	86057·54
									Mean	86057·76
									Clock	+ 0·18
									Temp.	— 0·09
	68·9	mean								86057·85

The second weight was now securely fixed.

Slider 19 divisions. Clock gaining 0″·18 on mean time. — Great weight *above.* — Barometer 29·70.

	Temp.	m.	s.	Arc of vibration.	Mean arc.	Interv. in seconds	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours
July 1st.	° 69·3	0	52	° 1·21	°				s	
		9	17	1·00	1·10	505	503	86057·82	1·98	86059·80
	69·2	17	42	0·86	0·93	505	503	86057·82	1·42	86059·24
									Mean	86059·52
									Clock	+ 0·18
	69·3	mean								86059·70
		Great weight *below.*								
	69·3	22	57	1·25						
		31	21	1·13	1·19	504	502	86057·16	2·31	86059·47
	69·3	39	45	1·00	1·06	504	502	86057·16	1·83	86058·99
									Mean	86059·23
									Clock	+ 0·18
	69·3	mean								86059·41

Slider 21 divisions. Clock gaining 0″·18 on mean time. Great weight *above.* Barometer 29·70.

	Temp.	Time of coincidence. m.	s.	Arc of vibration.	Mean arc.	Interv in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
July 1st.	° 69·3	28	9	° 1·23	° 1·13	503	501	86056·47	s. 2·08	86058·55
		36	32	1·03	0·96	504	502	86057·16	1·51	86058·67
		44	56	0·89	0·81	505	503	86057·82	1·07	86058·89
	69·3	53	21	0·74						
									Mean	86058·70
									Clock	+ 0·18
	69·3	mean								86058·88

E Great weight *below.*

	Temp.	m.	s.	Arc of vibration.	Mean arc.	Interv in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
	69·3	49	6	1·21	1·15	503	501	86056·47	2·16	86058·63
		57	29	1·09	1·03	504	502	86057·16	1·73	86058·89
		5	53	0·98	0·92	504	502	86057·16	1·41	86058·57
		14	17	0·88	0·83	505	503	86057·82	1·13	86058·95
	69·3	22	42	0·78						
									Mean	86058·76
									Clock	+ 0·18
	69·3	mean								86058·94

Slider 20 divisions. Clock gaining 0″·18 on mean time. Great weight *above.* Barometer 29·70.

	Temp.	m.	s.	Arc of vibration.	Mean arc.	Interv in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
July 1st.	69·3	1	46	1·34	1·23	502	500	86055·78	2·47	86058·25
		10	8	1·12	1·03	503	501	86056·47	1·73	86058·20
		18	31	0·94	0·86	505	503	86057·82	1·21	86059·03
		26	56	0·79	0·73	506	504	86058·49	0·87	86059·36
	69·3	35	22	0·68						
									Mean	86058·71
									Clock	+ 0·18
	69·3	mean								86058·89

F Great weight *below.*

	Temp.	m.	s.	Arc of vibration.	Mean arc.	Interv in seconds.	No. of vibrats.	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
	69·3	40	28	1·23	1·17	503	501	86056·47	2·23	86058·70
		48	51	1·11	1·04	503	501	86056·47	1·77	86058·24
		57	14	0·98	0·93	504	502	86057·16	1·41	86058·57
		5	38	0·88	0·84	506	504	86058·49	1·15	86059·64
	69·4	14	4	0·80						
									Mean	86058·79
									Clock	+ 0·18
									Temp	+ 0·04
	69·4	mean								86059·01

Slider 20 divisions.
Clock gaining 0″·18 on mean time.

Great weight *above*.

Barometer 29·70.

	Temp.	Time of coincidence. m.	s.	Arc of vibration.	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours	Corr. for arc.	Vibrations in 24 hours.
July 2d.	° 68·5	33	7	° 1·41	° 1·29	502	500	86055·78	s. 2·72	86058·50
		41	29	1·18	1·07	505	503	86057·82	1·87	86059·69
		49	54	0·97	0·89	504	502	86057·16	1·29	86058·45
		58	18	0·82	0·76	503	503	86057·82	0·94	86058·76
	68·5	6	43	0·71						
									Mean	86058·85
									Clock	+ 0·18
	68·5	mean								86059·03

G. **Great weight *below*.**

	Temp.	Time of coincidence. m.	s.	Arc of vibration.	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours	Corr. for arc.	Vibrations in 24 hours.
	68·0	51	20	1·17	1·10	504	502	86057·16	1·98	86059·14
		59	44	1·04	0·94	506	504	86058·49	1·44	86059·93
		8	10	0·94	0·89	504	502	86057·16	1·29	86058·45
		16	34	0·84	0·79	506	504	86058·49	1·02	86059·51
	68·0	25	0	0·75						
									Mean	86059·26
									Clock	+ 0·18
									Temp.	— 0·22
	68·0	mean								86059·22

Slider 18 divisions.
Clock gaining 0″·18 on mean time.

Great weight *above*.

Barometer 29·70.

	Temp.	Time of coincidence. m.	s.	Arc of vibration.	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours	Corr. for arc.	Vibrations in 24 hours.
July 2d.	° 68·6	18	31	° 1·25	° 1·14	504	502	86057·16	s. 2·12	86059·28
		26	55	1·04	0·96	504	502	86057·16	1·50	86058·66
		35	19	0·89	0·81	507	505	86059·16	1·07	86060·23
		43	46	0·74	0·68	505	503	86057·82	0·75	86058·57
	68·7	52	11	0·63						
									Mean	86059·18
									Clock	+ 0·18
	68·7	mean								86059·36

H. **Great weight *below*.**

	Temp.	Time of coincidence. m.	s.	Arc of vibration.	Mean arc.	Interv. in seconds.	No. of vibrats.	Vibrations in 24 hours	Corr. for arc.	Vibrations in 24 hours.
	68·8	57	41	1·23	1·16	503	501	86056·47	2·20	86058·67
		6	4	1·10	1·05	504	502	86057·16	1·80	86058·96
		14	28	1·00	0·94	504	502	86057·16	1·44	86058·60
		23	52	0·88	0·85	506	504	86058·49	1·18	86059·67
	69·1	31	18	0·82						
									Mean	86058·98
									Clock	+ 0·18
									Temp.	+ 0·09
	68·9	mean								86059·25

Slider 18 divisions. Clock gaining 0″·18 on mean time.		Great weight *above*.								Barometer 29·70.
	Temp.	Time of coincidence. m.	s.	Arc of vibration.	Mean arc.	Interv. in seconds.	No. of vibrats	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
July 2d.	69°·4	17	43	1°·24	1°·13	504	502	86057·16	s. 2·08	86059·24
		26	7	1·03	0·95	504	502	86057·16	1·47	86058·63
		34	31	0·88	0·81	504	502	86057·16	1·07	86058·23
		42	55	0 74	0·69	507	505	86059·16	0·78	86059·94
	69·3	51	22	0·64						
									Mean	86059·01
									Clock	+ 0·18
	69·3	mean								86059·19
I		Great weight *below*.								
	69·3	38	22	1·20	1·14	502	500	86055·78	2·12	86057·90
		46	44	1·08	1·03	505	503	86057·82	1·73	86059·55
		55	9	0·98	0·93	504	502	86057·16	1·42	86058·58
		3	33	0·88	0·84	505	503	86057·82	1·15	86058·97
	69·3	11	58	0·80						
									Mean	86058·75
									Clock	+ 0·18
	69·3	mean								86058·93

Slider 18 divisions. Clock gaining 0″·18 on mean time.		Great weight *above*.								Barometer 29·70.
July 2d.	69·3	53	29	1·31	1·19	503	501	86056·47	2·31	86058·78
		1	52	1·08	1·00	504	502	86057·16	1·63	86058·79
		10	16	0 92	0·84	506	504	86058·49	1·15	86059·64
		18	42	0·76	0·71	505	503	86057·82	0·82	86058·64
	69·3	26	7	0·66						
									Mean	86058·96
									Clock	+ 0·18
	69·3	mean								86059·14
K		Great weight *below*.								
	69·3	32	18	1·24	1·17	502	500	86055·78	2·23	86058·01
		40	40	1·10	1·05	504	502	86057·16	1·80	86058·96
		49	4	1 00	0·95	505	503	86057·82	1·47	86059·29
		57	29	0·90	0·85	504	502	86057·16	1·18	86058·34
	69·3	5	53	0·81						
									Mean	86058 65
									Clock	+ 0·18
	69·3	mean								86058·83

Slider 19 divisions. Clock gaining 0″·18 on mean time. — **Great weight *above*.** — Barometer 29·90.

	Temp.	Time of coincidence.		Arc of vibration.	Mean arc.	Interv. in seconds.	No of vibrats	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
	°	m.	s.	°	°				s.	
July 3d.	68·0	2	18	1·28	1·17	504	502	86057·16	2·23	86059·39
		10	42	1·06	0·98	504	502	86057·16	1·57	86058·73
		19	6	0·91	0·82	505	503	86057·82	1·10	86058·92
		27	31	0·74	0·69	506	504	86058·49	0·77	86059·26
	68·2	35	57	0·64						
									Mean	86059·08
									Clock	+ 0·18
	68·1	mean								86059·26
L	**Great weight *below*.**									
	67·7	23	52	1·29	1·21	504	502	86057·16	2·39	86059·55
		33	16	1·14	1·07	504	502	86057·16	1·87	86059·03
		40	40	1·01	0·97	504	502	86057·16	1·54	86058·70
		49	4	0·93	0·82	506	504	86058·49	1·10	86059·59
	67·8	57	30	0·72						
									Mean	86059·22
									Clock	+ 0·18
									Temp.	— 0·18
	67·8	mean								86059·22

Slider 19 divisions. Clock gaining 0″·18 on mean time. — **Great weight *above*.** — Barometer 29·90.

	Temp.	m.	s.	Arc of vibration.	Mean arc.	Interv. in seconds.	No of vibrats	Vibrations in 24 hours.	Corr. for arc.	Vibrations in 24 hours.
July 3d.	68·3	45	3	1·23	1·13	504	502	86057·16	2·08	86059 24
		53	27	1·03	0·95	504	502	86057·16	1·47	86058·63
		1	51	0·87	0·80	505	503	86057·82	1·04	86058·86
		10	16	0·74	0·68	506	504	86058·49	0·75	860592·4
	68·4	18	42	0·63						
									Mean	86058·99
									Clock	+ 0·18
	68·4	mean.								86059·17
M	**Great weight *below*.**									
	68·4	24	31	1·24	1·17	503	501	86056·47	2·23	86058·70
		32	54	1·11	1·05	504	502	86057·16	1·80	86058·96
		41	18	0·99	0·94	504	502	86057·16	1·44	86058·60
		49	42	0·90	0·86	506	504	86058·49	12·0	86059·69
	68·5	58	8	0·82						
									Mean	86058·99
									Clock	+ 0·18
									Temp.	+ 0·04
	68·5	mean								86059·21

The

The results of such of the preceding experiments as are to be used for calculating the length of the seconds pendulum, are brought under one view in the following table:

Place of the slider.	Expt.	Temp.	Barom.	No of vibrations. Great wt. *above.*	Difference.	No. of vibrations. Great wt. *below.*	Vibs. in excess or defect.
23	A	68·7	29·76	86059·39	·03	86059·42	—
23	B	71·3	29·86	86057·70	·23	86057·93	—
23	C	71·4	29·86	86057·93	·23	86057·70	+
23	D	73·1	29·95	86056·54	·43	86056·97	—
Pendulum remeasured.							
21	E	69·3	29·70	86058·88	·06	86058·94	—
20	F	69·3	29·70	86058·89	·12	86059·01	—
20	G	68·5	29·70	86059·03	·19	86059·22	—
18	H	68·7	29·70	86059·36	·11	86059·25	+
18	I	69·3	29·70	86059·19	·16	86058·93	+
18	K	69·3	29·70	86059·14	·31	86058·83	+
19	L	68·1	29·90	86059·26	·04	86059·22	+
19	M	68·4	29 90	86059·17	·04	86059·21	—
			Mean	86058·71		86058·72	

No other explanation of this table appears to be necessary, than that the column entitled "Difference" expresses the difference between the number of vibrations in the two positions of the pendulum, and that the last column indicates by the sign + or — whether the number of vibrations exceeds or falls short of the truth; which inference is drawn from a comparison of the number of vibrations when the great weight is above, with the number in that position of the pendulum when the great weight is below. The mean of the vibrations in the column "Great weight *above*" not differing sensibly from that headed "Great weight *below*," is a proof that the number of vibrations in either position of the pendulum may be considered as equal, and consequently that the one knife edge being the point of suspension, the other must necessarily be in the centre of oscillation.

Length of the Pendulum vibrating Seconds.

The distance between the knife edges was as follows:

	Inches. Divisions.	Inches.
By the 1st measurement	39·4 + 956·47 =	39·44094
By the 2d, ...	39·4 + 954·49 =	39·44086
By the 3d, ...	39·4 + 955·65 =	39·44090
	Mean	39·44090
Corr. for error in division of the scale (see page 176)		−0·00005
		39·44085

Hence,

Hence, 39·44085 inches may be taken as the distance between the knife edges at the temperature of 62 degrees.

Using the vibrations when the great weight was *below*, as being nearer to the truth than in the other position of the pendulum, we obtain the following results.

Expt.	Temp.	Barom.	Vibrations in 24 hours	Length of the seconds pen. in air.	Corr. for the atmosphere	Length of the seconds pen. in vacuo	Difference from the mean.
A	68·7	29·76	86059·42	39·13313	·00544	39·13857	+·00028
B	71·3	29·86	86059·93	39·13278	·00544	39·13822	—·00007
C	71·4	29·86	86057·70	39·13260	·00544	39·13804	—.00025
D	73·1	29·95	86056·97	39·13259	·00544	39·13803	—·00026
E	69·3	29·70	86058·94	39·13293	·00544	39·13837	+·00008
F	69·3	29·70	86059·01	39·13298	·00544	39·13842	+·00013
G	68·5	29·70	86059·22	39·13286	·00545	39·13831	+·00002
H	68·7	29·70	86059 25	39·13296	·00544	39·13840	+·00011
I	69·3	29·70	86058·93	39·13291	·00544	39·13834	+·00005
K	69·3	29·70	86058·83	39·13282	·00544	39·13825	—·00003
L	68·1	29·90	86059·22	39·13271	·00548	39·13819	—·00009
M	68·4	29·90	86059·21	39·13281	·00548	39·13829	—·00000
					Mean	39·13829	

The length of the pendulum thus obtained requires yet another correction to reduce it to what it would have been at the level of the sea. The elevation of the apartments of the Royal Society at Somerset House above low-water mark, is known to be 81 feet; and by several careful observations with an excellent mountain barometer by Ramsden, I found the room in Portland Place, in which the experiments were made, to be two feet below the Royal Society's apartments: and as the height of the pendulum above the floor was four feet, we obtain 83 feet for the elevation of the pendulum above the level of the sea. Now the force of gravity increasing inversely as the square of the distance from the earth's centre, the length of the pendulum must be increased in the same proportion; and taking the radius of the earth for the latitude of Portland Place to be 3954·583 miles, we have 39·1386 inches for the length of the pendulum vibrating seconds at the level of the sea.

It may be remarked that the greatest difference between the mean result and that of any one of the twelve sets of experiments contained in the preceding table, is only ·00028 of an inch, or $\frac{1}{134959}$th of the whole length of the pendulum; and as seven of the twelve sets are within one ten-thousandth of an inch of the mean result, it may be inferred that the above determination cannot be very distant from the trnth.

The length here given, is that required to perform one vibration in $\frac{1}{86400}$dth part of a mean *solar* day, this being the measure of time usually employed for the purpose; but I am at a loss to to conjecture why this is preferred to the sidereal day, a measure of time which marks a complete revolution of the earth, and is readily obtained, being the interval between the returns of any fixed star to the meridian.

I shall now proceed to notice the sources of error which may be supposed to have affected the results of the preceding experiments.

These may be classed under the following heads:

1. The measurement of the distance of the knife edges.
2. The number of vibrations in 24 hours.
3. The temperature, and
4. The form of the knife edges.

On the first, it is scarcely necessary to offer any remark. Since the mean results of three several sets of measurements are within one ten-thousandth of an inch of each other, and the different methods employed, preclude, it may be presumed, any accidental coincidence, we may with confidence infer that the error in the distance of the knife edges cannot amount to one ten-thousandth of an inch.

Among the number of vibrations in 24 hours given in the various sets of experiments, there appear to be differences which amount in some instances to 1·6. These differences however do not influence the truth of the result, beyond a certain minute quantity, the extent and origin of which I shall proceed to explain.

In order to determine the vibrations in 24 hours, it is necessary to ascertain the number of vibrations and parts of a vibration made by the brass pendulum during a certain number of *complete* seconds; but the moment of observation being limited to that when the brass pendulum is at the lowest part of the arc, the process is of necessity reversed, and the brass pendulum is observed to make a certain number of *complete* vibrations, during a certain number of seconds and parts of a second which constitute *the interval*. The disappearance of the disk can however be noted only to a single second, and the brass pendulum may arrive at the lowest part of the arc either precisely at this second, or at any portion of a second preceding it. An error might possibly arise from this circumstance amounting to nine-tenths of a second, by which the interval deduced from observation would be less than the truth; and as an error of one second in the interval, occasions a difference of 0·63 in the number of vibrations in 24 hours, if 0·55 (the proportional part of 0·63) be divided by 4 (the number of intervals forming each set of experiments), we have 0·14 for the greatest error *in defect* in the number of vibrations in 24 hours which can arise from this cause.

