THE

PHILOSOPHICAL MAGAZINE AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

GEOLOGY,

AGRICULTURE,

MANUFACTURES AND COMMERCE.

BY ALEXANDER TILLOCH,

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

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

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OF THE FORTY-FOURTH VOLUME.

CONTENTS.

THE

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PHILOSOPHICAL MAGAZINE AND JOURNAL.

I. *Description of a new Gazometer and Blow-pipe.* By JOSEPH HUME, *Esq.*

Long Acre, London, June 13, 1814.

SIR, — A VERY simple apparatus to serve as a gazometer and blow-pipe, and one that can be formed with ease and at little expense, will probably be acceptable to the majority of your readers, as well as to those experimentalists who are not furnished with the usual instruments.

As an example, and that I may more clearly describe the principles and advantages of such an apparatus, I send you a sketch of one constructed with vessels which I had at hand and are applicable to other purposes, and which most chemists possess; for, whoever wishes for a combination to produce analogous powers, will soon perceive that this contrivance is not limited to any particular size or form, but that others may be adopted; and that the flame of the lamp can be supported by the operator inhaling the air into his lungs, as well as by the ordinary custom of exhalation and, consequently, the application of contaminated breath.

In the present case I have taken a common Woulfe's receiver with three necks; a separating funnel; two stop-cocks, which are of glass and belong to two of Nooth's machines; and a piece of glass tube properly bent and drawn out for the blow-pipe. These form the whole of the plan which I now offer to your notice, and which are arranged agreeably to the following explanation:

A, (Plate I. fig. 7.) is the Woulfe's bottle with three necks, and filled with water;

B, is the separating funnel, fitted into the middle neck of the bottle;

C, a blowpipe, inserted into another neck of the same.

D and E, the two stop-cocks, fixed in their places, as shown in the figure.

The whole being thus connected, it is obvious that, if the operator open both the cocks and, with his mouth applied to E, suck out or inhale the air out of the superior vessel, the water will then ascend and occupy the place of the air thus exhausted; while the receiver below will have gained a proportional quantity of atmospheric air which had entered through the stop-cock at D. If both cocks be then instantly shut, the whole of the water, drawn up into the superior vessel B, will retain its position and may be there kept for any time.

Now, if a lamp, with a proper wick and lighted, be placed before the blow-pipe C, and the cock E be then opened to admit the atmospheric pressure to act, it is evident that the water in the funnel will begin to descend and take its former station, and, consequently, the air in the Woulfe's receiver, having no other way to escape, must be forced out at the aperture of the blow-pipe and through the flame of the lamp.

In this way I have found, that a very few cubic inches of common air are sufficient to impel the flame of the lamp, while a person can hermetically seal up a small glass tube, or perform some other useful or amusing experiment on minute objects.

When all the water has descended it must again be raised by suction as before, first opening the stop-cock D. This must be done at every operation, and will not occasion so much fatigue and continual blowing as when the blow-pipe alone is employed in the ordinary way. It affords also another advantage to an experimentalist, that of having both hands at liberty, which is particularly requisite in the formation of small glass instruments.

By a very evident contrivance, that of connecting the tubulure at D with a common bladder or receiver containing oxygen, hydrogen or any other gaseous fluid, and then by inhaling from the orifice or stop-cock at E, as has been already explained, this instrument can be charged with either of these airs, and in this way it may be employed as a gazometer or blow-pipe. In some cases the blow-pipe must be closed or furnished with a stop-cock, particularly when for certain experiments hydrogen is to be admitted.

It is almost superfluous to mention another useful application of this apparatus, namely, to convert it into the hydro-pneumatic blow-pipe of Mr. Tilley, which is so well explained in the Philosophical Magazine for April. This is to be accomplished merely by introducing a long metallic or glass-tube into the orifice at D, in place of the stop-cock; and in such a manner that, when firmly secured and luted, it may reach from within a very little of the bottom in the water, and proceed upwards to the very top of the whole apparatus. This tube, externally from D, D, should be so conveniently bent as to be held in the mouth of the operator, who, in place of inhaling as before, will now have to keep up a constant nourishment to the flame by occasionally blowing in through this tube and thus forcing the water to ascend into B; whence it will act by pressure as before, the cock E being left open for that purpose. It will in some degree assist the power if, in the intervals of blowing, the tongue or lips be so managed as to stop the superior aperture of this tube. But as Mr. Tilley's instructions are before your readers I shall not intrude further respecting this part of my subject.

I am not aware that any contrivance of the same description, and one that admits of such facilities in the construction, for feeding the blow-pipe by *inhalation* has ever been employed, otherwise I should not have troubled you with this communication for public notice.

I remain, sir,

Your very obedient servant,

JOSEPH HUME.

To Mr. Tilloch.

II. *Some Experiments and Observations on a new Substance which becomes a violet-coloured Gas by Heat.* By Sir HUMPHRY DAVY, *Knt. LL.D. F.R.S.**

A NEW and a very curious substance has recently occupied the attention of chemists at Paris.

This substance was accidentally discovered about two years ago by M. Courtois, a manufacturer of saltpetre at Paris. In his processes for procuring soda from the ashes of sea weeds, (*cendres de vareck*) he found the metallic vessels much corroded; and in searching for the cause of this effect, he made the discovery. The substance is procured from the ashes, after the extraction of the carbonate of soda, with great facility, and merely by the action of sulphuric acid:—when the acid is concentrated, so as to produce much heat, the substance appears as a vapour of a beautiful violet colour, which condenses in crystals having the colour and the lustre of plumbago.

M. Courtois, soon after he had discovered it, gave specimens of it to MM. Desormes and Clement for chemical examination; and those gentlemen read a short memoir upon it, at a meeting of the Imperial Institute of France, on Nov. 29th. In this memoir, these able chemists have described its principal properties; they mentioned that its specific gravity was about four times

* From the Philosophical Transactions for 1814, part i.

 that

that of water; that it becomes a violet-coloured gas at a temperature below that of boiling water; that it combines with the metals and with phosphorus and sulphur, and likewise with the alkalies and metallic oxides; that it forms a detonating compound with ammonia; that it is soluble in alcohol, and still more soluble in ether; and that, by its action upon phosphorus and upon hydrogen, a substance having the characters of muriatic acid is formed. In this communication they offered no decided opinion respecting its nature.

M. Ampere had the goodness to give me some of this substance; and M. Clement having requested me to submit it to some analytical tests, I made several experiments upon it, which convinced me that it was a new substance undecompounded in any of the circumstances to which I was able to expose it; and that the acid formed in processes upon it was not muriatic acid, but a new acid possessing a striking resemblance to that body.

M. Gay-Lussac (to whom M. Clement had furnished some of the substance, and with whom he had made some experiments upon it before the communication of his memoir) on Monday, Dec. 6, read to the Institute a paper, in which he stated that the acid formed by its action on hydrygen is a peculiar one. He mentioned several interesting particulars respecting the mode of its production, and he compared it to oxymuriatic gas or chlorine, and stated that two hypotheses might be formed on its nature; and that it might be considered as an undecompounded substance, or as a compound of oxygen. M. Gay-Lussac is still engaged in experiments on this subject, and from his activity and great sagacity, a complete chemical history of it may be anticipated. But as the mode of procuring the substance is now known to the chemical world in general, and as the combinations and agencies of it offer an extensive field for inquiry, and will probably occupy the attention of many persons; and as the investigation of it is not pursued by the discoverer himself, nor particularly by the gentlemen to whom it was first communicated; I shall not hesitate to lay before the Royal Society an account of the investigations I have made upon it; and I do this with the less scruple, as my particular manner of viewing the phænomena has led me to some new results, which probably will not be considered by the Society as without interest in their relation to the general theory of chemistry, and in their possible application to some of the useful arts.