 On

On the contrary, if the *second* coincidence or return of the brass pendulum to the lowest point of the arc, should have taken place nine-tenths of a second before the second at which the disappearance of the disk was noted, the error in the number of vibrations in 24 hours would amount to the same quantity, and would now be *in excess*.

If the first and third coincidences take place accurately at the time of the observed disappearance of the disk, and the observation of the second coincidence should differ nine-tenths of a second from the truth, it is obvious that the number of vibrations in 24 hours deduced from each interval will be erroneous about 0·56, the one being *in excess*, the other *in defect*. The mean of both will be the truth, though the observed difference between the two amounts to so considerable a quantity as 1·2.

The last coincidence of each set takes place when the arc of vibration is much reduced. It is therefore not impossible that an error of one second may sometimes, though rarely, occur in determining the time of this coincidence. This would occasion an error of about 0·63 in the number of vibrations in 24 hours, which divided by 4 as before, would influence the mean result 0·15 of a vibration.

In estimating these errors, I have taken an extreme case, as it is probable they would in most instances be compensated by the succeeding intervals. Supposing them however to be combined, the greatest effect on the mean result of any one set of experiments might amount to about 0·3 of a vibration in 24 hours, and the difference between the number of vibrations in either position of the pendulum, might have been double this quantity, and yet, when the great weight was below, not have differed from the truth more than 0·3 of a vibration.

It appears then, that if the experiments have been conducted with sufficient care, no greater difference should be found between the mean, and any one of the resulting lengths of the pendulum contained in the preceding table, than might have been occasioned by a difference of 0·3 of a vibration in 24 hours, and this is found to be about 0·0003 of an inch.

In fact, on referring to the table we perceive that the experiments A and D, which differ most from the mean, give, the one, ·00029 of an inch *in excess*, and the other ·00026 *in defect*.

In considering the sources of error, it may not be unnecessary to remark, that had the bar of the pendulum been made too thick, and the knife edges not been placed accurately at right angles to it, an error, though very minute, might have arisen from the effect of the obliquity in diminishing the distance of the centre of oscillation from the axis. This was sufficiently guarded against by by having the bar so thin as to ensure its becoming perpendicular by its own weight, had the position of the knife edge been in a small degree erroneous; for though the form the bar would assume is strictly speaking a curve, it may without sensible error be considered as a straight line.

With regard to temperature, every precaution was taken to prevent error. The thermometer used was made by Mr. Troughton for the late Sir George Shuckburgh. It is divided into half degrees, and the height of the mercury may be estimated to one-tenth of a degree. It has been already observed in the preceding part of this paper, that the thermometer was approached only at the first and last coincidences.

The experiments themselves afford, it is presumed, a sufficient proof of the stability of the knife edges. Every care was taken to form them in the first instance as perfect as possible; and after four sets of experiments had been made, they were found on remeasurement to have suffered no perceptible alteration; and it is evident by the near agreement of the results, that they remained uninjured during the succeeding experiments; it is difficult therefore to conceive that any error can have arisen from this source.

I may here remark, that the method I have employed in determining the length of the pendulum, possesses other advantages besides that of superseding the errors arising from unequal density or figure; and one, not the least considerable, is, that after a very few vibrations, the true length of the pendulum is bounded by certain known limits. Thus in the two first sets of experiments, after the remeasurement of the distance between the knife edges, we may remark that when the slider was at 29 divisions, the number of vibrations (the great weight being below) was, 86057·85 and in *defect;* and when the slider was removed to 19 divisions, the number of vibrations was 86059·41 and in *excess.* The true number of vibrations then is evidently between the two, and the utmost extent of error in using either of these numbers must fall short of 1·73 their difference when reduced to the same temperature. But if the mean be employed in the computation, the length of the pendulum will be found to differ only about four ten-thousandths of an inch from the mean result given in the foregoing table.

It may not be unnecessary to add, that every experiment made has been retained; nor do I consider any one as less entitled to credit than the rest, excepting that marked A, in the table; and that, only because the rate of the clock was not observed on the day of the experiment, but was taken to be the same as the rate of the following day.

The

The length then of the pendulum vibrating seconds in vacuo at the level of the sea, measured at the temperature of 62° of Fahrenheit, appears to be

		Inches.
By Sir G. Shuckburgh's standard	..	39·13860
By General Roy's scale	..	39·13717
By Bird's Parliamentary standard	..	39·13842

the latitude of the place of observation being 51° 31′ 8″·4 north *.

An objection might be urged against the use of the knife edge, on the ground that being an elastic substance it may possibly suffer temporary compression, and thus perhaps introduce a source of error. In order to meet any doubt that might arise on this important part of the subject, it is my intention to commence a series of experiments with a pendulum of the same construction as that which has been described, but vibrating on cylinders instead of knife edges, and I trust soon to have the honour of laying the result before the Royal Society.

London, July, 1817.

APPENDIX.

Since the preceding paper was written, a very curious and important theorem has been discovered by M. Laplace, of which Dr. Young has favoured me with a concise demonstration, together with some other investigations, which I shall subjoin in his own words.

"My dear sir, — I cannot forbear to congratulate you on the discovery of the singular property of your pendulum, which has lately been demonstrated by M. Laplace, since it appears to remove the only doubt, that could reasonably be entertained, of the extreme accuracy of the results of your experiments. The correction for the curvature of the rolling surfaces, in the case of a simple pendulum, is very easily obtained from the geometrical determination of the curve described, although M. Laplace's train of reasoning, from mechanical principles, is somewhat too elaborate to be readily followed through all the symbols in which it is enveloped: and the same geometrical considerations appear, at first sight, to be equally applicable to the case of compound pendulums in general, since the motions of all their effective parts are concentric with those of a simple one similarly suspended.

* The latitude was deduced from the data contained in the trigonometrical survey; Mr. Browne's house bearing from Portland Chapel 74° 38′ 50″ west from the north, the distance being 283 feet. This differs only 0″·1 from the latitude determined by Mr. Browne from a great number of observations.

But

But upon further reflection, it becomes evident that these motions, though concentric, are related to each other in proportions somewhat different from those of a similar pendulum vibrating on a single point, and it is therefore necessary to determine the modification of the motion produced by this difference of connexion. The investigation may however be conducted in a method much more simple and intelligible to ordinary capacities, than that which has been adopted by the celebrated mathematician to whom we are indebted for the theorem; and I am tempted to send you an "*aperçu*" of the reasoning by which I have satisfied myself respecting it.

"It follows immediately from the general theorem for finding the curvature of trochoids of all kinds, (Lectures on Nat. Phil. II. p. 559) that the radius of curvature of the path of any point, in the rod of a pendulum supported by a cylindrical axis, will initially be a third proportional to the distances of the point from the centre of the cylinder, and from the surface on which it rolls: so that when the cylinder is small, and the pendulum simple, the centre of curvature of its path may be considered as situated at the distance of the radius r below the point of contact: and this is obviously the only correction required for such a pendulum as that of Borda. But when the weight is divided, or of considerable magnitude, it becomes necessary to calculate the effect of the different curvatures of the paths of its different parts, and to compare these paths with that of a pendulum A of any given length a. Supposing, for the sake of simplicity, the weight of each horizontal section to be concentrated in the vertical line, and calling the distance of any particle P below the surface of the cylinder x, the radius of curvature of its path will be a third proportional to $x+r$ and x, that is, $\frac{xx}{x+r}$; and the inclination of the curve at a given distance from the vertical line being always directly as the curvature, or inversely as its radius, the force derived from the weight of P will be to the force, at an equal distance in the path of A, as a to $\frac{xx}{x+r}$, or as $\frac{a(x+r)}{xx}$ to 1. Now the point of the rolling pendulum confined to the vertical line is not the centre of curvature, but initially the surface of the cylinder: so that this must be considered as the point of intersection with the vertical line, and as the fulcrum of the lever; consequently the distance of P from the vertical line will be to that of the pendulum A, as x to a, and its immediate force will be $\frac{a(x+r)}{xx} \cdot \frac{x}{a} \cdot \mathrm{P} = \frac{x+r}{x}\mathrm{P}$; but this force, acting only at the end of a lever x, will have its effect at A again reduced in the ratio of of x to a, and will then become $\frac{x+r}{a}$P: and if we express the sum of all the similar forces belonging to the body by the character Σ, whether found by a fluxional calculation, or otherwise, we have the whole force at A, $\Sigma\frac{x+r}{a}$P. The reduced or rotatory inertia of the body, sometimes very improperly called the "momentum" of inertia, will also be expressed by $\Sigma\frac{xx}{aa}$P, being reduced in the ratio of the squares of the distances from the fulcrum; consequently the accelerative force will be to that of the pendulum A as $\frac{\Sigma\frac{xx}{aa}\text{P}}{\Sigma\frac{x+r}{a}\text{P}}$ to 1, or as $\frac{\Sigma xx\text{P}}{a\Sigma(x+r)\text{P}}$ to 1; since it is indifferent whether the integral or the differential be divided by the constant quantity a: and in order to express the length of the equivalent pendulum, we must suppose a to be as much lengthened as the force is weakened, so that we have for this length $\frac{\Sigma xx\text{P}}{\Sigma(x+r)\text{P}}$. It is obvious that the denominator of this fraction is the same that would express the force of the body with regard to the centre of the cylinder as a fixed point; and it might indeed have been inferred at once, from the principle of virtual velocities, that the force must be the same in either case, however irregular the form of the body may be: but it is somewhat more satisfactory to follow the mechanical steps by which the operation of the law takes place. If we make $r=0$, we have $\frac{\Sigma xx\text{P}}{\Sigma x\text{P}}=l$, for the length of the equivalent pendulum when the surface of the cylinder is supposed to be the centre of suspension; and it follows from the well-known properties of the centre of gravity, that ΣxP, the sum of the product of all the particles into their distances, is equal to Qd, the product of the whole weight Q into the distance of the centre of gravity from the point of suspension; and $\Sigma x^2\text{P}=\Sigma x\text{P}l$ $=d\,\text{Q}\,l$, so that the equivalent length for the rolling pendulum becomes $\frac{dl\text{Q}}{\Sigma(x+r)\text{P}}=\frac{dl\text{Q}}{\Sigma x\text{P}+\Sigma r\text{P}}=\frac{dl\text{Q}}{d\text{Q}+r\text{Q}}=\frac{l}{1+\frac{r}{d}}=l\left(1-\frac{r}{d}\right)$, r being supposed very small; which, for a simple pendulum, when $d=l$, becomes $l-r$, as it ought to do. We must however find the displacement of the centre of suspension which is capable of producing an equal alteration in the length of the equivalent pendulum; and for this purpose we must have recourse to the theorem of Huygens, which may be easily deduced from the expression

pression $\frac{\Sigma xxP}{dQ}$: for calling $x-d$, the distance of any particle of the body from its centre of gravity, y, we have $x^2=(d+y)^2=d^2+2dy+y^2$, and $\Sigma x^2P=\Sigma d^2P+2d\Sigma yP+\Sigma y^2P=d^2Q+0+\Sigma y^2P$, the integral of ΣyP, the product of the distance of each particle into its distance from the common centre of gravity always vanishing: consequently $l=\frac{\Sigma yyP+d^2Q}{dQ}=\frac{\Sigma yyP}{dQ}+d$, and $l-d=\frac{\Sigma yyP}{dQ}$; which is Huygens's theorem: the constant quantity $\frac{\Sigma yyP}{Q}$ being equal to $dl-d^2$. If now we suppose d to be increased by the small quantity s, the reciprocal, instead of $l-d$, will become $\frac{dl-dd}{d+s}=\frac{l-d}{1+\frac{s}{d}}=(l-d)\left(1-\frac{s}{d}\right)=l-d-l\frac{s}{d}+s$,

to which adding $d+s$, we have $l-l\frac{s}{d}+2s$, the increase of the length being $\frac{2d-l}{d}s$; and making this equal to $-\frac{l}{d}r$, we have $s=\frac{-lr}{2d-l}$: and when the pendulum is inverted, substituting $l-d$ for d, the expression becomes $\frac{-lr}{2l-2d-l}=\frac{lr}{2d-l}$, which, added to the former negative value of the same quantity, must always destroy it: so that the length of the equivalent pendulum will be truly measured by the simple distance of the surfaces of the cylinders, as M. Laplace has demonstrated.

"There is however another correction, of which it becomes necessary to determine the value, when a very sharp edge is used for the axis of motion, as in the pendulum which you have employed: since it appears very possible, that in this case the temporary compression of the edge may produce a sensible elongation of the pendulum. But it will be found, by calculating the magnitude of this change, that when the edge is not extremely short, and when its bearing is perfectly equable, this correction may be safely neglected.

"Supposing a to be the distance from the edge, in the plane bisecting its angle, at which the thickness is such, that the weight of the modulus of elasticity corresponding to the section shall become equal to the weight of the pendulum, the elasticity at any other distance x from the edge will be measured by x, while the weight is represented by a; so that the elementary increment x' will be reduced by the pressure of the weight to $\frac{x}{a+x}x'$, and the element of the compression will be $\frac{a}{a+x}x'$, and its fluxion

$\frac{a}{a+x}$

$\frac{a}{a+x}dx$, of which the fluent is aHL $\frac{a+x}{a}$. Now the height of the modulus of elasticity of steel is ten million feet, (Lect. Nat. Phil. II. p. 509) and the weight of a bar, an inch square, and of this height, would be about 30 millions of pounds; so that if the weight be 10 pounds, and the line of bearing an inch long, the thickness at the distance a must be one three-millionth of an inch; and supposing the angle a right one, a must be $\frac{1}{4244000}$; and making $x=1$, we have the whole compression of the edge within the depth of an inch $\frac{1}{4244000}$ HL 4244001; and this logarithm being 15·26, the correction becomes equal to the 360 thousandth of an inch. If the bearing were one-tenth of an inch only, the compression for both the opposite edges would become $\frac{1}{18000}$, supposing that they retained their elasticity, and underwent no permanent alteration of form. In fact, however, the edge must be considered as a portion of a minute cylinder, which will be still less compressible than an angle contained by planes; and the happy property, demonstrated by M. Laplace, will prevent any sensible inaccuracy from this cause, however blunt the edges may be, supposing that the steel is of uniform hardness in both.

Believe me, my dear sir, very sincerely yours,

Welbeck-street, Jan. 5, 1818. THOMAS YOUNG."

"P. S. It is easy to show that the determination of the length of the pendulum, by means of a weight sliding on a rod or bar, which is the method that I have proposed as the most convenient for obtaining a correct standard, is equally independent of the magnitude of the cylinder employed. The reduced inertia $\Sigma x^2 P$ here consists of two portions: for the rod we may take the equivalent expression dlQ, which we may call axy, a being the weight of the bar (Q), x the distance (d) of the centre of gravity, and y the equivalent length (l): for the ball we must employ the formula $\Sigma x^2 P=\Sigma y^2 P+d^2 Q$, and call $\Sigma y^2 P$, u, and $d^2 Q$, bz^2, b being the weight of the ball, and z the distance of its centre of gravity from the point of suspension: and in the same manner the force $\Sigma(x+r)P=(d+r)Q$ must be composed of the two portions $a(x+r)$ and $b(z+r)$, so that the equivalent length becomes $\frac{axy+u+bzz}{a(x+r)+b(z+r)}=\frac{z^2+\frac{axy+u}{b}}{z+\frac{ax+ar+br}{b}}$; which we may call $\frac{zz+v}{z+w}$ $=t$. The experiment being then performed in four different positions of the weight, at the distances d', d'', and d''', so that the second value of z may be $z-d'=z'$, the third $z-d''=z''$, and the fourth $z-d'''=z'''$, we must observe the times of vibration,

tion, and deduce from them the comparative lengths of the equivalent pendulum, t, $n't$, $n''t$, and $n'''t$: and hence the value of z, of v, and of t may be obtained, without determining w, and of course without employing the quantity r.

First, $\frac{z^2+v}{z+w}=t$, $\frac{z'^2+v}{z'+w}=n't$, $\frac{z''^2+v}{z''+w}=n''t$, $\frac{z'''^2+v}{z'''+w}=n'''t$.

II. $z+w=\frac{z^2+v}{t}$, $z'+w=\frac{z'^2+v}{n't}$, $z''+w=\frac{z''^2+v}{n''t}$, $z'''+w=\frac{z'''^2+v}{n'''t}$.

III. $z-z'=d'$; $z-z''=d''$; $z-z'''=d'''$.

IV. $d'=\frac{z^2+v}{t}-\frac{z'^2+v}{n't}$; $d''=\frac{z^2+v}{t}-\frac{z''^2+v}{n''t}$; $d'''=\frac{z^2+v}{t}-\frac{z'''^2+v}{n'''t}$.

V. $t=\frac{z^2+v}{d'}-\frac{z'^2+v}{n'd'}=\frac{z^2+v}{d''}-\frac{z''^2+v}{n''d''}=\frac{z^2+v}{d'''}=\frac{z'''^2+v}{n'''d'''}$.

VI. By comparing the first of these equations successively with the second and third, and bringing the terms containing v to the same side, we obtain

$$v=\left(\frac{z^2}{d'}-\frac{z'^2}{n'd}-\frac{z}{d''}+\frac{z''^2}{n''d''}\right):\left(\frac{1}{d''}-\frac{1}{n''d''}-\frac{1}{d'}+\frac{1}{n'd'}\right)=$$

$$\left(\frac{z^2}{d'}-\frac{z'^2}{n'd}-\frac{z^2}{d'''}+\frac{z'''^2}{n'''d'''}\right):\left(\frac{1}{d'''}-\frac{1}{n'''d'''}-\frac{1}{d'}+\frac{1}{n'd'}\right).$$

"This equation contains only the squares of the values of z with known coefficients; and if we substitute $z-d'$, $z-d''$, and $z-d'''$ for z', z'', and z''', respectively, we shall obtain an equation in the form $ez^2+fz=g$, whence $z=\pm\sqrt{(g+\frac{1}{4}f^2)}-\frac{1}{2}f$.

"T. Y."

LXV. *On the Length of the French Mètre estimated in Parts of the English Standard.* By Captain HENRY KATER, F.R.S.*

ONE of the objects of the Committee of the Royal Society appointed for the purpose of determining the length of the seconds pendulum, being the comparison of the French mètre with the British Standard Measure, two mètres were procured from Paris for that purpose, the one made in the usual manner, and called the *mètre à bouts*, and the other a bar of platina, on which the length of the mètre is shown by two very fine lines; this is named the *mètre à traits*.

The width of the *mètre à bouts* is one inch, and its thickness 0·3 of an inch. On one side the word "METRE" is engraved,

* From the Transactions of the Royal Society for 1818, Part I.

and

and on the other "FORTIN à Paris." The terminating planes are supposed to be perfectly parallel, and the distance between them is the length of the mètre.

The *mètre à traits* is the same width as the *mètre à bouts*, but only a quarter of an inch thick. The lines expressing the length of the mètre are so fine that one of them is scarcely perceptible even with the assistance of a microscope, unless the light be very favourable. The situation of the lines may however be discovered by two strong black dots, made with a graver at the extremities of each, and a fine line crosses them at right angles, to indicate the parts from which the measurements are to be taken.

This mètre, previous to being brought from Paris, was compared with a standard mètre by M. Arago, with all that care and ability which he is so well known to possess, and which so delicate an operation requires. The result was, that the distance between the lines was found to be less than a mètre by $\frac{17 \cdot 59}{1000}$ of a millimètre, or ·00069 of an inch.

The same micrometer microscopes were used in the comparisons which I am about to detail, as have been already described in my account of experiments on the length of the pendulum in the Philosophical Transactions of the present year; and as the length of the mètre is nearly 39·4 inches, I was enabled to refer it to the same divisions of Sir George Shuckburgh's scale as I had employed in the measurement of the pendulum.

I commenced with the *mètre à traits*. It was placed in contact with the standard scale, their surfaces being in the same plane. An excellent thermometer was laid upon the scale, and a piece of thick leather was placed upon its bulb in order to prevent its being affected by heat from the person of the observer.

The whole was suffered to remain in this state for two or three days; after which the following observations were made at various times, the microscopes being brought alternately over the mètre and the scale. The value of each division of the micrometer is $\frac{1}{23363}$ of an inch*.

* For the manner in which this value was obtained, see page 175 of the preceding paper.

Com-

Comparison of the *Mètre à traits.*

Temperature.	Reading of the microm. at 39·4 of the scale.	Reading of the microm. at the mètre.	Divisions to be deducted from 39·4 inches.	Distance in inches between the lines designating the mètre.	Correction for temperature in decimals of an inch.	Distance in inches between the lines designating the mètre, the mètre being at 32°, and the scale at 62°.
60·0	85·0	644·5	559·5	39·37606	·00604	39·37002
60·7	75·5	639·0	563·5	39·37588	·00589	39·36999
61·7	69·2	634·0	564·8	39·37583	·00568	39·37015
62·0	65·0	630·5	565·5	39·37580	·00562	39·37018
62·4	61·0	629·0	568·0	39·37569	·00554	39·37015
62·3	58·7	629·5	570·8	39·37557	·00556	39·37001
62·2	58·0	628·0	570·0	39·37560	·00558	39·37002
62·2	59·0	625·0	566·0	39·37577	·00558	39·37019
62·1	59·0	625·5	566·5	39·37575	·00560	39·37015
58·8	90·0	638·0	548·0	39·37654	·00629	39·37025
59·0	83·0	637·0	554·0	39·37629	·00625	39·37004
59·0	82·0	636·0	554·0	39·37629	·00625	39·37004
59·2	83·2	632·0	548·8	39·37651	·00621	39·37030
59·1	81·0	632·0	551·0	39·37642	·00623	39·37019
					Mean	39·37012
The distance between the lines designating the mètre was found by M. Arago to be too little by a quantity = ·00069 of an inch, which add						+000·69
The distance from zero to 39·4 of Sir G. Shuckburgh's scale is too short compared with the mean of its divisions by ·00005 of an inch, which subtract*						—·00005
Hence the length of the mètre in inches of Sir G. Shuckburgh's scale is						39·37076

The comparison of the *mètre à bouts* presented considerable difficulties, which I conceive it would be of little use to detail, as the necessity of comparisons of this kind is of very rare occurrence; I shall therefore proceed to describe the method which was at last found successful.

Four rectangular pieces of brass were prepared precisely similar to those described in the account of experiments on the pendulum in the Philosophical Transactions before referred to. These were marked C, *c*, D and *d*. The perfectly plane rectangular edges of the pieces C and *c*, being placed in contact, and kept thus by means of a spring, the distance of the fine lines drawn on their surfaces, parallel and very near to the rectangular edges, was found to be 500·5 divisions of the micrometer; and the pieces D and *d* being placed in like manner in contact, the distance of the lines on their surfaces estimated in the same divisions was 456·7.

The *mètre à bouts* being placed by the side of the brass scale and in contact with it, the pieces D and *d* were applied to its extremities, the surfaces of the brass pieces being a little below the surface of the mètre in order to preclude any error which

* See page 176.

might have arisen from the edges of the mètre projecting beyond its terminating planes. Each of the brass pieces was supported in this position upon a piece of lead of a sufficient thickness, and kept in close contact with the end of the mètre by means of a slight spring bearing against a pin driven perpendicularly into the lead.

In order to ensure a perfect contact between each brass piece and the terminating plane of the mètre, a flat ruler of brass was laid upon the surface of the mètre so as to project beyond its extremity, and the end of the lead was elevated or depressed so that the line of light seen between the piece of brass and the ruler, the eye being level with the surface of the brass, apppeared to be equal in every part; when it was inferred that the surfaces of the mètre and of the piece of brass were parallel, and consequently that their rectangular ends were perfectly in contact.

The distance between the lines on D and *d* was now taken by the microscopes, and transferred to the scale in the manner before described; and when a sufficient number of comparisons had thus been made, the pieces D and *d* were exchanged for those marked C and *c*, and the observations repeated with every precaution to ensure an accurate result, especially with respect to temperature.

The under surface of the mètre was then placed uppermost, and the apparatus being arranged as before, the same process was pursued as that which has just been described. The results are contained in the following tables.

Comparison of the *Mètre à bouts.*

The pieces D and *d* applied. Distance from D to *d*, 456·7 divisions. The word *Mètre* above.							
Temperature.	Reading of the microm. at 39·4 of the scale.	Reading of the micrometer at the brass pieces.	Difference.	Divisions to be deducted from 39·4 inches.	Length of the Mètre.	Corr. for temperature in decimals of an inch.	Length of the Mètre, the mètre being at 32° and the scale at 62°.
59·7	9 5	91·5	82 0	538·7	39·37694	·00610	39 37083
54·8	38·0	97 2	59·2	515·9	39·37792	·00713	39·37079
55·0	39·0	98·0	59·0	515·7	39·37793	00709	39·37084
55·1	36·2	95·0	58·8	515·5	39·37794	·00707	39·37087
55·2	36·0	95·0	59·0	515·7	39·37793	·00705	39·37088
						Mean	39·37084
The pieces C and *c* applied. Distance from C to *c*, 500·5 divisions. The word *Mètre* above.							
55·6	30·7	47·5	16 8	517 5	39·37786	·00696	39·37090
55·7	30 0	47 3	17·3	517·8	39·37784	·00694	39·37090
55 9	30·2	47 5	17·3	517·8	39·37784	·00690	39·37094
56·2	24·5	45·0	20 5	521·0	39·37770	·00684	39·37086
56·3	23·0	44·7	21 7	522 2	39 37765	·00681	39·37084
						Mean	39·37089

The pieces C and *c* applied. Distance from C to *c*, 500·5 divisions. The word *Fortin* above.

Temperature.	Reading of the microm. at 39·4 of the scale.	Reading of the microm. at the brass pieces.	Difference.	Divisions to be deducted from 39·4 inches.	Length of the Mètre.	Corr. for temperature in decimals of an inch.	Length of the Mètre, the mètre being at 32°, and the scale at 62°.
56·8	15·0	37·5	22·5	523·0	39 37762	·00671	39·37091
56 7	15·7	40·0	24·3	524·8	39·37754	·00673	39·37081
56·8	14·7	40·5	25·8	526 3	39 37747	·00671	39·37076
56·8	15·5	40·0	24·5	525·0	39·37753	·00671	39·37082
56·8	15·5	40 0	24·5	525·0	39 37753	·00671	39·37082
						Mean	39·37082

The pieces D and *d* applied. Distance from D to *d*, 456·7 divisions. The word *Fortin* above.

55·0	35·3	93·0	57·7	514·4	39·37798	·00709	39·37089
55·2	36·5	95·0	58·5	515·2	39·37795	·00705	39·37090
55·2	36·0	93·0	57 0	513·7	39·37801	·00705	39·37096
56·0	26·5	89 0	62·5	519·2	39·37778	·00688	39·37090
56·0	23·5	87 0	63·5	520·2	39·37773	·00688	39·37085
						Mean	39·37090

Summary of the preceding comparisons.

The word "*Mètre*" above ..	D and *d*, 39·37084 C and *c*, 39·37089	39·37087
"*Fortin*"	C and *c*, 39·37082 D and *d*, 39·37090	39·37086
	Mean	39·37086
Subtract for errror in division of the scale ..		—·0005
Length of the *Mètre à bouts* in inches of Sir G. Shuckburgh's scale		39·37081

The following is the manner in which the correction for temperature was obtained. The expansion of platina according to the experiments of Borda and others, is ·00000476 parts of its length for one degree of Fahrenheit; and as this is the expansion used by the French in adjusting the length of their mètre, it must be employed on the present occasion. The mètre being taken at 32°, the expansion for the difference between this and the temperature of measurement must be subtracted from the apparent length of the mètre. The English standard temperature is 62°: therefore, if the temperature of measurement be under this, the expansion of the scale for such difference of temperature must be deducted from the length of the mètre before obtained. These two corrections are combined in the column entitled "Correction for temperature." Sir G. Shuckburgh's standard scale is of cast brass; and as I could not conveniently determine its actual expansion with that degree of accuracy that would have satisfied me, I have taken for it, the mean result of two ex-

 periments

periments made on plate brass, which gave me an expansion of ·0000101 parts of its length for one degree of Fahrenheit. The mean of most of the experiments made on the expansion of brass gives ·0000104, and had I employed this last number instead of my own, the difference in the length of the mètre would have been utterly inconsiderable.

Supposing then both mètres to be of equal authority, we have for the length of the *mètre à traits* 39·37076, and for that of the *mètre à bouts* 39·37081 inches; the mean of which, 39·37079, may be taken for the length of the mètre in inches of Sir G. Shuckburgh's standard scale when each is brought to its proper temperature *.

London, November 1817.

* The length of the mètre compared with Bird's Parliamentary standard is 39·37062 inches.

LXVI. *On the Preservation of Seeds, the Use of Lime in Agriculture, and former State of Cultivation in Scotland.* By Mr. Gavin Inglis.

To Mr. Tilloch.

Sir, — I do not know what philosophical attention may have been bestowed on the self-preservation of seeds, or the apparent spontaneous evolution of indigenous plants. The subject is certainly interesting, and may well claim the attentive regard of those whose leisure and abilities may be fit for this branch of instructive knowledge. From the little experience and scanty opportunity of observation that have fallen to my lot, I can do nothing towards the elucidation of so important a research; but from the memorandums of what has come within my practice, I shall select a few occurrences that struck me as deserving of notice, and which, although not conclusive in themselves, may still be of some use to others better qualified for the task. From what I have observed, I am very much inclined to think that seeds, particularly those of the oily kinds, when mixed with cold earth, and lying at a depth in dry soil beyond the congenial influence of the sun's vivific rays, will never lose their vital atom, or vegetative principle, but will remain for ever dormant, unless b design or accident the substratum be raised, and the seed-bed brought within the reach of the sun's influence. These seeds must owe their incorruptibility to some self-preserving principle, and their dormancy to the debarred approach of the solar streams of light. Without admitting some such ratiocination, how is it possible to reconcile or account for the spontaneous production of the great variety of plants and flowers, apparently new

new to the spot, that mere culture calls into existence, or which the bare melioration of a once waste surface, without at all disturbing the dormant subsoil, has been known to produce? These vegetables can only have been protected from corruption, by a self-protecting principle inherent in previously existing seeds or germs, now by the hand of cultivation roused from their lethargic bed, and brought within the penetrating power of oxygenous light, displaying their love of life, in their tenaciously contending with, and overcoming, the utmost efforts of the cultivator to destroy their priority of right to unfold their beauties on their natal soil.

I have known in swards whose surface to all appearance had lain for ages in undisturbed repose, (when broken up and the subsoil turned towards the sun, and the new surface pulverised by the rake or the harrow,) an entire new race of vegetables in due time rise, take place of those now subsoiled, claim possession of the land of their nativity, and flourish in all the exuberant pride of new existence.—Lime the field, and a still greater abundance and variety will be produced.

A flower border that had been overrun with poppies was trenched down to thicken the soil, and get quit of the incumbents: sixteen years after, when there was not, and had not been a poppy in the garden for some time,—for experiment sake I retrenched the same ground, and brought back the buried soil to the surface, and a most plenteous blow of the strongest poppy and finest flower was produced. The seed of these plants must have lain in a quiescent state during the whole of that period, secluded from the life-giving rays of light, and secured against corruption and decay by the sheltering shield of nature, interposed to save her banished offspring from extinction.

A plot of red brocoli had been allowed to shoot, and was in full flower; part of the under blossom had recently dropped off, and the seed-pod but barely formed, when the whole was hacked down with the spade, and buried in digging for a new crop. Next season, when this ground was again turned over, the brocoli leaves and stems were found completely consumed, except the more ligneous fibres of the roots and under stock. In a short time after, I was very much surprised to observe the new made ground, that had received neither seed nor plant for that season, completely covered with a seed leaf of one uniform shade and appearance. Upon examination I found it was no common weed, but could not allow myself to suppose it was brocoli. It however turned out to be so, and allowing it to stand, I had the most abundant crop of brocoli plants; nor did the whole evolve the first year. The blow of brocoli continued for a succession

 of

of years, upon every subsequent turning over of the surface, regularly diminishing in numbers, but did not entirely disappear for a series of years. These plants must have been produced from seed of the brocoli completely fructified even in embryo, and while yet in flower, and in this very early stage of maturization, having acquired all the requisite principles to preserve and fortify it against corruption. I am of opinion that these germs might have remained in a quiescent state of complete preservation for myriads of ages, had nothing occurred to disturb their repose, ready to burst their fetters at any future period of the world, on being turned up to the vivifying light of the sun, and pour forth their foliage and flower in gratitude to their original creator and all-powerful preserver.

I remember a cart loaded with lime, *hot* from the kiln, accidentally breaking down while passing through Auchmoor. Not to impede the road, the lime shells were removed from the cart and laid on a spot adjoining, closely covered with moss and short bushy heather. Before another cart could be procured to carry off the lime, a heavy shower fell, and had considerably slacked the shells. Notwithstanding all the care in gathering it up, a portion of the dusty mineral was unavoidably lost, and remained amongst the moss and roots of the heather. Subsequent rains washed it completely into the turf. The moss and heath were soon destroyed, and finally died away. The frosts of the following winter opened the surface soil; succeeding thaws and rains washed the lime into the softened earth, dissolving and sinking deeper and deeper by every returning shower, till the lime, completely mixed and neutralized with the soil, having laid bare the once moss-grown and heath-covered spot, now gave birth to a more congenial race, which was soon seen pouring forth, and still continues to teem with an overflowing vegetation of all the richest grasses of the climate, intermixed with native white clover of the sweetest flavour. The fresh-burnt lime could neither contain nor communicate the germs of this vegetable race. Their primitive identity must have previously lived in this soil, but become dormant by the germs being hid under the matted turf, and the all-animating rays of light and heat (which I am inclined to consider only separate terms for the same vivific substance) completely debarred and excluded by the thick covering of moss and heather, now removed by the renovating action of the mineral.