The first experiments that I made on this substance, were to ascertain whether (*argentane*) muriate of silver could be formed from its solution in water or alcohol, and for this purpose it was purified by distilling it from lime. Its solution I found, when mixed with solution of nitrate of silver, deposited a dense a dense precipitate of a pale lemon colour: this precipitate, when collected and examined, proved to be fusible at a low red heat, and then became of a red colour. When acted upon by fused hydrate of potassa, it was rapidly decomposed, and a solid substance, having all the characters of oxide of silver, was formed. The matter soluble in water separated by a filter, and, acted upon by sulphuric acid, afforded the peculiar substance.

A solution of potassa, after being boiled on the precipitate, afforded the peculiar substance, when treated by the same acid.

The precipitate was much more rapidly altered by exposure to light, than the muriate of silver, and was evidently quite a distinct body.

Conceiving, from the action of potassa upon it, that it must be a compound of the peculiar substance and silver, I endeavoured to form it directly by the combination of the two bodies. I introduced some of the substance into the closed end of a small glass curved tube, and placed in the upper part of it some silver foil; I heated the foil nearly to redness, and then passed the substance over it in vapour: there was an immediate action, the silver was rapidly dissolved, and a fusible substance formed, in all its obvious sensible and chemical characters, the same as that obtained from solutions of the substance by nitrate of silver.

The modes which occurred to me, as most likely to effect its decomposition by chemical agents, were the action of the highly inflammable metals upon it which unite to oxygen and chlorine, or the action of chlorine which in general tends to the expulsion of oxygen, and to the separation of inflammable bases from that principle.

I heated some potassium in a little glass tube, and passed some of the substance in vapour over it: at the moment the vapour came in contact with the potassium, there was an inflammation, and the potassium burnt slowly with a pale blue light. There was no gas disengaged when the experiment was repeated in a mercurial apparatus.

The substance formed by the action of potassium was white, fusible at a red heat, and soluble in water. It had a peculiar acrid taste. When acted upon by sulphuric acid, it effervesced, and the peculiar substance appeared.

It was evident that in this experiment there had been no decomposition of the body; the result seemed to depend merely upon the combination of it with the potassium.

I exposed the body to the action of chlorine in a small glass tube; it absorbed the chlorine, and a substance formed which was volatile by heat, and which appeared as a yellow solid; it was soluble in water, and rendered the water of a yellowish-green colour and strongly acid; the solution when acted upon by by solution of potassa not in excess effervesced, and afforded the peculiar substance.

The acid formed by the solution of the substance united to chlorine reddened vegetable blues by its immediate contact, and soon after destroyed them.

When the new substance was heated in oxygen gas, or brought in contact with red hot hyperoxymuriate of potassa, it seemed to undergo no change.

MM. Desormes and Clement had stated, that, when the substance is combined with the metals, metallic oxides could be obtained from the solutions; I suspected that this depended upon the presence of moisture, or upon oxygen derived from the air, and experiment justified my suspicion.

I heated the substance with iron, mercury, tin, zinc, and lead, out of the contact of air; it united to them without any violence of action, and formed compounds fusible at a moderate heat, and volatile at a higher temperature. All the compounds, except that of zinc, which was white, were coloured of different shades of red brown, red and orange; the compound it formed with tin was of a deep orange, that with iron of a bright red brown, that with lead a bright orange, that with mercury an orange still more approaching to red, and which, when crystallized, was bright crimson.

The compound of iron and the substance, when exposed to an alkaline solution, immediately deposited black oxide of iron; but when I heated it in a small retort containing pure ammoniacal gas, no such change occurred, and it combined with the ammonia, and formed a compound which volatilized without leaving any oxide.

The compound of the substance with tin was soluble in water, and had the characters of an acid. It combined with the alkalies without depositing oxide.

The crimson compound of the substance with mercury united in the same manner without decomposition to potassa, and by the action of sulphuric acid, sulphate of potassa was formed, and the compound of the substance with mercury disengaged.

When the substance is made to act upon phosphorus, the two bodies combine with great rapidity at common temperatures, producing heat without light; small quantities of a strongly acid gas generally arise from the mixture, and by the application of heat it is produced in greater quantities. When the substance is in excess, an easily fusible and volatile compound of a red colour is obtained; when the phosphorus is in excess, the greater part of the product is more fixed.

I examined the gaseous acid formed by the action of phosphorus with attention. It gives dense white fumes by combining

bining with the aqueous vapour in the air. It has a smell very similar to that of the solid compound of chlorine and phosphorus, which itself is very analogous to that of muriatic acid. It is rapidly absorbed by water. When made to act upon ammonia, it forms with it a dense white salt, which, when acted upon by sulphuric acid, affords the peculiar substance, and at the same time a smell of hydrogen is perceived. When mercury is heated in the acid gas, the same compound as that produced by the action of the new substance directly upon mercury is formed, and hydrogen equal to half the volume of the gas is disengaged. When potassium is made to act upon it, there is no inflammation as in muriatic acid gas, but the potassium becomes converted into a body similar to that produced by its combustion in the vapour of the substance, and a gas equal to half the volume of the acid gas, which burns in the same manner as hydrogen, is disengaged.

When the easily fusible and volatile compound of the substance with phosphorus is heated in water, it rapidly dissolves in it, and forms a strong acid, which, when evaporated, leaves hydrophosphorous acid, and which, before its evaporation, neutralized by potash, and acted on by sulphuric acid, affords the peculiar substance.

When the difficultly fusible substance it forms with phosphorus is acted on by a small quantity of water, and heated in a glass tube, much gas spontaneously inflammable is disengaged, and a white sublimate arises, which, when acted on by cold water, becomes hot, and affords a considerable quantity of a gas having all the properties of hydrophosphoric gas.

The solution of this crystalline substance in water, neutralized by potash, and decomposed by sulphuric acid, afforded the peculiar substance; but when the solution was heated strongly before its neutralization, it left only hydrophosphorous acid, which when heated gave off hydrophosphoric gas, and became phosphoric acid.

It is easy to explain all these phænomena, except the production of the acid gas, which is a compound of the peculiar substance and hydrogen: to account for the appearance of this body, it is necessary to suppose the existence of hydrogen or of water in the substance, or of hydrogen in phosphorus.

I used the substance distilled through quick lime, which there is every reason to believe would absorb all the water united to it: in this case the acid gas, which gave hydrogen when decomposed by mercury, was produced in much smaller quantities; but, when the substance was moistened, the gas was afforded in very large quantities It is probable, that a little hydrogen existing in the phosphorus, and which appears when that substance is acted on by Voltaic electricity, may influence the result; but I am inclined to attribute it principally to the moisture adhering to the substance, and I have never been able to produce more gas from the fusible compound by distilling it with a new quantity of phosphorus.

When the fusible compound of the substance with phosphorus is distilled with a small quantity of water, the gas produced seems to be of the same kind as that obtained by the action of heat during the combination; and both these gases when absorbed by water afford, when acted upon by nitrate of silver, the same product as that formed by the action of a solution of the substance in water on the same salt.