Many of the greatest acquisitions now possessed by man, and improved by intelligence and industry, have been of accidental discovery; and some such occurrence as the foregoing must first have laid open to human view the meliorating quality and great importance of lime to the progressive improvements in agriculture,

ture, which, although comparatively of very recent application by the present race, has certainly been of the highest utility, and in many districts, under intelligent management, has produced most wonderful effects. These beneficial effects, however, have been chiefly confined to the earlier districts and infield lands. The great and incalculable results to be derived from a liberal application of this invaluable mineral, I am of opinion, have in a great measure been lost to the outfield and upland districts, by its being far too sparingly bestowed, not excepting divisions of the country where it is abundant and cheap. The matured improvement of these lands can only be retarded from the proprietors or cultivators not being sufficiently conversant with the great chemical changes effected by the free use of this corrective stimulant. To late, cold, stubborn soils, it is almost impossible to estimate its value or appreciate its worth, or to overdo such ground with quantity, provided it be duly wrought into and intermixed with the soil. Upon the contrary, an abundant and repeated application, with intelligent and attentive workings, will have the effect of creating an artificial climate, which never can be otherwise attained, and never will fail in rendering the spot on which it has been copiously applied, a marked degree earlier in all time coming than all the surrounding country of the same stratum. Its plentiful and superabundant application has the effect of changing the original colour of the soil, from whatever may have been its primitive aspect, through all the various gradations of hue to the deep absorbing black, adding additional climate in every darkening shade, by communicating an increased capacity to absorb, retain and digest the nutrific life-disclosing rays of light, and, in due proportion as adding to this attractive and conducive power, deducting from the local altitude or latitude of the field. Hence the absurdity of starving the land in cold, late countries, whitening the cankered steril surface with the same sparing, niggardly parsimony, that an old gutless miser would dust the antiquated curls of his great-grandfather's musty wig, from the scanty portion of flour just measured out to prepare a saltless dumpling, or tasteless pudding, the fashionless shove-over for a stingy meal. Disappointed in what the spiritless parsimony of the occupant considers or expected as an adequate return for the money and labour bestowed in this over-rated exertion, the free use of lime has in many instances been neglected, blaming the non-effect instead of the non-application of the mineral on certain soils; whereas nothing was wanting but spirit and enterprise to bestow an everlasting blessing on himself and posterity by a liberal *lavishment* on the hitherto unproductive fields, to have secured a ten-fold compensation in proportion to the amplitude of the donation, by the return its fostering influence must have given, and adding to the soil a quality it never would have relinquished, and continued yearly gratefully to repay.

In travelling over the country, it has often most forcibly struck me, that Scotland must have been, at some very remote period of its history, under a far more extensive system of cultivation than it is at the present highly improved and enlightened state of the nation; and conducted with a knowledge and skill that the present incumbents are not very apt to allow their forefathers to have possessed. There are fields and pasture walks that bear strong and marked impressions of long continued cultivation, of which even the unprecedented prices of the late war have never tempted the present occupants to resume the ploughing. I am also of opinion, that the use of lime as a manure has been known to the cultivators of antiquity; not from any remains of it that can be traced in the soil, except the comparison of colour between the infield and outfield land; but from the extensive excavations of various lime rocks, which never could have been otherwise consumed. The royal residence, the baronial keep-safe, or churches and religious establishments, were the principal, perhaps the only architectural applications of this mineral in the early ages. The humble vassal and dependant were literally burrowed in the earth. But I am inclined to think that much of that excavation of lime must have been long before the commencement of ecclesiastical history, (from no monkish records being to be found regarding this,) and when the nation must have been far more populous than it is at present. No farmer could ever be stimulated to plough and raise grain without an adequate compensation; grain could only be cultivated to feed and be consumed. The ploughing and consumption of any country must always bear a due proportion; domestic consumption alone must have been the object, &c.

Yours ever,

Gavin Inglis.

LXVII. *New Experiments on some of the Combinations of Phosphorus. By Sir* H. Davy, *LL.D. F.R.S. Vice Pres. R.I.**

In a paper published in the Transactions of the Royal Society, for 1812, I have detailed a number of experiments on phosphorus, from which I deduced the composition of some of its compounds with oxygen, with hydrogen, and with chlorine. Since the appearance of this paper,various researches have been brought forward on the same subject, in which some results, differing very

* From the Transactions of the Royal Society of London, 1818, Part II.

much

much from each other, and from mine, are stated. I ventured to conclude that the phosphoric acid contained double the quantity of oxygen to that in the phosphorous acid; and thât phosphoric acid contained about 3-5ths of its weight of oxygen.

M. Berzelius considers the oxygen in phosphoric acid to be 128·17, and M. Dulong, 124·5, the phosphorus being 100. M. Dulong and M. Berzelius suppose the quantity of oxygen in phosphorous acid to be to that in phosphoric acid as 3 to 5.

The motive which immediately induced me to resume the inquiry respecting the phosphoric combinations, was M. Dulong's paper. This ingenious chemist has not only endeavoured to establish new proportions in the known compounds of phosphorus, but has likewise attempted to prove the existence of two new acids of phosphorus; and has denied several facts which I considered as sufficiently established.

The details which I have to lay before the Society in the following pages, will serve to correct and fix, I hope, with tolerable accuracy, the proportional number or equivalent of phosphorus, and at the same time will show the truth of the general series of proportions that I assigned to its compounds. In a case where my conclusions differ so materially from those of M. Berzelius and Dulong, it may be supposed that I have not adopted them without considerable caution; and I have preferred my own results to theirs, only because they have been confirmed by minute and repeated experiments.

I was certain from various experiments, made both long ago and recently, and the results of which had been confirmed by Mr. Brande, that the proportion of oxygen, which M. Dulong assigns to phosphoric acid, is considerably smaller than that denoted by the combustion of small quantities of phosphorus in oxygen gas. I knew that minute portions of phosphuretted hydrogen were separated from phosphorus by Voltaic electricity; and it occurred to me as possible, that water might be formed in the combustion of phosphorus, and separated from the phosphoric acid when it entered into saline and metallic combinations. To ascertain if this were the case, I passed phosphorus to saturation through red-hot lime in a green glass tube connected with a mercurio-pneumatic apparatus: the combination took place with vivid ignition; but no elastic fluid was produced. A portion of the phosphuret of lime formed, was introduced into a tray of platinum, and heated in a glass retort filled with oxygen gas; the phosphuret of lime burnt brilliantly, and became partly converted into *phosphate of lime;* but on restoring the original temperature of the retort, there was no appearance of *vapour* or of *moisture.*

Having examined the phosphate of lime formed in this operation,

tion, and satisfied myself that it was the same as that formed by other methods, it became evident that there were no sources of error in the experiments on the combustion of phosphorus in oxygen gas, arising from the formation or separation of water; and the only circumstance which could be urged against the accuracy of processes on this combustion, was the small quantity of materials* on which they had been made.

The vividness and rapidity of the combustion of phosphorus, renders it impossible to burn considerable quantities of phosphorus in the common way in glass vessels. Phosphuret of lime burns much more slowly and less intensely. I endeavoured to ascertain the quantity of oxygen absorbed by a given weight of phosphorus converted into phosphuret of lime; but the experiment did not succeed. Though the phosphuret of lime was in fine powder and distributed over a large surface, yet the phosphate of lime which formed and fused on the exterior, defended the interior of the phosphuret from the action of the oxygen, and prevented its combustion.

After various unsuccessful trials to convert considerable quantities of phosphorus into phosphoric acid by combinations containing oxygen, I at last thought of a very simple mode of burning phosphorus, which answered perfectly.

Phosphorus requires a considerable heat for its volatilization. By inclosing it in a small tube, so constructed that the phosphorus can burn in vapour only from the aperture of the tube, large quantities of it may be burnt by the heat of a spirit-lamp in a retort filled with oxygen, and the absorption of oxygen and the quantity of phosphoric acid formed may be minutely ascertained.

The accompanying sketch (Pl. V. fig. 1.) will give an idea of the apparatus. The neck of the little curved tube, or small distilling retort, after the phosphorus is introduced, is drawn out, and an aperture left of about 1-10th of an inch; it should not be smaller, or it becomes choked by the phosphoric acid formed. Regulating the heat by raising or lowering the spirit-lamp, the combustion may be carried on slowly, or rapidly, at pleasure.

Operating in this way, I have often burnt from 5 to 10 grains of phosphorus withous any accident, and ascertained exactly the quantity of oxygen absorbed: there is only one source of error —a quantity of phosphorus remains in the upper part of the tube, which cannot be burnt except by a greater heat than the retort

* A source of error might be suspected in carbon combined with phosphorus; but I have been convinced by experiments made on the action of chlorine on the phosphorus I employed, that it contained no appreciable quantity of carbon. I suspect that what is often taken for carburet of phosphorus, is in reality a red oxide.

will

will bear; and it is difficult to ascertain the precise weight of this, as the tube always unites with some phosphoric acid where it is red hot at its mouth; but this can be only a trifling source of error.

In these experiments, and in all the others detailed in this paper, I received much useful assistance from Mr. Faraday of the Royal Institution; and much of their value, if they shall be found to possess any, will be owing to his accuracy and steadiness of manipulation.

Experiment I.—Six grains of phosphorus. The small tube with the phosphorus weighed before the combustion 56·5 grains; after the combustion 50·9; so that it had increased 4-10ths; and this increase was in great measure from phosphorus that had escaped combustion; and when this was burnt out by a strong red heat, the increase of weight of the tube was under 1-10th: so that at least 5·9 of phosphorus had been converted into acid: 23·5 cubical inches of oxygen were absorbed: thermometer being at 46° Fahrenheit; barometer 29·6 inches.

Experiment II.—Ten grains of phosphorus. The glass tube containing the phosphorus weighed 103·1 grains; after the experiment 95·6; but much phosphorus remained unconsumed. After the tube had been heated to redness, it weighed 94 grains; so that at least 8·4 grains of phosphorus were consumed in the first process. The absorption of gas was 34 cubical inches. Barometer 29·8; thermometer 47°.

Experiment III.—Ten grains of phosphorus. By weighing the tube after the experiment, and then distilling and burning the residual phosphorus, it was found that 9·1 grains of phosphorus had been burnt, which had absorbed 35·25 cubical inches of oxygen. Barometer 29·7; thermometer 49° Fahrenheit.

I give these experiments as the most accurate I have made. The pressure and temperature vary so little, that the corrections for them are of no importance. Supposing that 100 cubical inches of oxygen (the barometer being between 29·8 and 29·6, and the thermometer between 46° and 49° Fahrenheit) weigh 33·9 grains, phosphoric acid will be composed, according to the first result, of 100 phosphorus to 135 oxygen; according to the second, of 100 to 137·2; and according to the third, of 100 to 131·3: the mean will be 100 to 134·5.

The light of the phosphorus burning in vapour in these experiments was excessively bright; yet the top of the retort never became softened; and the phosphoric acid, which increased the weight of the tube, principally combined with the glass at the aperture where it was red hot. I cannot but consider this process of burning phosphorus in the gaseous state in a great excess of oxygen, as the most accurate mode that has yet been devised

vised for ascertaining the composition of phosphoric acid. In this instance no phosphorous acid, as I ascertained by direct trials, is formed form the vapour; and no substances are concerned except those that actually combine. M. Dulong's method of ascertainining the composition of phosphoric acid, appears to me much too complicated to afford any results approaching to accuracy. He first combines copper wire with phosphorus, by passing phosphorus over it by means of a stream of hydrogen gas; he then dissolves his phosphuret of copper in nitric acid, and determines the quantity of phosphoric acid formed by precipitation: in all of which processes sources of error may exist.

M. Berzelius's methods of ascertaining the composition of phosphoric acid, that of reviving gold from its oxide by means of phosphorus, and that of determining the quantities of phosphate and muriate of silver formed from perphosphorane, or the perchloride of phosphorus, appear to me still more exceptionable; yet his results on the quantity of oxygen approach nearer to mine than those of M. Dulong.

The facts which I endeavoured to establish respecting chlorine, in a paper published in the Philosophical Transactions for 1810, show that the proportional or equivalent volume in which chlorine combines, is to that in which oxygen combines, as 2 to 1; and it follows, that 10 grains of phosphorus in forming the white sublimate, or perchloride, ought to combine with between 76 and 80 cubical inches of chlorine.

In experiments that I formerly made on this subject, by admitting chlorine to phosphorus in exhausted vessels, and ascertaining the absorption by introducing solution of chlorine, I overrated the absorption. I did not at that time know, what I have since ascertained, that a solution of chlorine in water, *apparently* saturated with chlorine, by agitation with it in long narrow vessels, will still take up more, by exposure to a great surface of chlorine in larger vessels. Under all circumstances, it is difficult to gain very precise results in experiments on the action of phosphorus on chlorine. Mercury acts so rapidly upon chlorine, that it cannot be employed in experiments in which the absorption is to be determined. When common water is used, some of the gas is absorbed by the water, and, the sublimate being a very volatile substance, its vapour always increases the volume of the residual gas. Some aqueous vapour likewise, in experiments over water, enters with the gas, which forms a volatile hydrate, the effect of which is likewise to diminish the apparent absorption of chlorine.

I have always found the absorption greatest, when I have operated in small retorts, connected by small stop-cocks with the vessel containing the chlorine, over water. Making the proper corrections

corrections for the absorption by the water, the apparent absorption has been from 35 to 38 cubical inches for every five grains of phosphorus.

M. Dulong's two methods of ascertaining the quantity of chlorine in the sublimate, appear to me at least as objectionable as his process for determining the composition of phosphoric acid, and liable to great errors: the first from the uncertainty of the absolute quantity of chlorine admitted; and the second, from the loss arising from the vapour of the sublimate, which must be carried off by the current of chlorine. How great a deficiency may originate from the last circumstance, is shown by the following experiment. Five grains of phosphorus were converted into sublimate by chlorine in great excess, the remaining chlorine was displaced by passing common air through the vessel for some time, till not the slightest smell of chlorine could be perceived; the retort was then weighed, and a current of air passed through it. Though this current could hardly have replaced the air contained in the retort, yet the loss of weight was 1·7 grain, and copious vapours were produced in the atmosphere. In a second trial of the same kind, there was a greater loss of weight: and by barely exhausting the retort, and then again admitting air, there was a loss of 7-10ths of a grain.

When chlorine is made to act upon phosphorus over mercury not carefully dried, some muriatic acid gas is always formed; but when the mercury has been recently boiled, no effect of this kind is produced, and the vapour in the gas forms a minute quantity of a liquid hydrate of the perchloride, which, by more water, is converted into muriatic and phosphoric acids, as I proved by some very delicate experiments; so that there is certainly no hydrogen denoted in phosphorus by the action of chlorine, and in their mutual action a mere binary compound of the two substances is formed.

After reflecting much upon the methods of combining chlorine and phosphorus, so as to gain correct results, it occurred to me, that in operating over water, and introducing a perfectly saturated solution of chlorine to absorb the vapour of the sublimate and of its hydrate formed from the water in the chlorine, I should gain a result nearly correct. I made an experiment in this way on four grains of phosphorus, in a retort containing 13 cubical inches. I ascertained the absorption, introduced into the retort a tube, containing about half a cubical inch of saturated solution of chlorine, and suffered the fluid slowly to act upon the sublimate, cooling the retort by immersion in water: I then ascertained the degree of the second absorption, which was nearly a cubical inch and a half. I likewise ascertained that water had its powers of dissolving chlorine diminished,

nished, and not increased, by uniting with phosphoric and muriatic acids, so that the apparent absorption must have been less than the real one. Adding the second absorption to the first, and making the proper corrections, the quantity of chlorine uniting to four grains of phosphorus was 31·9 cubical inches; barometer being 30·1 inches, and thermometer 46° Fahrenheit.

Rather a larger proportion would be given, if the correction for the presence of vapour had been made for some of the other experiments: and the result agrees exactly with the mean deduced from the absorption of oxygen in the formation of phosphoric acid; for, assuming that 100 cubical inches of chlorine weigh 76·5 grains, then the sublimate will consist of one of phosphorus to nearly six of chlorine; and taking the composition of phosphoric acid from this datum, it would consist of 100 of phosphorus and 135 of oxygen.

To ascertain the composition of phosphorous acid, I used a new method, that of converting the perchloride of phosphorus, or perphosphorane by phosphorus, into the chloride which affords phosphorous acid by the action of water. This is easily done by heating them together in a close retort; and it enables us to determine with certainty, which opinion is correct, *that* assuming the oxygen in phosphorous acid to be three, or *that* which supposes it to be 2·5, the oxygen in phosphoric acid being five.

Five grains of phosphorus were converted into perchloride in a small retort of the capacity of six cubical inches: it was necessary to exhaust this retort twice, to remove the residual common air mixed with the chlorine, and some perchloride must have been lost during this process. A small quantity of chlorine, which could have been little more than sufficient to compensate for the loss, remained in the retort. Five grains of phosphorus were introduced, and the retort suffered to remain filled, principally with common air; heat was very slowly applied; all the phosphorus, except an atom not so big as the head of a small pin, disappeared, and a little of the sublimate still remained, when the retort burst from the expansion of the vapour of the new chloride formed; but the chloride found on the fragments was pure, and held no phosphorus in solution.

A second experiment was made in a retort of the capacity of 11 cubical inches. Five grains of phosphorus were converted into perchloride: the retort was twice completely exhausted, by which at least a grain and a half or two grains of perchloride must have been lost. Five grains of phosphorus were introduced; a little of the sublimate was lost by falling into the stopcock of the retort; yet the conversion of the phosphorus by heat into the liquor was almost complete; there remained only

a minute

a minute fragment. In this experiment, however, the liquor held phosphorus in solution. When this phosphorus was precipitated by water, and obtained with the fragment by sublimation in a small glass tube, it did not equal 7-10ths of a grain, and was no more than could be expected from the loss of the sublimate.