I attempted to form a compound of the substance with hydrogen directly, by heating it in several experiments to redness in a glass tube filled with hydrogen. When the gas was moist, or when the tube contained vapour, a strong acid fluid was formed of a deep yellow colour. When the gas and the substance were dry, there was an expansion of volume; and on breaking the tube, fumes appeared similar to those produced by the action of the gas formed during the union of phosphorus and the substance, and which precipitated in the same manner a solution of nitrate of silver. This peculiar acid, which consists of the substance united to hydrogen, has a very strong attraction for water, and a very small quantity of water absorbs a large quantity of the gas; and when combined with water, it rises with it in vapour, and in its state of liquid acid it rapidly dissolves the substance, and becomes tawny.

The new substance, as MM. Desormes and Clement have shown, is rapidly soluble in solution of potash; when it is in excess the solution becomes red brown. On evaporating the mixture and heating it to redness, a substance is formed, exactly similar to that produced by the combination of the substance with potassium.

As potassa is a compound of potassium and oxygen, it is evident that, to form a compound of potassium and the substance from potassa, oxygen must be expelled, and I found by experiment that this was the case; and in investigating minutely the action of fixed alkaline solutions on the substance, I ascertained the existence of a class of substances, precisely similar to the hyper-oxymuriates, consisting of oxygen, the substance and potassium, and formed in a manner exactly analogous.

If the substance is thrown into a moderately strong solution of potassa, as it dissolves, crystals fall down, and by saturating the

the solution with the substance, considerable quantities are obtained. By pouring off the mother-liquor and evaporating it a little, more of the crystals fall down.

All these crystals, if precipitated from a solution not too much saturated, are of the same kind; they are little soluble in water, have a taste analogous to that of the hyper-oxymuriates of potassa, scintillate when thrown upon burning coals, and form a deflagrating mixture when mixed with charcoal. When fused, they give off abundance of oxygen gas, and become the same substance as that formed by the action of potassium on the new substance.

If the liquor which has ceased to afford the crystals be evaporated to dryness, it yields a considerable quantity of a substance which is not capable of detonating with combustible bodies, and which is the same as that afforded by the combination of the substance with potassium.

It is evident then that the oxygen contained in the potassa is newly combined by the action of the new substance, and two compounds formed; one consisting of potassium and the new substance, the other of potassium, the new substance, and the oxygen contained in the potassa.

By passing the vapour of the substance over dry red hot potassa formed from potassium, oxygen is expelled, and it appears that oxygen cannot remain in the triple compound at a heat above the red heat.

By dissolving the substance in solutions of soda and baryta similar results are obtained, and in each case two compounds are formed. The oxygen is condensed in one, and the other consists simply of the new substance and a metal.

To separate entirely the deflagrating salt from the soluble salt is not easy, there always remains in the mother-liquor a little of the deflagrating compound; but by separating the first crystals from solution of potassa not too strong, the deflagrating compound of oxygen, potassium, and the substance is obtained, apparently pure.

As the new substance combines with potassium and the metals with much less energy than chlorine, it occurred to me, that it would probably be expelled from its combinations by that body; and this I have found to be the case in all the experiments I have made. When the compound of the new substance and potassium is heated in contact with chlorine, potassane (muriate of potassa) is formed, the violet gas appears, but soon combines with chlorine, and they form together the peculiar acid compound I have before described; but towards the end of the process, as the proportion of chlorine diminishes, the violet-coloured gas again appears.

When

When the compound of the substance with silver was treated in the same manner (*argentane*) muriate of silver was formed, and the substance combined with chlorine at the commencement of the operation, but was disengaged uncombined towards the end.

Similar phænomena occurred when the compounds of the substance with mercury and lead were acted on by chlorine.

The action of acids on the compounds of this substance is what might be expected from its analogies to chlorine.

When concentrated sulphuric acid is poured upon the compound of the substance and potassium, some of the substance appears; but a part of it rises in combination with hydrogen and water, and condenses by cold, and appears of a deep orange colour from having dissolved some of the substance. The sulphuric acid likewise seems to retain some of the substance; for it continues red after being strongly heated, and the acid is partly decomposed, for sulphurous acid gas is disengaged.

It seems probable, that the acid is decomposed to furnish oxygen to a portion of the potassium which quits in consequence the new substance, and that water is likewise decomposed to furnish hydrogen to another portion of the substance; and that the hydrogen and the substance, in their acid form, combine with the water of the sulphuric acid, and rise in vapour, sulphate of potassa being at the same time produced.

When sulphuric acid is poured on the salt consisting of the substance, oxygen, and potassium, the substance reappears, and there is a slight effervescence. In this case, part of the oxygen is employed to form potassa, and the remainder is expelled unaltered.

When concentrated nitric acid is made to act on the triple compound, similar phænomena occur, and the substance reappears with effervescence.

When nitric acid is applied to the double compound, there is a smell of nitrous gas, and the substance is instantly reproduced.

With concentrated muriatic acid, the phænomena presented by the two different alkaline compounds, the binary and the triple, are very interesting. When the acid is brought in contact with the triple salt, there is no effervescence, but a substance, which appears to be a compound of chlorine and the new body, is formed and dissolved in the water of the acid, and potassane is precipitated.

When the double compound is used, there is a complete solution with a partial decomposition; and by applying a gentle heat, the excess of muriatic acid is driven off, and the same acid as that procured by the action of the substance on hydrogen remains

mains dissolved in the liquor. When mixtures of the two salts are employed, the substance itself appears.

It appears that in the instance when the triple compound is employed, there is not only sufficient oxygen to attract the hydrogen from the chlorine which is to combine with the potassium, but likewise enough to decompose a portion of muriatic acid, sufficient to afford chlorine to enter into combination with the whole of the substance.

When the binary compound is used, the result is a simple instance of double affinity; the new substance quits the potassium to unite to the hydrogen of the muriatic acid, and the chlorine and the potassium combine: and that the decomposition is only partial depends probably upon the attraction of the compound of iodine and potassium for water. When mixtures of the compounds are used, the oxygen is employed to attract hydrogen from the substance, to which it seems to adhere with a much weaker attraction than to chlorine.

MM. Desormes and Clement have mentioned, that when the new substance is exposed to liquid ammonia, a black powder is formed, which, when dry, fulminates by the slightest contact or friction. I introduced some of the substance into solution of ammonia, and separated the liquor from the black powder, and evaporated it to dryness; it left a white saline substance, the same as that produced by the union of ammonia with the peculiar acid, which consists of the substance combined with hydrogen; and hence it appeared probable that a portion of ammonia had been decomposed to furnish hydrogen.

I made the experiment on the action of strong solution of ammonia on the substance in a pneumatic apparatus; but no azote was given off. Hence I am induced to conclude, that the black powder is a compound of the new substance and azote, similar in its character of a binary compound to the detonating oil discovered by M. Dulong; and this conclusion is strengthened by the results of its detonation in a tube of glass partially exhausted: they are, I find, the peculiar substance and a gas which is not inflammable, and which does not support flame; and unless the substance is moist, I have never been able to discover any other product; but the minute quantity I have employed prevents me from being confident on this point.

It was an object of considerable interest to ascertain the proportion in which the new substance combines, as compared with that in which the other substances that form acids by their action on inflammable bodies enter into union.