These two experiments prove distinctly that the oxygen in phosphorous acid is half that in phosphoric acid; for if the proportion had been that which M. Dulong and M. Berzelius indicate, 1·67 grains of phosphorus, at least, ought to have remained after the action of the sublimate.

A collateral experiment was made. 32·7 grains of the fluid chloride, made by passing phosphorus through corrosive sublimate in great excess, were acted on by water, and precipitated by nitrate of silver; the precipitate was immediately separated from the fluid, after it had been greatly diluted with distilled water. Distilled water was then repeatedly passed through it, and it was dried and fused, when it weighed 98·4 grains; which, allowing 24·5 per cent. of chlorine in horn silver, would give the composition of the fluid chloride as 24·108 of chlorine, and 8·592 of phosphorus.

The comparative quantity of precipitate in this experiment was so much less than I had found in a former experiment, that, notwithstanding the care with which the process had been conducted, I resolved to make some more experiments of the same kind. In the first, in which the decomposition by water was made in a small bottle, from which no vapour could escape, and in which I superintended the weighing and drying of the horn silver formed, with the greatest care, 18·4 of the liquid chloride afforded only 54·5 of chloride of silver, which agrees as nearly as could be expected with the former experiment. In two other experiments, made with equal care, and in which the liquid was poured into a solution of nitrate of silver, six grains gave 17·1 of horn silver, and 29·4 gave 89·9 of fused horn silver.

In examining minutely the circumstances of the action of the liquid chloride, or solutions containing phosphorous and muriatic acids, or nitrate of silver, I found no difficulty in explaining the cause of the error in the former experiments. Phosphorous acid acts upon nitrate of silver, and more rapidly in proportion to its concentration, and gradually produces a copious precipitate from it; so that if there be an excess of nitrate of silver, and the precipitate be not immediately separated from the solution, there is always a considerable increase of weight. M. Dulong, and M. Berzelius, whose experiments agree with my former ones, *may* have been misled by a precipitation from the

nitrate

nitrate of silver by phosphorous acid, as I am sure *I* was. M. Berzelius does not state how he prepared his liquid chloride of phosphorus; but M. Dulong, who objects to my process by corrosive sublimate, and employs, instead of it, the action of chlorine on phosphorus in forming his fluid, must have been exposed to other sources of error. He speaks of acting on dry phosphorus by dry chlorine; but it must be always extremely difficult to free a gas that cannot be kept over mercury, of all its vapour; and as perchloride always forms during the action of phosphorus on chlorine, a part of which produces a fluid, and easily volatile hydrate with water, and soluble in 11 proportions in the liquid chloride, this process must be very liable to error. I have never been able to form the perchloride, even from chlorine slowly passed through muriate of lime, without producing a small quantity of liquid hydrate of perchloride, which, when the solid perchloride was converted into liquid by more phosphorus, rose in vapour with it, and which, containing nearly a double quantity of chlorine, (for the water forms a very small part of it,) occasions the precipitation of a much larger quantity of horn silver than the pure chloride formed from corrosive sublimate.

These various experiments on the combination of phosphorus with oxygen and chlorine, sufficiently agree with each other to afford the means of determining the proportion in which phosphorus combines with other bodies, or its equivalent number considered as an element.

If the absorption of oxygen be considered as offering the data, and phosphoric acid be supposed to consist of two proportions of oxygen, and one of phosphorus, the number representing the proportion in which phosphorus combines, will be 22·3. If phosphoric acid be considered as consisting of four proportions of oxygen, the proportional number or equivalent of phosphorus will be 44·6.

If the absorption of chlorine in forming phosphorane be made the datum, the number will be the same, 22·2, or the double, 44·4. If the quantity of horn silver formed from the liquid chloride, taking the mean of all the experiments, be assumed as the datum, the number would be 23·5, or the double 47: the mean of all these proportions is 22·6, or the double 45·2; or taking away decimals, 45.

In referring to the analyses which have been made of the different combinations of phosphoric acid, for the purpose of ascertaining if they correspond with this number, I found the data so uncertain and so discordant, that it was impossible to form any conclusions from them. The phosphate of soda, as is well known, has alkaline properties; yet, according to M. Berzelius, it it contains but 17·67 of soda to 20·33 of acid; whereas it ought to contain, according to the proportion indicated by my experiments, (if neutral,) nearly an equal weight of soda. M. Berzelius mentions several combinations of baryta and lime with phosphoric acid, of which only two approach to a correspondence with the number I have given for phosphorus; that containing 45·5 of acid to 48·7 of lime; and that containing 39·1 of acid to 60·8 of barytes. New researches are required to explain the anomalies presented by the phosphates.

I shall give three experiments on the quantity of hydrate of potassa necessary for saturating given quantities of phosphoric acid made from given weights of phosphorus.

Eighteen grains of phosphorus converted into phosphoric acid by combustion in oxygen, required for its saturation 47 grains of dry hydrate of potassa.

5·7 grains of phosphorus converted into acid, required 14·7 of hydrate of potassa.

Five grains of phosphorus converted into perchloride, demanded, to produce perfect neutralization, 68 grains of hydrate of potassa.

These three experiments agree so well with each other, and with the proportionate number gained from the absorption of chlorine and oxygen by phosphorus, that it is impossible not to put confidence in them.

If 13·1 be considered as the quantity of hydrate of potassa required to neutralize the phosphoric acid formed in the last experiment, and the 54·9 of hydrate remaining, be supposed to contain 43 grains of potassa, then the chlorine required to expel the oxygen from the potassa would be rather more than 40 cubical inches.

We owe to the ingenuity of M. Dulong the discovery of an acid, which he names the hypophosphorous acid, and which he supposes to contain half the quantity of oxygen in the phosphorous acid. I have satisfied myself as to the correctness of his views respecting the existence of this acid, and the properties of its compounds; but I cannot regard the method he has adopted for its analysis as entitled to confidence. He takes a given quantity of hypophosphite of soda, acts upon this by chlorine, converts the excess of chlorine into muriatic acid, precipitates by nitrate of silver and earthy salts, and from the comparison of all these data, in which some substances of uncertain composition may be concerned, draws his conclusions.

I have found that the neutral hypophosphite of barytes, when acted on by heat in close vessels, is converted into acid phosphate of barytes, disengaging an elastic fluid, which is almost

 entirely

entirely the hydrophosphoric gas, or phosphuretted hydrogen saturated with phosphorus. I say *almost entirely*, because in the beginning of the process, a little gas spontaneously inflammable is produced, and a minute quantity of moisture appears: and when the heat is raised to redness, a very little phosphorus is produced, probably from the decomposition of a part of the phosphoric gas. Now supposing the quantity of phosphoric acid in phosphate of baryta known, and the quantity of phosphorus in phosphuretted hydrogen known; it is very easy, from an accurate experiment on the decomposition of the hypophosphite of baryta, to learn the composition of hypophosphorous acid.

I made two experiments on this subject; in one, 50 grains of dry hypophosphite of barytes were used, and the distillation conducted in a small glass tube. About 23·25 cubical inches of gas were produced. The loss of weight of the apparatus could not be ascertained, as unluckily a little of the phosphate was lost; a small portion of phosphorus was deposited in the upper part of the tube, from the decomposition of a minute quantity of the bi-phosphuretted gas; but this could not have equalled the 4-10ths of a grain, as the tube only lost 4-10ths by being heated to whiteness.

In the second experiment, 29 grains of the hypophosphite were used, and the loss of weight only ascertained, which was 3·5 grains. To be able to form any opinion as to the composition of the hypophosphorous acid, it was necessary to ascertain the composition of the phosphate of baryta produced in these experiments; which was easily done by precipitating a given quantity of the hypophosphite of barytes by sulphate of soda in solution. Fifteen grains of hypophosphite of barytes, in an experiment very carefully made, afforded 11·3 of sulphate of barytes. Now, supposing this sulphate of barytes to contain 7·4 of baryta, the hypophosphite would consist of 7·4 of baryta, and 7·6 of hypophosphorous acid; and 13·1 of the acid phosphate of baryta, formed from its decomposition, would contain 5·7 phosphoric acid, and 7·4 baryta. And in the experiment in which 29 grains of hypophosphite of baryta were decomposed, supposing the whole loss of weight to be owing to perphosphuretted hydrogen given off, and this gas to be composed of 22·5 of phosphorus to 4 of hydrogen, or of 5·29 hydrogen to 29·76 phosphorus, and the 25·5 of acid phosphate remaining composed of 14·47 baryta nearly, and 11·03 phosphoric acid, adding the 29·76 of phosphorus to the 4·72 in the phosphoric acid, and subtracting 39, the quantity of oxygen required to form water with the 5·24 of hydrogen, the hypophosphorous acid may be conceived to be composed of 7·69 phosphorus,

phosphorus, and 2·54, which denotes rather less than half the oxygen in phosphorous acid: i. e. as 7·43 to 1·5, an approximation nearer than could have been expected.

Assuming the composition of the phosphuretted gas to be what is stated in the preceding page, which agrees very nearly with an experiment which I formerly made, the first experiment on the quantity of gas disengaged would give a proportion of oxygen rather less than that which has been just calculated upon; but it must be remembered, that a certain quantity of common phosphuretted hydrogen is produced, which containing less hydrogen in a given volume, would sufficiently explain the difference of result.

M. Dulong has advanced an ingenious opinion, that the hypophosphorous acid *may be considered* as a triple compound of hydrogen, oxygen, and phosphorus. There is another view which may be taken of its composition, namely, that it may be a compound of phosphoric acid and perphosphuretted hydrogen. Phosphuretted hydrogen, as may be deduced from some experiments of M. Dulong, has the properties of a very weak alkali; and when expelled from the neutral hypophosphites, they become acid. This view agrees very well with the equivalent, or proportional numbers, which represent phosphoric acid and phosphuretted hydrogen. If it be adopted, the hypophosphites must be considered as triple compounds, analogous to the salts containing fixed alkali and earths, or ammonia and earths combined with acids.

M. Dulong imagines that the acid formed by the slow combustion of phosphorus in the air, and which I have supposed to be a mixture of phosphorus and phosphoric acids, is a peculiar acid, a chemical compound of phosphorous and phosphoric acids, which he names phosphatic acid. I cannot say that his arguments give much probability to this opinion. This substance has no crystalline form, no marked character which distinguishes it from a mere mixture of phosphorous and phosphoric acids; and as far as my experiments have gone, it is far from uniform in its composition; and phosphorous and phosphoric acids mixed together, produce a substance of exactly the same kind.

That a mixture of phosphorous and phosphoric acids should be produced by the slow combustion of phosphorus, is not surprising, when it is considered that this phænomenon is connected with different chemical processes, viz. the action of the vapour of phosphorus upon air, the action of solid phosphorus upon the elastic atmosphere, and upon the air dissolved in the moisture attracted by the acids formed; and, unless vapour be present in the air, the process of the slow conversion of phosphorus into acids soon stops.

 I have

I have mentioned in the paper to which I have referred, in the beginning of this communication, that the hydrophosphorous acid is decomposed by heat; and that phosphoric acid, and perphosphuretted hydrogen are the results. In examining the nature of the phosphoric acid formed, I find that it contains water, so that it is a hydrated phosphoric acid. In carefully conducting the experiment, I find likewise, that a small proportion of water is given off with the perphosphuretted gas. I shall give the results of an experiment: 17·5 grains of hydrophosphorous acid were decomposed by heat in a small glass retort carefully weighed; 6·5 cubical inches of elastic fluid were generated, and the loss of the retort was four grains. Now, if it be assumed that the hydrate of phosphoric acid * remaining equalled 13·5 grains, and that it contained, according to the law of definite proportions, 1·88 of water, and that the bi-phosphuretted gas weighed 1·937, and consisted of 1·6446 phosphorus, and ·2924 hydrogen; then the oxygen in the phosphorous acid will be to the phosphorus as 44 to 66, which is as near a result as can be expected.

For 4 proportions of phosphorous acid are	300	or the double	150
and 10 of water	170	or	85
which together amount to ..	470	or	235
which form 3 proportions of phosphoric acid	315	or	157·5
with 3 of water to form the hydrate	51	or	25·5
	366		183·0
4 of water decomposed, of which the hydrogen is 8, to form with 45 of phosphorus phosphuretted hydrogen	53	or	26·5
3 of water given off	51	or	25·5
making	470	or	235·

I have no doubt that the acid which I used formerly was drier than the acid employed in this experiment, which will account for the difference of the result. Supposing a hydrophosphorous acid could be procured, containing only the quantity of water sufficient to convert it into dry phosphoric acid, it would consist, as I have stated in my former paper on phosphorus, of four proportions of water, and four proportions of phosphorous acid.

I have adopted throughout the whole of these calculations, the supposition that the hydrogen in water is to the oxygen as 2 to 15: and consequently I have taken the number representing oxygen as 15, which is extremely convenient, as the multiples are simple, 30, 45, 60, &c. Taking the proportion of

* I proved it to be a hydrate by heating it with magnesia, when abundance of water was given off from it.

phosphoric

phosphoric acid in phosphate of potassa, which may be deduced from the experiments, page 449, it appears more convenient to represent the proportional number, or equivalent of phosphorus, by 45, or 45·2, than by 22·5, or 22·6, which gives facility in adopting either hypothesis of the composition of hypophosphorous acid. If it be supposed a simple compound of oxygen and phosphorus, the series of proportions in the acids of phosphorus will be

Hypophosphorous acid,	Phosphorus 45	Oxygen	15
Phosphorous acid	45	Oxygen	30
Phosphoric acid	45	Oxygen	60
or hypophosphorous acid 263 ..	Phosphoric acid 2 proportions		210
	Phosphuretted Hydrogen 1 prop.		53

I shall conclude this paper by a few incidental observations on the compounds of phosphorus.

M. Dulong states that no phosphorous acid is formed when phosphorus is burnt in excess of oxygen or atmospheric air; as, he says, I have asserted. I cannot find that I have any where made such an assertion; but notwithstanding what M. Dulong pretends, the assertion is true, as the following experiment will prove. Half a grain of phosphorus was set fire to in a retort containing 16 cubical inches of common air; the acid products were washed with distilled water, and passed through a filter, and evaporated. When the acid became nearly dry, small globules of phosphuretted hydrogen were disengaged from it, indicating the presence of phosphorous acid. The experiment was repeated two or three times, care being taken to separate the red powder which has been considered as an oxide of phosphorus, and always with the same result.

Whenever phosphorus is inflamed, and suffered to become extinguished in oxygen gas in excess, unless the *product* is strongly heated after the spontaneous combustion is over, *an acid*, of which the hydrate produces phosphuretted hydrogen by heat, is always found in the products; and this acid is probably produced by the action of the solid phosphorus on the phosphoric acid in contact with it. This fact, and the circumstance, that much phosphorous acid is produced by the combustion of phosphorus in rare air, renders it almost certain that the phosphorous acid is a direct combination of phosphorus and oxygen, and destroys an idea which might otherwise be formed from the phænomena of the decomposition of its hydrate, namely, that it is a compound of three proportions of phosphoric acid, and one of phosphuretted hydrogen.

M. Dulong and M. Berzelius speak of freeing phosphorane, or the liquid chloride of phosphorus, from phosphorus, by distillation. In experiments made in the laboratory of the Royal

 Institution,

Institution, in which it has been twice carefully distilled at a low heat, it has still contained minute quantities of phosphorus.

It has been supposed that dry phosphoric acid is fixed at a white heat; but I find that this is not the case: it rapidly rises in vapour at this temperature, and evaporates even at the point of fusion of flint glass: and the hydrate of phosphoric acid is susceptible of being volatilized at a much lower temperature.

In converting the solid sublimate composed of phosphorus and chlorine into the liquid compound, when the phosphorus is first used in contact with the sublimate, a yellow crystalline mass is formed, which, when acted on by a higher degree of heat, affords the liquid chloride, which rises from it in vapour, and leaves phosphorus behind. It is possible that this yellow solid is a compound of phosphorus and chlorine, containing half as much chlorine as the liquid. Should this be proved to be the case by future exeriments, it will give weight to the idea, that the hypophosphorous acid is a binary compound of oxygen and phosphorus.

LXVIII. *Comparison between the Chords of Arcs employed by* Ptolemy *and those now in Use. By* G. A. Walker Arnott, *A.M. Edinburgh.*

To Mr. Tilloch.

Sir, — In a late elegant publication (The Philosophy of Arithmetic), it is stated that the ratio of 1 to 3·1416, or of the diameter to the circumference of a circle, must have been almost known to Claudius Ptolemy. This celebrated philosopher and mathematician, and first of ancient astronomers, left behind him, in the third book of his Almagest, a table of the chords of the arcs of the circle, calculated in sexagesimals to every 30′ or half-degree, and which are found to coincide with those in the trigonometrical tables we at present employ, with a much more considerable degree of exactness than could reasonably be looked for from the small advances made at that time in this subject. It is therefore my purpose here to exhibit a table of comparison between these, the insertion of which in your Magazine may gratify such of the curious as may not have seen the work itself of this distinguished man.