I made several experiments on this subject. Four grains of hydrate of potassa, I found, were saturated by 6·25 of the new substance, and 2·8 the quantity of potassium in four grains of hydrate

hydrate of potassa is to 6·25 the quantity of the substance, as 75, the number representing potassium, is to 166. Again, one grain of hydrate of soda required 2·1 grains of the new substance for its saturation, and one grain of hydrate of soda contains ·578 of sodium; so that, supposing the combination of the new substance with sodium to contain a double proportion of the substance, the number representing the proportion in which it combines will be nearly 160.

Two grains of the compound of the substance with sodium decomposed by sulphuric acid afforded 128 of dry sulphate of soda, and calculating on this experiment the number is 165·5.

I have made some experiments on the quantity of the substance absorbed by tin, mercury, and lead. Mercury absorbs nearly three-fourths of its weight of the new body to become the crimson substance; from which it appears that it must absorb two proportions.

My experiments have been made upon quantities too small to afford very exact results; but they show that the new substance enters into union in a quantity much more than twice as great as that of chlorine, and, considered as an element, it offers a number much higher than those of the simple inflammable bodies, and higher even than those of most of the metals.

The most correct mode of ascertaining the number representing the proportion (supposing it to be definite, as is the case with all other bodies that have been accurately examined) in which it combines, will probably be by ascertaining the specific gravity of its gaseous compound with hydrogen. This gas, as I have stated, affords only half its volume of hydrogen, and it appeared to me to neutralize an equal volume of ammonia; so that, supposing it to consist of two proportions of hydrogen, and only one of the substance, that is, to be analogous to muriatic acid gas in its nature, it must be one of the heaviest elastic fluids existing.

Taking the number representing the proportion in which the new substance combines as 165, and supposing that it occupies the same volume in this gas that chlorine occupies in muriatic acid gas, 100 cubical inches of the gas will weigh at mean temperature and pressure 95·27 grains, *i. e.* supposing hydrogen in the same quantity to weigh 2·27 grains.

I am not at present in possession of an apparatus for weighing the gas with accuracy. A particular device will be required for this purpose, as the gas cannot be preserved over mercury. It may be collected during the action of phosphorus on the moistened substance in a vessel exhausted of air; or it may be made by heating the compound of the substance and potassium in muriatic acid gas in a glass vessel: in this case, there is, I find,

find, a double decomposition, the chlorine quits the hydrogen to unite to the potassium, and the substance quits the potassium to unite to the hydrogen.

The new substance, I find, is not decomposed when Voltaic sparks are taken in it in its gaseous state from ignited points of charcoal: at first there are white fumes, probably from the action of moisture or hydrogen in charcoal, on the substance; but these fumes soon cease, and when the tube in which the experiment is made is cooled, the substance appears unaltered.

From all the facts that have been stated, there is every reason to consider this new substance as *un undecompounded body*. In its specific gravity, lustre, the high number in which it enters into combination and colour, it resembles the metals; but in all its chemical agencies it is more analogous to oxygen and chlorine; it is a non-conductor of electricity, and possesses, like these bodies, the negative electrical energy with respect to metals, inflammable and alkaline substances; and hence, when combined with these substances in aqueous solution and electrized in the Voltaic circuit, it separates at the positive surface: but it has a positive energy with respect to chlorine; for, when united to chlorine in the compound acid I have described in pages 5 and 6, it separates from the chlorine at the negative surface. This likewise corresponds with their relative attractive energy. Chlorine expels the new substance from all its combinations on which I have made any experiments.

The new substance seems to possess a stronger attraction for most of the metals than oxygen; but it is expelled from phosphorus and sulphur by oxygen: I found by passing oxygen and the compound of it with phosphorus through a glass tube heated red, phosphorous acid was formed, and the violet gas appeared.

That it produces so little heat and so seldom light in entering into combination, may be accounted for from its solid form and its great weight as an element. Potassium, however, as I have mentioned, burns in the violet-coloured gas, and when this gas is thrown upon the flame of hydrogen, it seems to support its combustion.

The saturating or neutralising powers of the new substance appear to be greater than those of oxygen, and less than those of chlorine.

It agrees with chlorine and fluorine in forming acids with hydrogen, and it agrees with oxygen in forming an acid with chlorine.

In my first experiments I conceived that it formed substances analogous to alkalies in combining with the alkaline metals; for the compound produced by its action upon solution of potassa, even when the substance was in great excess, reddened turmeric

turmeric paper, and rendered green paper tinged with the juice of violets: but I have since found that this is owing to a small quantity of subcarbonate of potassa which existed in the hydrate of potassa; and when the compound is treated with the acid the substance forms with hydrogen, and heated to redness, it loses this property; and when thus formed, its taste more resembles that of a neutral salt than of an alkali. I cannot yet say with certainty whether its compound with potassium has powers, like the oxides, of neutralising those acids which it does not decompose, as in all the experiments I have made on this point I used the compound which reddens turmeric: this neutralised the phosphorous, sulphurous, and boracic acids; but the effect may possibly depend upon the undecomposed carbonate.

The name *ione* has been proposed in France for this new substance from its colour in the gaseous state, from ἴον, *viola;* and its combination with hydrogen has been named *hydroionic acid.* The name *ione,* in English, would lead to confusion, for its compounds would be called *ionic* and *ionian.* By terming it *iodine,* from ἰώδης, violaceous, this confusion will be avoided, and the name will be more analogous to chlorine and fluorine.

The acid it forms with hydrogen may, however, be with propriety named in our language *hydroionic acid.* I venture to propose for the acid it forms with chlorine, the name of *chlorionic acid,* and for that it forms with tin *stannionic acid.* With respect to the other compounds, they may be called as a class *iodes,* with the name of the base, as *iode of mercury,* and with *proto, deuto,* &c. to signify the proportions; or if a termination to the base should be preferred, as I have proposed for the combinations of chlorine, the terminations may be in *m,* with the vowels in their usual order to signify proportions. Thus, *phosphoroma* would signify the combination of one proportion of iodine with phosphorus, and *phosphoromè* would signify two proportions of iodine to one of phosphorus.

If this last plan, which involves no theoretical views, should be adopted, it may be extended with different consonants to the combinations of *fluorine,* and the vowel may be made to signify the proportion, and the consonant the nature of the compound. The vowel of termination, to the Latin name of the base, I have already proposed, on another occasion, for the compounds of oxygen. Thus, *argenta* may be made to signify the protoxide of silver, and *ferré* the deut-oxide of iron. *n* is the consonant which I have suggested to represent the combination of chlorine, as *argentana* the *protochloride* of silver; and *l* in this system may represent fluorine. Thus *calcala* would be fluor spar, or one proportion of fluorine and one of calcium; and the different combinations of calcium with the supporters of combustion

bustion with oxygen, fluorine, chlorine, and iodine in one proportion, would be thus expressed—*calca, calcula, calcana*, and *calcama*.

I throw out these hints for discussion, rather than with any wish for their adoption, and for the purpose of directing the attention of chemists towards the subject of nomenclature, which ought to be settled on some fixed principles; and in naming a new class of compounds, great caution should be used to prevent the necessity of alteration.

In my last paper, presented to the Society two months ago, I ventured to suggest that it was probable, that new species of matter, which act with respect to inflammable bodies, like oxygen, chlorine and fluorine, would be discovered. I had not hoped, at that time, to be able so soon to describe the properties of a body of this kind, which forms an acid with hydrogen, like chlorine and fluorine, and which in some of its combinations resembles oxygen.