The *first* column contains the chords of every two degrees of the semicircle, as calculated by Ptolemy in *sexagesimals*. In the *second* column are the same chords converted into *sexagesimals* from our common *decimal* tables: and here I may add, that that number is taken, nearest to which, either above or below, the true number approaches, when extended to thirds, fourths, &c. of the radius. In the *third* are Ptolemy's calculations turned into decimals; and in the *fourth* we have an extract

tract from the trigonometrical tables now in use.—Before inserting the table, we may borrow the following example for the purpose of showing how near Ptolemy approached to our customary ratio of 1 : 3·1416 : the chord of 2° in sexagesimals to radius ξ or 60° is β ε μ, or 2° 5′ 40″, which in decimals is nearly ·034907 to radius 1, and this multiplied by $\frac{180^\circ}{2^\circ} = 90$ gives 3·1416 nearly : now as the chord of 2° is almost the measure of its arc, the above number may be taken as the length of the semicircumference.

Edinburgh, 12th November 1818.

D.

2°	2° 5′ 40″	2° 5′ 39½″	·0349074	·0349048
4	4 11 16	4 11 16½	·0697963	·0697990
6	6 16 49	6 16 49	·1046718	·1046720
8	8 22 15	8 22 15	·1395139	·1395130
10	10 27 32	10 27 31	·1743148	·1743114
12	12 32 36	12 32 36	·2090555	·2090570
14	14 37 27	14 37 27½	·2437361	·2437386
16	16 42 3	16 42 3	·2783472	·2783462
18	18 46 19	18 46 20	·3128657	·3128690
20	20 50 16	20 50 16	·3472963	·3472964
22	22 53 49	22 53 49½	·3816157	·3816180
24	24 56 58	24 56 58	·4158261	·4158234
26	26 59 38	26 59 39	·4498981	·4498022
28	29 1 50	29 1 50	·4838426	·4838438
30	31 3 30	31 3 30	·5176389	·5176380
32	33 4 35	33 4 35	·5512731	·5512748
34	35 5 5	35 5 4½	·5847454	·5847434
36	37 4 55	37 4 55	·6180324	·6180340
38	39 4 5	39 4 5½	·6511343	·6511364
40	41 2 33	41 2 33	·6840417	·6840402
42	43 0 15	43 0 15	·7167361	·7167358
44	44 57 10	44 57 10	·7492130	·7492132
46	46 53 16	46 53 16	·7814630	·7814622
48	48 48 30	48 48 30	·8134722	·8134732
50	50 42 51	50 42 51	·8452361	·8452366
52	52 36 16	52 36 16	·8767407	·8767422
54	54 28 44	54 28 44	·9079815	·9079810
56	56 20 12	56 20 12	·9389444	·9389432
58	58 10 38	58 10 38	·9696204	·9696192
60	60 0 0	60 0 0	1·0000000	1·0000000
62	61 48 17	61 48 16½	1·0300787	1·0300762
64	63 35 26	63 35 25	1·0598426	1·0598386
66	65 21 24	65 21 24	1·0892778	1·0892780
68	67 6 12	67 6 11	1·1183889	1·1183858
70	68 49 45	68 49 45	1·1471528	1·1471528
72	70 32 3	70 32 3	1·1755694	1·1755706
74	72 13 4	72 13 4	1·2036296	1·2036300
76	73 52 46	73 52 46	1·2313261	1·2313230
78	75 31 7	75 31 6½	1·2586435	1·2586408

D.

80	77° 8 5	77 8 4	1·2855787	1·2855752
82	78 43 38	78 43 37½	1·3121204	1·3121180
84	80 17 45	80 17 44½	1·3382639	1·3382612
86	81 50 24	81 50 23	1·3640000	1·3639968
88	83 21 33	83 21 32½	1·3893194	1·3893168
90	84 51 10	84 51 10	1·4142130	1·4142136
92	86 19 15	86 19 15	1·4386806	1·4386796
94	87 45 45	87 45 45	1·4627083	1·4627074
96	89 10 39	89 10 38½	1·4862917	1·4862896
98	90 33 55	90 33 54½	1·5094213	1·5094192
100	91 55 32	91 55 31	1·5320926	1·5320888
102	93 15 27	93 15 27	1·5542917	1·5542920
104	94 33 41	94 33 41	1·5760231	1·5760216
106	95 50 11	95 50 10½	1·5972731	1·5972710
108	97 4 56	97 4 55	1·6180370	1·6180340
110	98 17 54	98 17 54	1·6383056	1·638040
112	99 29 5	99 29 4	1·6580787	1·6580752
114	100 38 26	100 38 26	1·6773426	1·6773412
116	101 45 57	101 45 57	1·6960972	1·6960962
118	102 51 37	102 51 36	1·7143380	1·7143340
120	103 55 23	103 55 23	1·7320509	1·7320508
122	104 57 16	104 57 16	1·7492407	1·7492394
124	105 57 14	105 57 13	1·7658981	1·7658952
126	106 55 15	106 55 15	1·7820139	1·7820130
128	107 51 20	107 51 19	1 7975926	1·7975880
130	108 45 25	108 45 25	1·8126157	1·8126156
132	109 37 32	109 37 32	1·8270926	1·8270910
134	110 27 39	110 27 38	1·8410139	1·8410098
136	111 15 44	111 15 43½	1·8543704	1·8543678
138	112 1 47	112 1 47	1·8671620	1·8671608
140	112 45 48	112 45 47	1·8793889	1·8793852
142	113 27 44	113 27 44	1·8910370	1·8910372
144	114 7 37	114 7 36½	1·9021157	1·9021130
146	114 45 24	114 45 24	1·9126111	1·9126096
148	115 21 6	115 21 5	1·9225278	1·9225234
150	115 54 40	115 54 40	1·9318519	1·9318516
152	116 26 8	116 26 8	1·9405926	1·9405914
154	116 55 28	116 55 28	1·9487407	1·9487402
156	117 22 40	117 22 40	1·9562963	1·9562952
158	117 47 43	117 47 43	1·9632546	1·9632544
160	118 10 37	118 10 37	1·9696157	1·9696156
162	118 31 22	118 31 21	1·9753796	1·9753766
164	118 49 56	118 49 56	1·9805370	1·9805362
166	119 6 20	119 6 20	1·9850926	1·9850924
168	119 20 34	119 20 33½	1·9890463	1·9890438
170	119 32 37	119 32 36	1·9923935	1·9923894
172	119 42 29	119 42 28	1·9951343	1·9951282
174	119 50 9	119 50 8	1·9972639	1·9972590
176	119 55 38	119 55 37	1·9987870	1·9987816
178	119 58 55	119 58 54	1·9996991	1·9996954
180	120 0 0	119 0 0	2·0000000	2·0000000

LXIX. *On the Structure of the poisonous Fangs of Serpents. By* Thomas Smith, *Esq. F. R. S.* *

When the poisonous fangs of serpents are attentively examined, a slit or suture may be observed extending along the convex side, from the foramen at the base to the aperture near the point. (Plate V. A. B. C. D.) This is a consequence of an unusual, and hitherto, I believe, entirely unnoticed structure, resulting from the mode of formation of the tube through which the poison flows.

My attention was called to this structure, by having lately received from my friend Mr. Herbert Ryder, the assay master to the mint at Madras, the bones of the skull of a cobra de capello. I had some years since noticed the slit running along the convex side of the fang, in making a preparation of the head of the common viper of this country, in which it is distinctly seen when magnified; nevertheless, it seems to have been overlooked by all the numerous authors who have written upon the subject of the venomous fangs of the viper, and who, as far as structure is concerned, do not appear to have advanced beyond Pliny, to whom, and even anterior to whose time, the circumstance of their being tubular was well known.

All teeth being formed from a pulp, which has the shape that the tooth itself is destined to retain, it has probably been imagined that the tube of the poisonous fangs of serpents was produced by a perforation passing through the pulp; this is not, however, the case, the tube being completely external, and formed by a deep longitudinal depression on the surface of the pulp.

In order to render this more clear, I must here observe that a slight longitudinal furrow, or depression, is to be seen on all the teeth of the cobra de capello; on those which are nearest to the poisonous fangs it is most evident, and occupies the convex side of their curvature; it however is confined entirely to the parietes of the tooth, and does not at all affect the form of its cavity.

But in the poisonous fangs, this depression is sunk deep into the substance of the tooth, and occupies a portion of the space, which in the others is allotted to the cavity which contains that part of the pulp which remains when the tooth is completely formed; and the edges of the depression being brought together along the greater part of the tooth, form the slit or suture that I have before described, but, being kept at a distance at both extremities, there results a foramen at the base and at the apex.

* From the Transactions of the Royal Society for 1818, Part II.

That

That this is a correct view of the mode in which the poisonous tube is formed, receives additional support from what I have observed in a species of the genus *hydrus* of Schneider. In this serpent, as in many others nearly allied to it (*les hydres* of M. Cuvier), there are simple teeth on the same bone which supports the poisonous fangs. These teeth so much resemble the fangs, that it requires a very close investigation to distinguish between them; and this arises from the simple tooth having not only a longitudinal furrow exactly resembling the edges of the slit of the poisonous fang, but also a very visible cavity at the base, where the foramen occurs in the others; and I have even found a fine tube in a tooth of this sort; it was however confined to the parietes, and did not affect the cavity of the tooth.

To this gradation from a slight superficial furrow to a deep depression, may be added the fact, that no traces of either are observable in the teeth of those serpents which are not armed with venomous fangs: this I found to be the case in a large species of boa.

As a consequence of the structure that I have described, if a horizontal section be made of a poisonous fang, in which the edges of the longitudinal depression are rounded, we shall have a cylindrical cavity (the poison tube) nearly surrounded by a semilunar one (the cavity which contains the pulp). This is shown in the annexed drawings of the fangs of the *cobra de capello*.—(Pl. V. E. F. G. H.)

If, however, the edges of the depression should be angular (as in the rattle-snake), the horizontal section shows a figure somewhat different, the poison tube being more completely surrounded by the cavity which contains the pulp. This is shown in the drawing by the section of a fang of an unknown species of serpent, which has exactly the same form as that of the rattle-snake, but is twice as large. (Pl. V. I. K.)

In sections taken at different parts of the fang, the proportions between the poison tube and the cavity which contains the pulp will be different; the latter greatly increasing towards the base of the tooth; and near the apex the poison tube only will be seen, the fang at that part being solid. In a section also of a completely formed fang, the poison tube, at its anterior part, will be closely invested by the thickened parietes of the cavity which contains the pulp; this cavity however is never obliterated, but exists in all the teeth of serpents, even when they have arrived at their full growth.

In the fangs, when completely formed, the edges of the slit, or suture, are frequently soldered together; when they are angular, so large a surface comes in contact, that they appear to be united by bony matter; in the *cobra de capello*, where they are rounded,

rounded, though in very close contact, they do not cohere. In the viper, the slit seems filled up by the enamel, which being nearly transparent, a bristle in the poison tube may be seen through it, and causes an appearance as if the slit was open.

In the first case, therefore, there is no channel observable on the exterior of the tooth; the line of junction, however, of the edges of the slit is very distinctly marked: in the *cobra de capello* there is an external furrow from the foramen of the base to that of the apex, owing to the edges of the slit being rounded; the same is the case in those species of *hydrus* that I have examined.

I should observe, that the poison tube is not coated with enamel; for the membrane or capsule in which the tooth is formed, and from the inner surface of which it is well known that the enamel is deposited, does not pass between the edges of the slit into the poison tube: as, however, it passes over the slit, it will cover it with enamel, and in some cases, by that means alone, the edges become soldered together.

As some excuse for the errors which may be found in this paper, I must observe, that many of my observations have been confined to small teeth of a species of *hydrus*, which I was therefore obliged to dissect under the microscope.

I have to thank Sir Everard Home for the great interest that he has taken in the object of my inquiry, and for the assistance which he has afforded me; on the value of which it would be needless to enlarge before the members of this Society.

The drawings annexed to the paper will sufficiently attest my obligations to Mr. Clift. I owe much to him, in addition, for the zeal with which he exhibited to me every thing in the Museum of which he has the custody, that was likely to promote my views, and for information upon several points, which was required in the progress of the investigation.

Description of Plate V.

a, b, c, d, are representations of the poisonous fangs of the *cobra de capello,* in four stages of their growth.

A, B, C, D, are magnified representations of the same.

A, is a full-grown fang firmly fixed to the bone.

B, is not quite perfect, the lower part of the foramen at the base not being yet formed.

C, in this a very small part of the foramen is formed.

D, the part of the tooth above the foramen alone appears.

E, F, G, are end-views of B, C, D, showing the poison tube nearly surrounded by the cavity which contains the pulp, and the proportions between them, at three different parts of the tooth.

H, a section made by sawing the full-grown fang A, just above the

the lower foramen, showing the rounded edges of the slit, which consequently leave a slight channel along the tooth.

I, K, magnified representations of sections of the fangs of an unknown species of serpent, which have exactly the same form as those of the rattle-snake.

I, is a section of a young fang taken about the middle: in this stage of growth, the cavity which contains the pulp almost entirely surrounds the poison tube; and the edges of the depression which form the suture are seen to be angular, and present so large a surface to each other, that the suture is completely filled up even in this early stage of growth.

K, is a section of a full-grown fang of the same species of serpent at the same part as the preceding. Here the cavity of the pulp is seen greatly contracted from the more advanced stage of growth.

LXX. *On the received Theory of Heat.* *By* A CORRESPONDENT.

To Mr. Tilloch.

SIR,—IN addition to my hasty remarks (in your October Number) I think that heat is only the effect of a decomposing principle circulated from, or sympathetic with, the sun, because of its peculiar activity when immediately under it, and its indolence when out of its presence, except when collected in the form of combustion, electricity, galvanism, or lightning, &c. It then shows an appearance corresponding with the matter it is compounded with, and disorganizes any substance in proportion to the purity, quantity and rapidity of its application to it. Combustion is perhaps maintained by the flowing in of the decomposing principle from the adjoining air, and lightning an accidental mass bursting to a natural distribution, and violently disuniting the component parts of the atmosphere, upon whose junction meteoric stones may be formed from the half-melted materials. On the equator this influence is the most regular and effective—raising befor the sun a prodigious increase of atmosphere out of earth, water, &c. forcing by expansion a current of air from it, which being resisted (in degree) by a more dense atmosphere, produces a reaction upon the surface of the earth, and thereby a constant revolution towards the sun. If elementary heat was the cause of this, I think our atmosphere would be wholly condensed before it reached the icy regions; its heat would be entirely withdrawn, and the matter it is composed of descend to the earth in particular latitudes. In the case of insensible perspiration in plants and animals, we perceive a decomposition of of water (at least), whose place is supplied by the substance immediately under it, communicating a continuance and succession of motion and sensation called heat throughout the system. When an organ in the human frame is much debilitated, a blister or sudden decomposition on the surface is often more efficaciously invigorating to what is beneath it, than internal medicine—from the latter being only a spur to an already jaded animal, and giving dangerous kicks to its neighbouring organs; while the former is a happier imitation of natural stimulus. I conclude therefore with venturing to submit that heat is an effect of disorganization, and not an elementary principle.

I am, sir, very respectfully, yours, &c.

Claughton-House, Lancaster, Nov. 17, 1818. S. S.

P. S. As we recede from the equator or ecliptic, the decomposing principle acts with less energy; therefore the atmosphere contains less earthy matter; so that what is lightning in a dense atmosphere may rationally be *aurora borealis* at the poles.

LXXI. *Notices respecting New Books.*

The Transactions of the Royal Society, Part II. for 1818, just published, contain:

XIV. On the Parallax of certain fixed Stars. By the Rev. John Brinkley, D.D. F.R.S. and Andrews Professor of Astronomy in the University of Dublin.—XV. On the Urinary Organs and Secretions of some of the Amphibia. By John Davy, M.D. F.R.S. Communicated by the Society for the Improvement of Animal Chemistry.—XVI.—On a Mal-conformation of the Uterine System in Women; and on some physiological Conclusions to be derived from it. In a Letter to Sir Everard Home, Bart. V.P. R.S. from A. B. Granville, M.D. F.R.S. F.L.S. Physician in Ordinary to H. R. H. the Duke of Clarence; Member of the Royal College of Physicians, and Physician-Accoucheur to the Westminster General Dispensary.—XVII. New Experiments on some of the Combinations of Phosphorus. By Sir H. Davy, LL.D. F.R.S. Vice-Pres. R.I.—XVIII. New experimental Researches on some of the leading Doctrines of Caloric; particularly on the Relation between the Elasticity, Temperature, and latent Heat of different Vapours; and on thermometric Admeasurement and Capacity. By Andrew Ure, M.D. Communicated by W. H. Wollaston, M.D. F.R.S.—XIX. Observations on the Heights of Mountains in the North of England. By Thomas Greatorex, Esq. F.L.S. In a Letter to Thomas Young, M.D. For. Sec. R.S.—XX. On the different Methods of constructing a Catalogue of fixed Stars. By J. Pond, Esq. F.R.S. Astronomer Royal.—XXI. A Description

tion of the Teeth of the Delphinus Gangeticus. By Sir Everard Home, Bart. V.P.R.S.—XXII. Description of an Acid Principle prepared from the lithic or uric Acid. By William Prout, M.D. Communicated by W.H.Wollaston, M.D. F.R.S.—XXIII. Astronomical Observations and Experiments, selected for the Purpose of ascertaining the relative Distances of Clusters of Stars, and of investigating how far the Power of our Telescopes may be expected to reach into Space, when directed to ambiguous celestial Objects. By Sir William Herschel, Knt. Guelp. LL.D. F.R.S. —XXIV. On the Structure of the poisonous Fangs of Serpents. By Thomas Smith, Esq. F.R.S.—XXV. On the Parallax of α *Aquilæ*. By John Pond, F.R.S. Astronomer Royal.—XXVI. On the Parallax of the fixed Stars in Right Ascension. By John Pond, F.R.S. Astronomer Royal.—XXVII. An Abstract of the Results deduced from the Measurement of an Arc on the Meridian, extending from Lat. 8° 9′ 38″·4, to Lat. 18° 3′ 23″·6, N. being an Amplitude of 9° 53′ 45″·2. By Lieut. Col. William Lambton, F.R.S. 33d Regiment of Foot.