This new fact will, I hope, do something towards settling the opinion of chemists respecting the nature of acidity, which seems to depend upon peculiar combinations of matter, and not on any peculiar elementary principle.

It is probable that *iodine* will be found in many combinations in nature. We may expect that it will be discovered in various marine vegetables, and in sea water; and probably the loss of weight indicated in the analysis of certain fossil substances may depend upon its expulsion.

Its compounds with the metals will probably form a new class of pigments; and it is not impossible, that the triple salts it forms containing oxygen, may be made substitutes for nitre in the manufacture of gunpowder.

Paris, Dec. 10, 1813.

III. *Proposal for an Improvement of the Galvanic Trough.*

March 14, 1814.

Sir, — If you should deem the following worthy a place in your next number, it is much at your service. I beg to remain

Your most obedient servant,

To Mr. Tilloch. A. B.

The late important and highly interesting discovery of Sir H. Davy has rendered the Voltaic battery an apparatus of such extensive application, that any improvement, or any suggestion which may tend to an improvement, cannot be unacceptable.

Plate I. fig. 1. A. The trough, which should be made of earthenware, with partitions and lateral grooves for the plates to slip into.

into. B, small conical vessels (as fig. 6.) for the purpose of containing mercury, to insure the absolute contact of the wires of the connecting rod *c*, which must be made of baked wood. DD, wires for connecting the plates. E, the stand and adjusting screw. 4. Glass plate and stand. 5. An apparatus for fixing the conducting wires to, which will be found very useful. It consists of the joint A, and two pieces of glass tube and platina wires. It will also remove the inconvenience of holding the wires. Figs. 2. and 3. are longitudinal and transverse sections.

It must appear very evident, that the above will form an apparatus precisely similar to the *couronne des tasses* of Volta, when the rod *c* is let down.

I do not pretend to say this would be an improvement; but it is an interesting question, whether a battery on this plan would not act with a greater promptitude. If after trial it should not be found to possess any peculiar advantages, it will be at least well adapted for the public lecturer, as his young hearers will be enabled to form a better idea of the nature of Voltaic electricity. I ought, however, to state that I have not myself had an opportunity of trying the arrangement which I have suggested.

IV. *Errors of* Criticus Secundus *in his Communication respecting the Nautical Almanac for* 1815.

To Mr. Tilloch.

Sir, — I have sincerely to entreat your forgiveness for having myself committed an error in one part of my communication of last month, concerning the errors in the Nautical Almanac for the year 1815, where I have copied out the Trinity Sundays in the two months that are *right*, instead of those that are *wrong*. Thus we see that sometimes errors are catching. The only reparation I can now make, is to announce the mistake, and point out the correction, by observing that the numbers of the Trinity Sundays in the two months of July and August are *right* as printed in the Almanac, but that all those in the four months June, September, October, November, as printed, are *wrong*, being all *one* too little.

I am, sir,
Your humble servant,
July 11, 1814. Criticus Secundus.

V. *An*

V. *An Account of a Family having Hands and Feet with supernumerary Fingers and Toes.* By ANTHONY CARLISLE, *Esq. F.R.S. In a Letter addressed to the Right Hon. Sir* JOSEPH BANKS, *Bart. K.B. P.R.S.**

DEAR SIR,—THE following account of a family having hands and feet with supernumerary fingers and toes, and the hereditary transmission of the same peculiarity to the fourth generation, appears to be worth preserving, since it displays the influence of each of the propagating sexes; the male and the female branches of the original stem having alike reproduced this redundancy of parts. I have carefully inspected two persons of this family at the time of their being in London, namely, Abiah Colburn, and his son Zerah Colburn, and have taken the particulars of the rest from Abiah Colburn himself, whose narrative was several times repeated to me, without any deviation.

Zerah Colburn, a native of the township of Cabot in the province of Vermont, in North America, has been lately brought to London, and publicly exhibited for his extraordinary powers in arithmetical computations from memory. This boy has a supernumerary little finger growing from the outside of the metacarpus of each hand, and a supernumerary little toe upon the outside of the metatarsus of each foot. These extra fingers and extra toes are all completely formed, having each of them three perfect phalanges with the ordinary joints, and well shaped nails.

Abiah Colburn, the father of Zerah, has five fingers and a thumb upon each hand, and six toes on each foot; he has also five metacarpal bones in each hand, and six metatarsal bones in each foot. The extra limbs have distinct flexor and extensor tendons.

The wife of Abiah Colburn has no peculiarity in her limbs. During the existing marriage, she has borne eight children, six sons, and two daughters. Four of those sons inherit the peculiarity of their father more or less complete, while the two daughters are free from the family mark, as well as two of the sons, namely, the fourth in succession who was a twin, and the eighth.

The eldest son of these parents, named Green Colburn, has only five toes on one of his feet, but the other foot and both his hands possess the extra limb.

The second child, Betsy Colburn, is naturally formed.

The third, Zebina Colburn, has five fingers and a thumb upon each hand, and six toes upon each foot.

The fourth and fifth were twin brothers, and named David

* From the Philosophical Transactions for 1814, part i.

and Jonathan; David, who is dead, had nothing of the father's mark, but,

Jonathan has the peculiarity complete.

The sixth, Zerah Colburn, the extraordinary calculating boy, is marked like his father, as before described.

The seventh, Mary Colburn, is naturally formed.

The eighth and last child, Enas Colburn, is also exempt from the father's peculiarity.

Besides the persons I have mentioned, this hereditary redundance of limbs has been attached to the little fingers and to the little toes of several of the ancestors of the family. The mother of Abiah Colburn brought the peculiarity into his family. Her maiden name was Abigail Green: she however had not the extra finger on one of her hands; the other hand and her feet were similarly marked with those of her son Abiah.

David Colburn, the father of Abiah, had no peculiarity. By his marriage with Abigail Green, he had three sons and one daughter. Two of these sons and the daughter were fully marked in all the limbs; the other son had one hand and one foot naturally formed.

Abigail Green inherited these supernumerary limbs from her mother, whose maiden name was —— Kendall, and she had five fingers and a thumb upon each hand, and six toes on each foot.

The marriage of —— Kendall with Mr. —— Green produced eleven children, whom Abiah Colburn's mother, who was one of the eleven, reports to have been all completely marked: but the present family are unacquainted with the history of the other ten branches, and they do not possess any knowledge of their ancestors beyond —— Kendall, the great grandmother of Zerah Colburn.

Numerous examples of the hereditary propagation of peculiarities have been recorded: all family resemblances, indeed, however trifling they may appear to a common observer, are interesting to the physiologist, and equally curious; though not so rare as those described in the preceding history. In every department of animal nature, accumulation of facts must always be desirable, that more reasonable inductions may be established concerning the laws which direct this interesting part of creation: and it might be attended with the most important consequences, if discovery could be made of the relative influence of the male and female sex in the propagation of peculiarities, and the course and extent of hereditary character could be ascertained, both as it affects the human race in their moral and physical capacities and as it governs the creatures which are subdued for civilized uses. Nor is it altogether vain to expect, that

that more profound views and more applicable facts await the researches of men, who have as yet only begun to explore this branch of natural history, by subjecting it to physical rules.

Though the causes which govern the production of organic monstrosities, or which direct the hereditary continuance of them, may for ever remain unknown, it still seems desirable to ascertain the variety of those deviations, and to mark the course they take, where they branch out anew, and where they terminate. There is doubtless a general system in even the errors of nature, as is abundantly evinced by the regular series of monstrosity exhibited both in animals and vegetables.

It has happened in my professional capacity, that I have had to extirpate a supernumerary thumb from each of the hands of two girls, who were both idiots, though the families to whom they belonged were unknown to each other. I have seen many instances of supernumerary thumbs and supernumerary fingers in persons to whom the singularity was not hereditary, and I have read of many others; but whether of my own experience, or of authentic record, the redundancy has been on the outer side of the little finger, and outer side of the thumb, never on the back or inside of the hand, or on the sides of the intermediate fingers: and in similar cases as to the toes, the rule has been invariably the same. In the Sacred Writings an example of this kind is given, II Samuel, ch. xxi. ver. 20. "And there was yet a battle in Gath, where was a man of *great* stature, that had on every hand six fingers, and on every foot six toes, four-and-twenty in number; and he also was born to the giant." The same account is repeated in I Chronicles, ch. xx. ver. 6.

In the *Elementa Physiologiæ* of Baron Haller, numerous examples of this deformity are cited from various authors, with some instances of their hereditary descent, and others of a cutaneous junction between the extra limbs and the next adjoining*.

That local resemblances, such as those of external parts, the hands, the feet, the nose, the ears, and the eye-brows, are hereditary, is well known; and it is almost equally evident, that some parts of the internal structure are in like manner transmitted by propagation: we frequently see a family form of the legs and joints, which gives a peculiar gait, and a family character of the shoulders, both of which are derived from an hereditary similarity in the skeletons. Family voices are also very common, and are ascribable to a similar cause. Apparently many of our English surnames have been taken from the hereditary peculiarities of families, and the same practice existed

* Vide vol. viii. p. 98.

among the Romans: Pliny, in his eleventh book, chap. xliii. relates an instance of a Roman poet, named Volcatius, who had six fingers on each hand, and received the surname of Sedigitus in consequence. He also states, that two daughters of a noble Roman, named M. Curiatius, had each six fingers, and that they took the surname of Sedigitæ. Persons who had the surname of Flaccus were so called from their pendulous ears; and numerous other instances are recorded by classic writers of surnames being derived from family marks.

Anatomical researches have not been so generally extended as to determine the prevalence of internal peculiarities, and perhaps they do not reach to the sanguineous system. I have known two instances, in two different families, of the high division of the brachial arteries having the ulnar branch placed above the fascia of the biceps muscle at the inner bend of the elbows, and yet the father, the mother, the brothers and sisters of those two persons were not so formed. Those marks called *nævi materni*, which are derangements of the sanguineous vessels, are not hereditary, whilst less remarkable changes in the ordinary skin are often so. I have lately seen a man, and who is now living, who has a small pendulous fold attached to the skin of his upper eyelid, and the same peculiarity has been transmitted to his four children. It would have been interesting to know, whether any similarity of structure existed in the families of the two rare examples of a total transposition of the abdominal and thoracic viscera. (Phil. Trans. for 1674, No. cvii. p. 146, by Dr. Sampson, and vol. lxxviii. p. 350.)

In particular breeds of animals, the characteristic signs are generally continued, whether they belong to the horns of kine, the fleeces of sheep, the proportions of horses, the extensive varieties of dogs, or the ears of swine. In China the varieties of gold and silver fishes are carefully propagated, and with us, what are vulgarly called "fancy pigeons" are bred into most whimsical deviations from their parent stock.

As wild animals and plants are not liable to the same variations, and as all the variations seem to increase with the degree of artificial restraint imposed, and as certain animals become adapted by extraordinary changes to extraordinary conditions, it may still be expected that some leading fact will eventually furnish a clue, by which organic varieties may be better explained. A few generations of wild rabbits, or of pheasants under the influences of confinement, break their natural colours, and leave the fur and feathers of their future progeny uncertainly variegated. The very remarkable changes of the colour of the fur of the hare, and the feathers of the partridge, in high northern latitudes, during the prevalence of the snow, and the adaptation

adaptation of that change of colour to their better security, are coincidences out of the course of chance, and not easily explained by our present state of physical knowledge.

I have the honour to be, dear sir,

Your much obliged and obedient servant,

To the Right Hon. Sir Joseph Banks, Bart. K.B. P.R.S. &c. ANTHONY CARLISLE.

VI. *Description of a new Apparatus for preparing with Facility and Œconomy pure Muriatic Acid.* By L. V. BRUGNATELLI. *Read in the Italian Institute*.*

OF all the chemical apparatus generally used for preparing the acids, unquestionably that employed in the manufacture of muriatic acid is the most complex and expensive. For this purpose, a large tubulated retort, a balloon glass with two necks or a tubulated globular glass, Woulfe's apparatus, tubes of security, and lutes of the best quality, are required. The operator also requires much dexterity to unite these divers pieces, to conduct the operations, and great care in transferring the concentrated muriatic acid from the balloon glass to the bottles in order to avoid its noxious fumes. However well the process may be conducted, the acid, by the common method, still retains a dirty yellow colour, and is injured by sulphuric acid. The means adapted for purifying it are very troublesome, and are likewise attended with a considerable loss of acid.

Considering that all muriatic acid is disengaged from common salt in the state of gas, and that this gas combines very rapidly with pure water, I have devised a simple apparatus with which it can be condensed by water, purified from the sulphuric acid and the greater part of the colouring matter. Having made the experiment several times, I have found it always answer perfectly my expectations. This new apparatus consists of only three pieces: 1st, a matrass which may or may not be tubulated; 2dly, a bent tube made in the manner represented in fig. 8. Plate I. which constitutes the most important part; and 3dly, a common glass receiver rather narrow and high, its mouth ground and made to receive a ground stopper. In the matrass *a*, fig. 1. placed over a sand-bath, put, for example, eight ounces of clean culinary salt dried to a coarse powder. In the bent tube *b b*, introduce a solution of muriate of barytes, which should be raised to the same height in the two arms *e*, forming its curvature. Pour five ounces of sulphuric acid on

* From Brugnatelli's *Giornale de Fisica, Chimica, Storia Naturale,* &c. vol. vii. April 1814.

 the

the salt; to the neck of the matrass immediately adapt the bent tube, inserting it through a cork, which also contains a capillary tube communicating with the matrass, (if the latter be not tubulated,) and made air-tight with some soft wax. When the white fumes appear in the other extremity of the bent tube it must be promptly inserted in the mouth of the receiver, which should contain distilled water equal in weight to that of the salt employed, but not occupying above half its capacity. The mouth of the receiver must be slightly closed with a cork through which the long limb of the bent tube passes. As soon as the muriatic gas has begun to be disengaged, continue it without interruption at the ordinary temperature and pressure of the atmosphere, and, after a time, add fire to the apparatus in the course of the process, in order to develop the gas with greater rapidity.

The solution of muriate of barytes put into the curvature *e* of the tube *b b* ascends during the passage of the gas into the expanded part *f*, which is partly filled again; and hence all the acid gas is constrained to traverse it, and as it were wash itself, previous to reaching the water in the receiver *C*. If in the course of the process the solution of barytic salt put into the bent tube decomposes and becomes turbid, it at the same time assumes a dirty yellow colour. The acid gas which passes into the receiver *C* is sometimes in such a quantity in the lower part of the tube, that the water about it becomes very hot. For this reason I have found it convenient to immerse the receiver *C* in a larger vessel *d* containing cold water, or, what is still better, water mixed with snow. Near the end of the process, when the hydrate of muriatic acid (muriatic gas combined with water) in the receiver *C* begins to ascend up by the tube *b*, the capillary tube *g* in the matrass must be opened to allow the apparatus to cool. The bent tube is afterwards withdrawn, and the mouth of the receiver *C*, containing the muriatic acid, immediately stopped.