A Treatise on Marine Surveying. In two Parts. By Murdoch Mackenzie senior, late Marine Surveyor in His Majesty's Service. Corrected and republished with a Supplement by James Horsburgh, F.R.S. Hydrographer to the Hon. the East India Company. 8vo, pp. 183.

The Treatise on Marine Surveying by the late Mr. Murdoch Mackenzie, of which we are now presented with an improved and enlarged edition, has long maintained the character of the most scientific, useful and exemplary work on that branch of nautical knowledge ever published in this or perhaps in any other country. It has nevertheless not been reprinted since its first appearance in 1774, and is now extremely scarce.

To those, therefore, who are curious to reach the summit of nautical science, and to naval officers in general, a republication of this work, by an editor so able and well-informed as Mr. Horsburgh, cannot fail to be highly acceptable. Although the original work has been preserved in the form given it by the author as nearly as possible, we have met with several important alterations which appear to us to have been rendered essentially necessary by the rapid improvements Navigation has lately received from the introduction of chronometers and other means, and have been introduced by the ingenious editor with a degree of accuracy and skilfulness, to which we beg to bear the humble tribute of our most unqualified approbation.

A Supplement has been also added, containing some interesting examples, with precepts relative to marine surveying, and other information applicable to the advancement of young officers in useful knowledge. The

The work is dedicated in very handsome terms to the venerable President of the Royal Society, as the "acknowledged patron of science, and of every invention or improvement calculated to advance the happiness and to promote the welfare of mankind."

Dr. Thomas Forster has just published a small Tract on the periodical Affections of the Brain and Nervous System. In endeavouring to deduce the periods of diseases from periodical changes in the atmosphere, the author alludes to the following remarkable circumstance,—that the periods of many nervous diseases correspond with those well known changes of weather which so often happen near the new and full of the moon, as to have been ascribed, even by popular opinion, to her special influence on the weather. He notices also numerous periodical plants, which open and shut their flowers at particular hours of the day and night, in order to prove an atmospherical cause of the periods observed by plants as well as animals.

The same author will shortly publish Observations on the Periods at which the different Organs of the Brain become active, and those at which their Activity ceases.

A considerable work has long been expected from Dr. Spurzheim, on Education, founded on the knowledge of the Physiology of the Brain.

Mr. Bicheno of Newbury has published a book On the Nature of Benevolence, in which are some curious and novel remarks on the Poor Laws.

The expected Account of the Mission from Cape Coast Castle to the Kingdom of Ashantee, in Africa, will, we understand, appear in a few days. It has for its author Thomas Edward Bowditch, Esq. Conductor and Chief of the Embassy; and comprises the History, Laws, Superstitions, Customs, Architecture, Trade, &c. of that part of Africa. To which is added, a Translation from the Arabic, of an Account of Mr. Park's Death, &c. with a Map, and several Plates of Architecture, Costumes, Processions, &c. In one 4to volume.

Observations on Ackermann's *Patent Moveable Axles for Four-wheeled Carriages, containing an engraved Elevation of a Carriage, with Plans and Sections conveying accurate Ideas of this superior Improvement.* Crown 8vo. pp. 54.

The Axles, which are the subject of these observations, are the invention of Mr. Lankensperger, of Munich; but the patent for them, as is usual in the case of inventions by foreigners, has been taken out in the name of Mr. Ackermann, as agent for the contriver.

T. W. C. Edwards, M.A. author of The First Principles of Algebra, is publishing a Course of familiar Lectures on the Outlines of Chemistry and Philosophy, including the latest discoveries and improvements.

These Lectures are to be illustrated by a variety of neat diagrams and experiments. The volume will be portable, and well adapted for the use of schools as well as private reading.

Just published, Elements of Medical Logick; illustrated by Practical Proofs and Examples. By Sir Gilbert Blane, M.D. Physician to the King, &c. 8vo.

Practical Illustrations of the Progress of Medical Improvement for the last Thirty Years; or, Histories of Cases of Acute Diseases, as Fevers, Dysentery, Hepatitis, and the Plague, treated according to the Principles of the Doctrine of Excitation, by himself and other Practitioners, chiefly in the East and West Indies, in the Levant, and at Sea. By Charles Maclean, M.D. &c. 8vo.

Illustrations of the Power of Emetic Tartar in the Cure of Fever, Inflammation, and Asthma; and in preventing Phthisis and Apoplexy. By William Balfour, M.D. Author of "A Treatise on Rheumatism, &c."

In the Press, A Treatise on Midwifery, enforcing new principles, which tend materially to lessen the sufferings of the Patient and shorten the duration of Labour. By John Power, Accoucheur, Member of the Royal Medical Society of Edinburgh.

An Account of the Epidemic and Sporadic Disorders which prevailed this year, 1818, at Rochester, and near it. By Walter Vaughan, M.D. Licentiate of the Royal College of Physicians of London. 8vo.

Illustrations of the Power of Compression and Percussion in the Cure of Rheumatism, Gout, and Debility of the Extremities; and in promoting general Health and Longevity. By William Balfour, M.D. Author of "A Treatise on the Sedative and Febrifuge Powers of Emetic Tartar, &c."

Just published, The Theory of Parallel Lines perfected, or The Twelfth Axiom of Euclid's Elements demonstrated. By Thomas Exley, A.M.

An Account of the History and Present State of Galvanism. By John Bostock, M.D. F.R.S. pp. 164.

Practical Hints on Decorative Printing. By William Savage. Part I. The present Part contains, Chap. I. Introductory Sketch of the Progress of the Art.—Chap. II. On Printing Materials.

Materials.—Chap. III. On Press-work.—Chap. IV. On Printing in Colours. The illustrative decorations which accompany these chapters are—Two pages of types, showing the varieties they have undergone—Two subjects, female figures, to show the effect of different coloured inks, with fine engravings—A Sybil writing;—two blocks—Female figure and child, a street sweeper, three blocks; an impression is given of each block, with their progressions and combinations—Four head pieces, printed as cameos; three blocks each—Ancient tower near Denbigh, to imitate a drawing in India ink; nine blocks—Cottage and landscape to imitate a drawing in India ink; nine blocks—Earl Spencer's arms in their heraldic colours; six blocks—Butterfly in colours; seven blocks—Parrot in colours; seven blocks—Grecian vase from the collection of Sir William Hamilton in the British Museum, to imitate the original; six blocks—Cottage and landscape, to imitate a coloured drawing; fourteen blocks—Six tables of inks showing eighteen colours.

LXXII. *Proceedings of Learned Societies.*

ROYAL INSTITUTE OF FRANCE.

Investigation of the Dry Rot in Timber.

At the Sittings of the Royal Academy of Sciences at Paris, on the 16th ult., a Report was read on "An Essay on the Dry Rot, by Robert MacWilliam, Architect: and on the 23d the Secretary, Mons. Cuvier, addressed and transmitted to the author an acknowledgement of the receipt of the work by the Academy, and of the proceedings that had in consequence taken place; intimating to him, that it was on account of the importance of the objects of which he had treated, and of his scientific researches, that the Academy had been led to have the analysis (*compte verbal*) made out; and adding, that though it was contrary to their usage to deliver to authors a copy of their Reports on printed works, the Academy had made an exception in his favour, and directed their Secretary to present him with a copy of this Report, and to thank him for having made them acquainted with a work, the interest and instruction of which were such as to induce them to give it an honourable place in the library of the Institute.

LXXIII. *In-*

LXXIII. *Intelligence and Miscellaneous Articles.*

On the Nautical Almanac.

To Mr. Tilloch.

Sir,—One of your correspondents has, in a former number of your valuable Miscellany (Vol. LI. page 186) suggested some improvements in the future Nautical Almanacs, to be published under the direction of the New Board of Longitude, which I hope will be attended to. There is one circumstance however connected with this subject, to which he has not drawn the attention of the New Board, but which I think would be highly useful; namely, the place of the moon in right ascension and declination to *seconds* of a degree. At present the place of the moon is given to the nearest *minute* only, which is not sufficiently correct for many useful purposes. In the *Connaissance des Tems* the right ascension is given to the nearest *second*; and I hope the editors of that valuable work will, in the future volumes, pursue the same plan with respect to the declination.—It is well known that the moon's parallax in right ascension and declination may be much more readily found, than her parallax in longitude and latitude; as in the former case we need not have recourse to finding the longitude and height of the nonagesimal: and consequently all problems relative to her apparent place may be much more easily and expeditiously solved.—The late elegant formulæ of M. Olbers, for this purpose, have also given an additional interest to the subject: and it will readily occur to your astronomical readers, that this method of determining the apparent place of the moon is the one best adapted for determining the various circumstances relative to occultations of the fixed stars, as well as other phænomena in which the apparent place of the moon is involved.

I am, sir, your obedient servant,

Dec. 15, 1818. Ptolemy.

P.S. I observe that the publisher of the Nautical Almanac has printed and distributed a list of what he calls *additional* corrections for the Nautical Almanac for 1819. May I request some of your numerous correspondents to inform me where the *primary*, or *preceding*, corrections are to be found? as I observe three very remarkable errors, not at all noticed in these *additional* corrections, viz. the entire omission of two solar eclipses (one on March 25, the other on October 18th), and the insertion of an occultation of *Antares* on December 15th as visible at Greenwich, which cannot be the case: besides several *other* errors which I have never yet seen published.

ON

ON PURIFYING COAL GAS.

In our last Number but one, we inserted a letter from Mr. S. Parker, of Liverpool, on the above subject; and in our last Number, another from Mr. G. Lowe, of Derby; also the specification of a patent for the same purpose taken out by Mr. G. H. Palmer in January last. To the information afforded by these articles we now add, that an apparatus extremely similar to that specified by Mr. Palmer has been publicly exhibited at the Agricultural Repository in Winslow-street, Oxford-street, upwards of two years. It was made by Mr. Manby, Director of the Horseley Company's Iron Works near Dudley in Worcestershire, and who has been for several years past engaged in the manufacture of similar apparatus, and in fitting them up in various parts of this country.

LAMPS.

In lamps it is essential that the oil be kept on a uniform level with the wick—the reservoir therefore has necessarily been in an obtrusive and inelegant position. A gentleman in Paris has recently made some beautiful lamps, where the oil is in the base of a column, and is pumped up by a spring and pendulum working in the oil, the surplus supply returning to the reservoir by a waste pipe; thus enabling the workman to give them any ornamental or fanciful shape.—S. S.

STEAM ENGINES IN CORNWALL.

From Messrs. Leans' Report for November 1818, it appears that the following was the work performed during that month, by the engines reported, with each bushel of coals.

	Pounds of water lifted 1 foot high with each bushel.	*Load per square inch in cylinder.*
23 common engines averaged	21,386,818	various.
Woolf's at Wheal Vor ..	31,957,312	17·8 lib.
Ditto Wh. Abraham ..	36,441,367	16·8
Ditto ditto	19,233,473	6·7
Wheal Abraham engine ..	31,508,419	10·9
Dalcoath ditto ..	37,127,355	11·3
United Mines ditto ..	34,680,485	18·6
Treskirby ditto	34,987,815	11·25
Wheal Chance ditto ..	31,274,981	12·2

THE UNUSUAL SEASON.

Among the many phænomena produced by the unusually warm summer and autumn which we have had this year, may be reckoned the appearance already of several of the ordinary productions of spring. Three weeks ago the Narcissus was in bloom in

 a shela sheltered situation in Hampshire; and what is still more extraordinary, the young leaves of the lime-trees are already fully expanded on some trees on Wanstead Flats in Essex. Early this month (December) a swallow was seen, and the spring snowdrop (*Galanthus nivalis*) was in flower.

What particular constitution of atmosphere has led to this unusual anticipation of spring appears to be unknown; but it seems to be not merely the warmth, as warm autumns have not hitherto been followed by similar phænomena.

Greek Antiquities in the Crimea.—Extract of a letter from the Engineer Von Stier, at the fortress of Fanagoria, in the government of Tauris, formerly the Crimea, Aug. 20, 1818.

"Among the curiosities of this place are the remains of antiquities of the time of the Greeks, who planted colonies here. In the beginning of this month, in digging up a hill, a stone vault was discovered, which contained a corpse six feet and a half long, in a very good state of preservation. The head was ornamented with a golden garland of laurels; and on the forehead a golden medal which represents a man's head with the inscription *Philip*. On both sides of the corpse stood golden and earthen vessels, as was the custom among the Greeks, also several golden chains and ear-rings; and on one of the fingers was a gold ring with a valuable stone, on which were represented a male and female figure of exquisite workmanship. From all this it may be concluded that this was the burying-place of one of Philip's generals."

LECTURES.

Mr. Guthrie on Surgery.—Mr. Guthrie, Deputy Inspector of Military Hospitals, will commence his Spring Course of Lectures on Surgery, on Monday, January 18, at five minutes past Eight in the Evening, in the Waiting Room of the Royal Westminster Infirmary for Diseases of the Eye, Mary-le-bone Street, Piccadilly. To be continued on Mondays, Wednesdays, and Fridays.

Two Courses will be delivered during the Season.

In each Course the Principles of Surgery will be explained, and the Practice resulting from them, with reference both to Domestic and Military Surgery, fully pointed out.

The Diseases of the Eye, although forming an integral Part of the Lectures on Surgery, will, for the convenience of illustration, be delivered every Thursday evening until completed.

The Operations referred to in the Lectures will be shown in each Course.

Terms of Attendance.—Perpetual Five Guineas.—Single Course Three Guineas.

Medical Officers of the Navy, the Army, and the Ordnance, will

will be admitted gratis, on obtaining a recommendation from the Heads of their respective Departments, which must be presented to Mr. Guthrie between the hours of two and half-past four, at his house, No. 2, Berkeley-street, Berkeley-square.

Lectures on the Practice of Physic.—Dr. C. Forbes, Deputy Inspector of Military Hospitals; Physician to His Royal Highness the Duke of Kent; Senior Physician to the Surry Dispensary; Physician to the Royal Westminster Infirmary for Diseases of the Eye, &c. will commence a Course of Lectures on the Practice of Physic, on Wednesday, January 21st, at Nine o'clock in the Morning, at the Royal Westminster Eye Infirmary, Mary-le-bone Street, Golden-square.

Terms of Attendance.—Single Course Three Guineas.—Perpetual Five Guineas.

Medical Officers of the Navy, the Army, and the Ordnance, will be admitted to attend these Lectures, on presenting a recommendation from the Heads of their respective Departments, to Dr. Forbes, at his House, No. 25, Argyll-street, before Nine o'clock in the Morning.

Mr. Taunton will commence his next Course of Lectures on Anatomy, Physiology, Pathology, and Surgery, on Saturday, January 23, 1819.

LIST OF PATENTS FOR NEW INVENTIONS.

To Jeremiah Spencer, of Great James-street, Bedford-row, in the county of Middlésex, for improvements on a certain description of fire grates, by which improvement the combustion of smoke is more easily effected.—5th December, 1818.—2 months allowed to enrol specification.

To Frederick William Seyfert, of St. John's-street, Clerkenwell, in the county of Middlesex, for improvements on certain descriptions of watches and clocks.—5th December.—2 months.

To Marc Isambard Brunel, of Chelsea in Middlesex, for his sheets of tin foil capable of being crystallized in large varied and beautiful crystallization.—5th December.—6 months.

To John Whiting, of Ipswich, Suffolk, for a window shutter. 5th December.—2 months.

To Henry Pershouse, of Birmingham, for his improved method of stamping pans for seals.—10th December.—2 months.

To James Barron, of Wells-street, Oxford-street, in Middlesex, for his improvement in the making and fixing of knobs, generally used on drawers, doors, and cabinet furniture, and known by the name of drawer and mortice furniture knobs or handles. —10th December.—6 months.

METEORO-

Meteorological Journal kept at Walthamstow, Essex, from November 15 *to December* 15, 1818.

[Usually between the Hours of Seven and Nine A.M. and the Thermometer (a second time) between Twelve and Two P.M.]

Date.	Therm.	Barom.	Wind.
November			
15	48 50	29·40	SW—NW. — Wind, clear and *cumuli;* rain after 11 A.M. till about 2 P.M.; fine afterwards; moon-light and *cirrostratus;* at 10 P.M. moon in a large *halo,* and stars visible *within* the area of the *halo,* and thin *stratus* over the other part of the sky.
16	48 56	29·62	SE.—Very rainy till about noon; a cloudy and dark day; cloudy and windy evening; moon-light at 11 P.M.
17	47 50	29·61	W.—Very fine: clear and *cirrocumuli; extreme* fine day; clear sky; sun and wind; fine moon-light.
18	34 45	30·00	SW.—Fine morn, but rather hazy; fine day; moon-light.
19	44 52	30·00	SW—SE.—Gray morn; gray day; fine starlight.
20	39 48	29·91	E—SE.—Clear and windy; fine day; sun and wind; star-light.
21	40 44	29·70	SE—E.—Gray and windy; fine day (*cold*); a swallow was seen near Lea Bridge; dark night. Moon last quarter.
22	35 44	29·70	E—NE.— Fine morn; very fine day; stars visible, but rather hazy.
23	44 55	29·50	SE—SW.—Rainy morn; sun and *cumuli;* wind, and very damp; star-light.
24	50 52	29·64	SE—W.—Very damp and rainy at 7 A.M.; foggy at 10 A.M.; fine afternoon; clear and *cirrostratus;* star-light.
25	35 45	30·00	S by W.—Foggy morn; fine day; sun, rather hazy; some stars visible.
26	50 52	30·10	SE.—Gray and damp early; rain at 8 A.M.; very rainy till after dark; cloudy at 9 P.M.
27	51 53	30·30	S—SE.—Cloudy at 7 A.M. and at 8, foggy; *very fine* day; very dark night.
28	49 57	30·30	SE.— Clear and *cirrostratus*; fine day; hazy and some sun; very dark night. New moon.