Distilled water at the mean temperature and pressure of the atmosphere absorbs about 450 times its own volume of muriatic gas, and increases about one-third its original bulk. The muriatic acid obtained by this apparatus is found very strong, fuming, transparent, of a yellowish colour, and entirely divested of sulphuric acid. When tubulated matrasses are not at hand, we may use, as before observed, a capillary tube through the cork which closes the untubulated matrass.

In order that the process may be finished with safety and ease, it is better to operate on a small quantity of salt, and repeat the process oftener if required. Thus the matrass or retort may be of small size, and serve for the same process many times.

VII. *An*

VII. *An Account of some coloured Snow and Hoar-frost which fell at Arezzo March* 1813. *By M.* Fabroni, *of Arezzo**.

Pliny and Livy have mentioned showers of burnt bricks, and much ridicule has been thrown upon them for it.

If their expressions however are to be interpreted, as seems to be reasonable, by the fall of a powder similar to that of pounded bricks, this phænomenon was seen in our days throughout the whole of Tuscany, and perhaps still further.

I now write from Arezzo, in the department of the Arno, and all that I am going to say on this extraordinary and curious subject is applicable to the spot where I now am.

During the evening of the 13th of March 1813, the ground being almost entirely covered with snow, there fell a new quantity of snow, or rather hail not very compact, of a reddish-yellow colour, which the people improperly called red.

This hail seems to have begun to fall at nine in the evening, and to have continued until next morning. The heaviest fall took place about three in the morning.

Lightning was visible during the night: the north wind blew with considerable force at intervals, and a dull uniform noise was heard in the atmosphere, similar to what is produced at a great distance from the sea by a tempest. The sky seemed to threaten snow, and some persons thought it was variegated with reddish-yellow clouds.

The thunder roared once or twice at the time of the heaviest fall of the hail. Next day this hail formed a separate stratum above the snow, from which it was easily distinguished by its colour. The snow which had fallen first was white, although it had acquired a state of congelation very similar to the hail. The colour was the same not only in all the parts of one and the same mass of hail, but also in what fell in the interior of the town, in the plains, and on the adjacent mountains.

When placed in a clean vessel, the surface was covered with a foreign substance in proportion as it melted, and after its fusion it precipitated an earthy sediment and remained turbid for a few hours.

If before liquefying it, it was carefully washed until it lost its colour, the ice remaining melted without yielding any precipitate: in short, it did not differ from the water from which the sediment above mentioned was separated.

This water when filtered was insipid, inodorous, transparent, without colour, incapable of changing turnsole tincture, and of disturbing the solution of nitrate of silver; but it became slightly

* *Annales de Chimie*, tome lxxxviii. p. 146.

opaline

opaline by the use of the ammoniacal oxalate in powder. It was, in short, in appearance similar to that of snow or common hail. Nevertheless, threee kilogrammes of this water, which I reduced by evaporation to one decagramme, immediately assumed all the qualities of spring water kept long in open vessels.

The dry sediment (the proportional quantity of which with the liquid I could not precisely ascertain, but which might be about one gramme to three kilogrammes of hail or snow,) was of great tenuity, had an earthy appearance, was soft to the touch, and was of a dark nankeen yellow colour: it had neither smell nor taste, and was completely incombustible when ignited bodies were applied to it.

It was rough on the tongue, shrunk up in the blow-pipe, and became of an ochery red. It gave nothing to boiling water, even after long digestion. I tried the isolated action of the sulphuric, nitric and muriatic acids on this substance. All these acids produced a brisk but short effervescence: they dissolved a part of the substance, and refused to dissolve the rest.

The solutions are of a fine citron yellow, and the first portions of acid employed were the highest coloured. The tone of colour becomes lower when the solution cools.

All of them give a white precipitate upon the addition of oxalate of ammonia; and after this re-agent has ceased to produce the effect, we obtain a precipitate, also white, but more considerable, by means of carbonate of potash.

The calcareous prussiate forms in it at first a reddish shade, which turns to deep yellow, and afterwards becomes clear: then a small quantity of blue prussiate is precipitated. The nitric solution is rendered turbid by the nitrate of silver: if we dry it, it presents a white and not very combustible residue, to which there must be an addition of acid to make it become soluble in water.

The sulphuric solution when pushed to evaporation over the fire becomes of a deep yellow, and emits sulphurous acid gas. When the matter is dried, if we pass distilled water into it and filter it, the substance separated from the liquor has the colour of bister, is shining and acid, attracts humidity from the air, and assumes in some measure the aspect of artificial tannin. Its small quantity did not permit me to examine it. The filtered liquid is white and acid: by the addition of a little potash, it crystallizes in tetrahedral pyramids joined by their bases. The carbonisation of the sulphuric solution is not obtained, if we previously throw on the earthy sediment a little nitric or muriatic acid.

In general we may say that the acids succeed but imperfectly in dissolving it, notwithstanding the action of heat. A considerable

derable portion of the substance to be analysed, always resists the affusion of new quantities of acid.

The best method of producing an almost complete solution consists in using sulphuric acid, and a considerable quantity of potash, which neverthelcss ought not to neutralize all the acid. By this means we obtain a salt which has the characters of alum, although it contains a proportion of alkali greater than that which is necessary to the crystallization of common alum, and we may dissolve the earth with the exception of the same.

It results from all the above observations:

1. That the snow or hail which fell at Arezzo derived its colour from a very fine earthy substance interposed with uniformity between the small crystals of the hail, without however being inclosed in their nucleus.

2. That this substance is composed almost entirely of alumine, very little carbonated lime, and a still less quantity of iron, manganese, and silex; finally, of a very feeble animal or vegetable principle, capable of being carbonized by the action of sulphuric acid and of putrefying water.

I do not mean to decide upon the etiology of the phænomenon which took place during some grand electrical operations of nature; but it ought by no means to be ascribed to the stormy nature of the winds.

In fact, no storm was experienced in the town or environs: besides, the wind could not produce effects so perfectly identical to a great extent, or in all directions, nor porphyrize the earth, nor divide it with so much regularity in the mass of snow or hail. Lastly, the ground being every where previously covered with snow, it can scarcely be supposed that the wind could take up an argillaceous earth with it to scatter it afterwards with fresh snow.

VIII. *Report of the Progress of the Sciences in France in* 1813. *By* J. C. Delametherie.

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

PHYSICS.

This branch of science determines the laws of motion in various bodies, and these are divided into two parts; viz. those which relate to solid bodies, and those which relate to fluid bodies. Lagrange in his *Mécanique* (tome i.) considers these laws under two different heads. 1. Statics, or the theory of equilibrium. 2. Dynamics, or the theory of motion. The theory of the equilibrium of fluids is consequently denominated Hydro-

Hydrostatics. The theory of the motion of fluids has been subjected by Lagrange to the most rigorous analysis.

1. The properties of the equilibrium of a free system of bodies, relative to the motion of translation.

2. The properties of equilibrium relative to the movement of rotation.

Of Statics. Lagrange has referred all the laws of statics, or of the theory of the equilibrium of solids, to three principles; that of the lever, that of the composition of forces, and that of the virtual velocities.