November

Date.	Therm.	Barom.	Wind.
November.			
29	53 58	30·22	W—SW.—Cloudy and damp; gray day, and some wind; set shower at 1 P.M.; very dark night.
30	50 54	30·20	SE—SW.—Gray; hazy and gleams of sun; dark night.
December			
1	48 47	30·00	SE.—Gray morn; very dark day; dark night.
2	44 46	29·90	NW.—Cloudy morn; dark day; rain early; clear starlight.
3	48	29·62	SE.—Cloudy and windy; very showery and windy; moonlight.
4	46 49	29·50	SE.—*Cirrostratus* and clear; very red before sunrise; fine day; cloudy and windy. Moon first quarter.
5	42 50	29·55	SE.—Clear and windy; fine day; moonlight.
6	36 49	29·55	SE.—Clear and white dew; fine day; sun, and clear; cloudy and windy night; *aurora borealis.*
7	47 52	29·39	S.—Cloudy; showery and hazy; light, but neither moon nor stars visible.
8	47 53	29·70	SE.—Gray morn; a shower of hail at 10 A.M.; gleam of sun and very damp; light, but no stars visible.
9	41 47	29·95	NW.—Gray morn; rainy day; rainy night.
10	36 45	30·05	NW.—Clear and clouds; fine day; sun and wind; bright moon and starlight.
11	33 41	30·05	NW.—Clear morn; very fine day; cloudy but light.
12	33 40	30·10	N—NW.—Clear moonlight morning; showers and sun; showery night. Full Moon.
13	36 40	30·10	W.—Clear early; very red sun-rise; *cirrostratus*; very fine day; *cumuli* and light, but neither moon nor stars.
14	37 40	30·10	N.—*Cirrostratus*; cold fine gray day; at 9 P.M. cloudy night; moon and star-light late.
15	36 40	30·10	NE.—*Cumuli* and clear; very fine sun and *cumuli*; cold and windy; bright moonlight.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

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

1818	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Nov. 15	15	51·5	29·58	Fine—heavy rain P. M.
16	16	55·	29·50	Cloudy
17	17	50·	29·84	Fine
18	18	47·5	30·10	Ditto
19	19	54·	30·10	Cloudy
20	20	48·	30·	Fine
21	21	43·	29·86	Cloudy
22	22	43·	29·86	Very fine
23	23	53·	29·72	Fine—rain morning and evenSin
24	24	49·	29·85	Rain
25	25	46·5	30·16	Fine
26	26	52·	30·15	Rain
27	27	54·	30·42	Cloudy
28	new	55·	30·40	Ditto
29	29	57·	30·30	Ditto
30	30	54·	30·20	Fine
Dec. 1	1	51·	30·05	Ditto
2	2	43.	30·05	Ditto
3	3	51·5	29·64	Cloudy
4	4	47·	29·55	Fine
5	5	49·	29·56	Ditto
6	6	46·5	29·67	Ditto
7	7	47·	29·52	Cloudy—heavy rain in the morn- [ing
8	8	49·5	29·82	Fine
9	9	41·	30·14	Cloudy
10	10	43·	30·25	Ditto
11	11	43·	30·27	Ditto
12	full	43·	30·22	Ditto
13	13	44·	30·27	Ditto
14	14	42·5	30·33	Ditto

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For December 1818.

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	52	57	55	30·40	25	Fair
28	53	57	55	·38	24	Fair
29	55	57	55	·34	20	Fair
30	54	55	54	·20	20	Cloudy
Dec. 1	47	47	50	29·99	15	Cloudy
2	42	47	40	30·01	16	Cloudy
3	45	47	46	29·65	0	Rain
4	47	52	50	·48	26	Fair
5	51	54	46	·52	26	Fair
6	46	50	45	·62	30	Fair
7	44	52	52	·53	0	Rain
8	52	53	50	·89	0	Rain
9	44	47	44	30·05	17	Cloudy
10	40	47	39	·14	24	Fair
11	36	44	40	·14	22	Fair
12	40	44	40	·09	0	Small rain
13	40	43	40	·17	15	Fair
14	40	42	40	·19	13	Fair
15	39	43	39	·15	10	Fair
16	30	35	27	·04	8	Fair
17	22	30	26	·01	8	Fair
18	24	40	32	29·72	0	Rain
19	28	38	40	30·27	5	Fair
20	44	49	46	·06	5	Cloudy
21	48	48	37	·22	8	Fair
22	28	28	30	·52	0	Foggy
23	32	37	28	·35	0	Foggy
24	27	30	27	·22	7	Fair
25	27	37	38	·06	0	Cloudy
26	35	35	35	29·90	6	Cloudy

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

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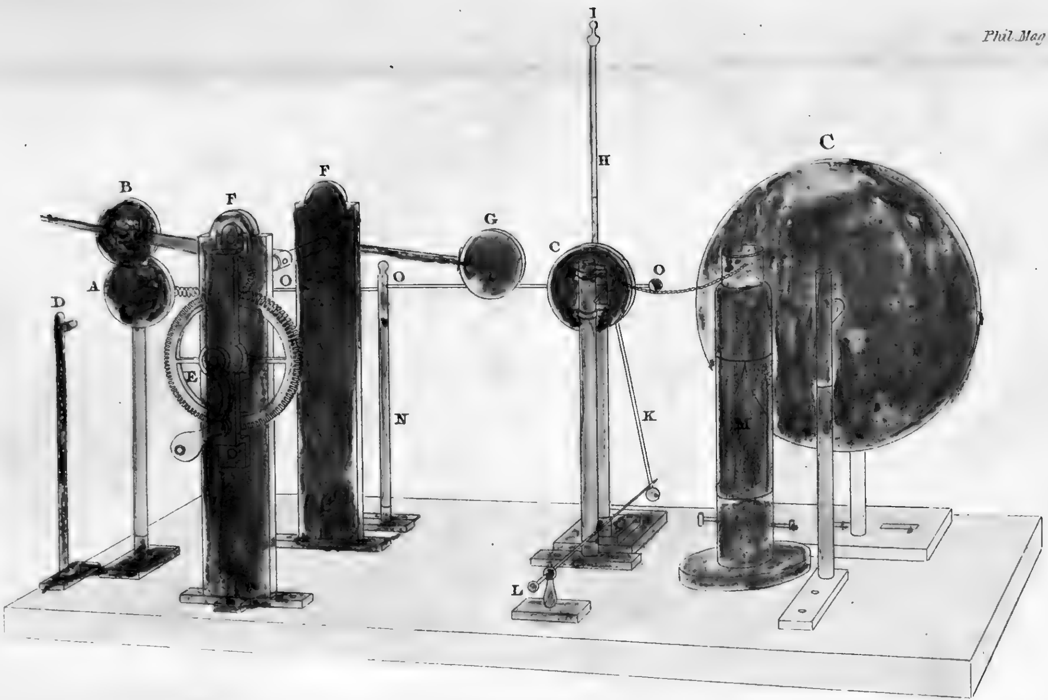
END OF THE FIFTY-SECOND VOLUME.

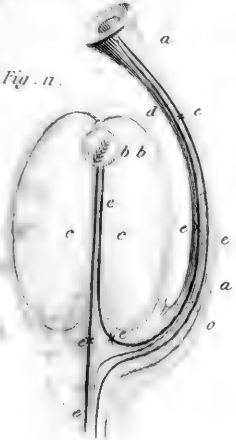
Printed by R. and A. Taylor, Shoe-Lane, London.



S. Porter. sc.

Phil. Mag. Vol. III. Pl. II.

Fig. 11.



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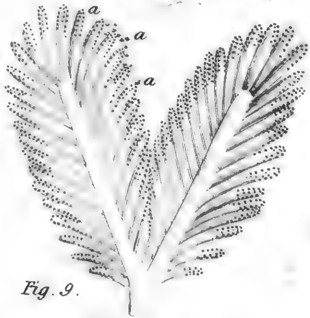

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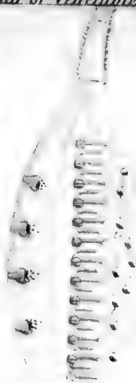

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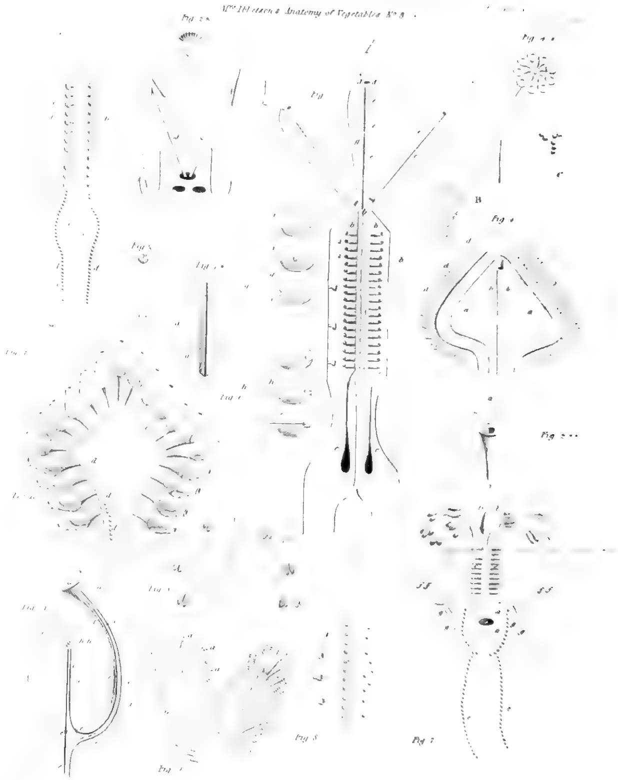
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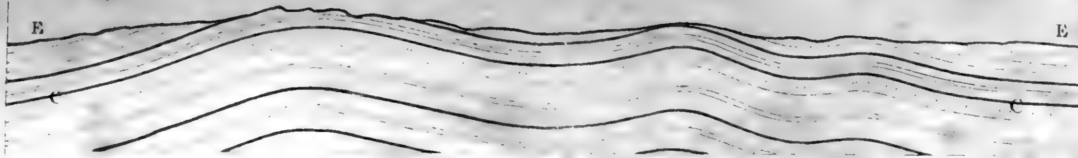

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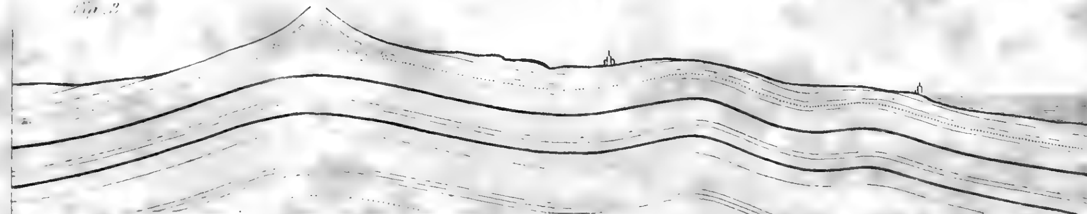

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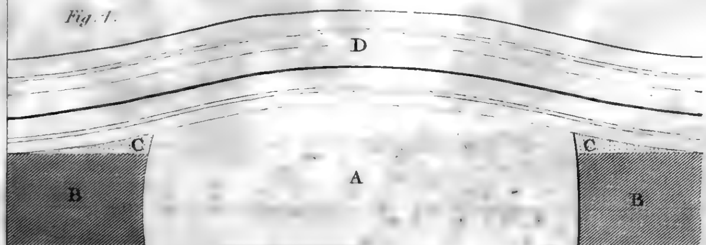

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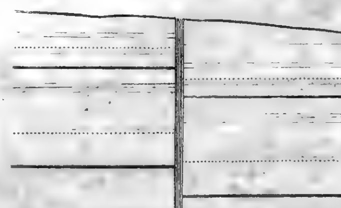

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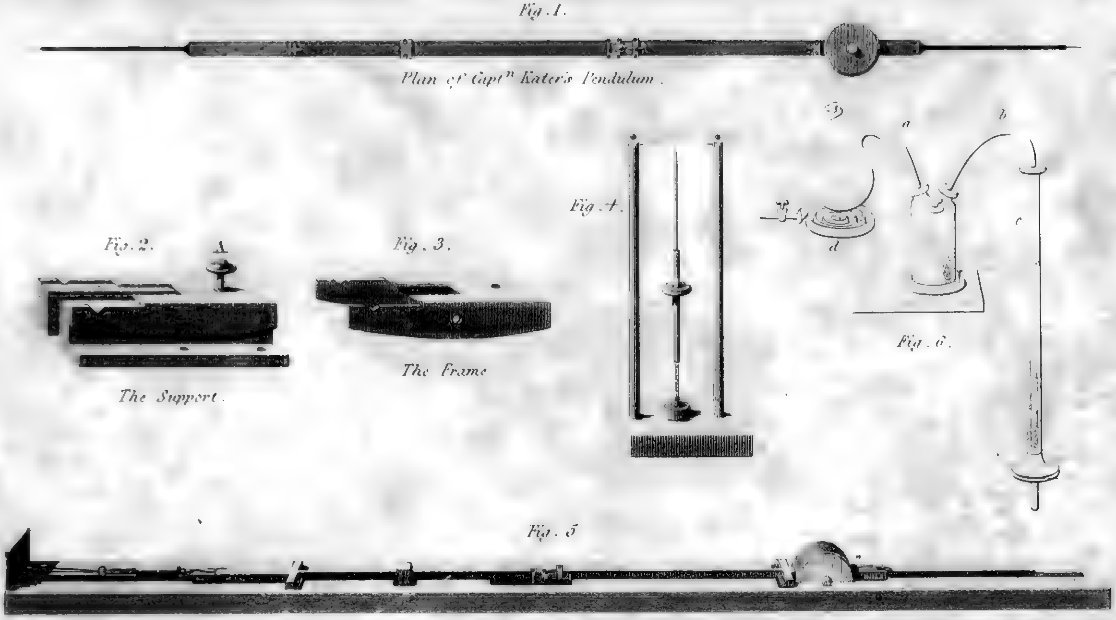
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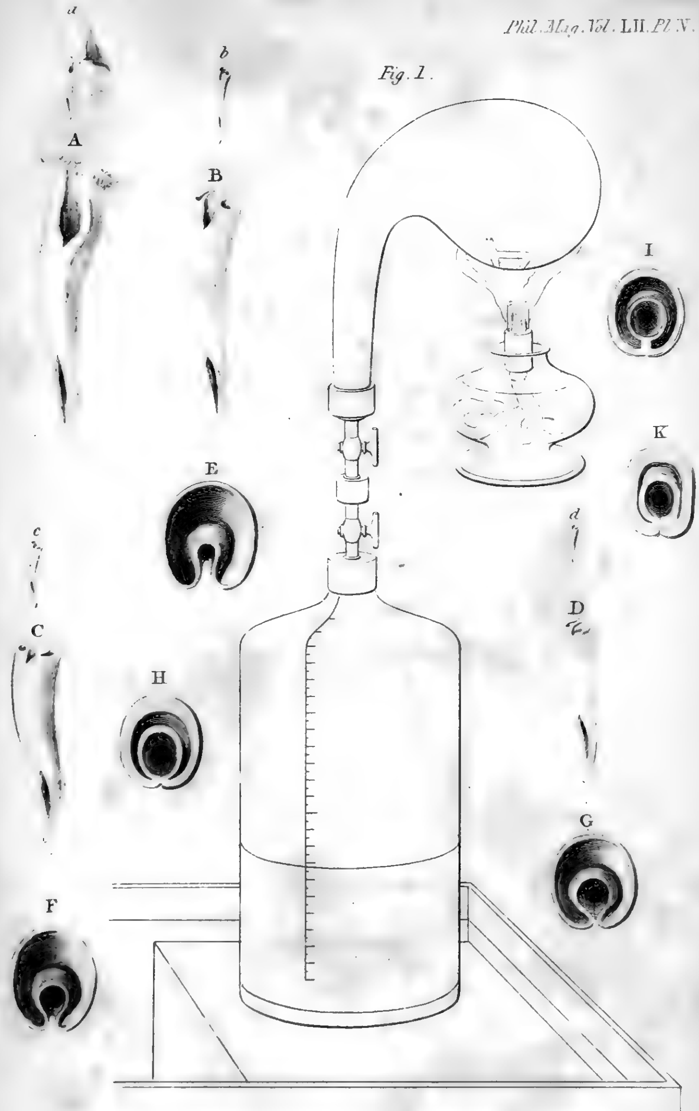

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