Of Machines They make up for the weakness of our organs, and new machines are daily invented. The Voltaic pile has been one of the most valuable discoveries of late years for the advancement of science.

Of Hydrostatics and Hydronamics. "Although we are ignorant," says M. Lagrange (tome i. p. 174), "of the interior constitution of fluids, it cannot be doubted that the particles of which they are composed are material, and that for this reason the general laws of equilibrium cannot suit them as they do solid bodies. In fact, the general property of fluids, and the only one which distinguishes them from solid bodies, consists in all their parts yielding to the smallest possible force and moving with all possible facility."

In order to conceive the cause of these phænomena, we must admit that each particle of matter has a *peculiar force*, which it never loses.

From sal ammoniac for instance, when triturated with lime, the volatile alkali is extricated with all its usual vivacity.

From the sulphuric acid, when poured upon the same salt, the marine acid is also extricated with force.

The molecules of the ammonia and those of the marine acid had each therefore a peculiar force which was *in nisu*, like those of a spring.

The molecules of solids, on the contrary, are most frequently nearly immoveable, because their peculiar forces are *in nisu*, as in the metals, stones, &c. Nevertheless they have sometimes particular motions, like those of warm substances, sonorous bodies, electric bodies, &c.

The molecules of fluids have a constant rotatory motion around their axis or point of equilibrium, and they yield to the least force. Whereas solids oppose all their *vis inertiæ* in the ratio of their masses. This is the cause of the differences exhibited by solids and fluids in their motions.

Fluids are divided into two classes: Ethereated fluids, which Davy improperly calls *imponderable* fluids. Non-ethereated fluids,

fluids, such as water, oil, &c. We shall make known the additions which have been made this year to our stock of knowledge on this subject. Bremontier has published some interesting observations on the movement of waves.

Rhuland has made some new experiments on the radiation of heat: he has proved *that the more caloric is lost by bodies by radiation, the more the surrounding bodies are obliged to give up to them their proper heat, in order to re-establish the equilibrium of heat.* (Journal de Physique, Nov. 1813.)

It appears to me that we may conclude that there is thus established a double movement of caloric: viz. a caloric *effluent* from warm bodies, and a caloric *affluent* from bodies less warm.

The greatest heat ever produced has been obtained by Sir Humphry Davy, by means of the Galvanic battery of the Royal Institution. (Here follows a description of the apparatus and experiments, which are familiar to our readers.)

Configliati, by repeating the experiments of Leslie upon artificial cold, has obtained some very curious results, and succeeded in freezing mercury.

CHEMISTRY.

Under this head M. Delametherie passes a just eulogium on the recent labours of Sir Humphry Davy and Berzelius, of whose improvements he gives a full analysis.

He then notices the labours of the French chemists. Bouillon Lagrange and Vogel have furnished a new analysis of different kinds of sea-water; viz. from the Mediterranean, at Marseilles, and the Ocean at Bayonne and Havre.

One thousand grains of sea-water from Havre yielded:

	Grains.
Of Saline matter obtained by evaporation ..	36·
Carbonic acid	0·23
Muriate of soda	25·10
Muriate of magnesia	3·50
Sulphate of magnesia	5·78
Carbonate of lime and magnesia	0·20
Sulphate of lime	0·15

One thousand grains of sea-water from Bayonne afforded:

Of Saline matter by evaporation	38·
Carbonic acid	0·23
Muriate of soda	25·10
Muriate of magnesia	3·50
Sulphate of magnesia	5·78
Carbonate of lime and magnesia	0·20
Sulphate of lime	0·15

One

One thousand grains of sea-water taken at Marseilles yielded:

	Grains.
Of Saline matter by evaporation	41·
Carbonic acid	0·11
Muriate of soda	25·10
Muriate of magnesia	5·25
Sulphate of magnesia	6·25
Carbonate of lime and magnesia	0·15
Sulphate of lime	0·15

The muriate of magnesia is the only deliquescent salt in sea-water; hence arises the property which sea-salt possesses, of liquefying on exposure to air. A small quantity of the muriate of soda contained in sea-water is carried up when we distil it: this is the cause of our finding muriate of soda in vegetables at a certain distance from the sea.

In the chemical analysis of animal substances Berzelius has made great improvements. He has submitted the animal fluids, and particularly the blood, to new analyses. The blood, he says, is composed of two parts; a liquid, the *serum*, and one which is suspended, the *coagulum*. In the animal œconomy we ought to distinguish three principal substances: *a*, fibrine; *b*, albumen; *c*, gelatine.

The serum, according to Berzelius, is a solution of a great quantity of albumen with a little *fibrine*. Both are combined with soda. It also contains some other saline substances.

The coagulum is the colouring matter. It differs from albumen in its insolubility in serum, and by its colour. The colouring matter amounts to about one-third of the mass: the colour seems to be owing to iron, of which it contains about one-third of its weight; but this iron can be separated by combustion only.

This colour cannot be produced artificially by uniting albumen with subphosphate of iron, as Fourcroy and Vauquelin have asserted. Nor it is possible to produce it by uniting iron with soda, as Parmentier and Deyeux have supposed.

We may compare the colour of the blood to the other red colouring principles formed by animals, cochineal, kermes, the purple of the murex, &c.

Four hundred grains of colouring matter, when incinerated, yielded:

	Grains.
Oxide of iron	30·
Subphosphate of iron	7·5
Phosphate of lime and a little magnesia	6·
Pure lime	20·
Carbonic acid and loss	16·5

The

The serum of the blood gave upon analysis:

	Grains.
Water	90·5
Albumen	80·
Muriate of potash, soda, and lactate of soda	4·
Soda, phosphate of soda, and a little animal matter	4·1

In speaking of the lactate of soda, Berzelius observes, that the existence of the lactic acid discovered by Scheele had been erroneously doubted by Fourcroy and Vauquelin. The blood contains no gelatine nor earthy phosphate.

The fibrine, albumen, and colouring matter resemble each other so closely, that they may be considered as modifications of one and the same substance: they give earthy phosphates and carbonates of lime solely when they are decomposed.

The author thinks that the phosphate of iron does not exist in the non-decomposed colouring matter, and that which we obtain by incinerating it is a product of combustion.

The colouring matter dried and exposed to the fire in a red-hot crucible melts, swells, and burns with a clear flame; it leaves a pory charcoal, which burns with difficulty. In burning, a smell of ammonia is constantly exhaled, although it had been exposed several times to a strong fire.

This extrication of ammonia from a burning charcoal, and which has been long exposed to the fire, is according to him a remarkable phænomenon: hence he concludes that this ammonia is a new product.

Margarine.—Chevreul has entered upon an extensive inquiry into the combinations of fat bodies and the alkalis. When we put, he observes, soap from hogs' lard and potash in a great mass of cold water, one part is dissolved and another is deposited in the form of small shining pellets: he calls it *mother of pearl substance* (matière nacrée).

This matter is formed of potash	8·16
Another fat substance	91·84

This last substance has the lustre of a pearl. The author on this account calls it *margarine,* from the Greek word. In order to obtain it, he places the mother of pearl substance in water acidulated with muriatic acid. The acid seizes the potash, and the margarine swims above. The latter is lighter than water, and melts at 56·560 centigrade.

From all the facts recently published by Sir Humphry Davy and Berzelius, M. Delametherie draws the following conclusions:

1. We can no longer regard pure air, or oxygen as it has been called, as the principal agent of heat or of combustion, since it is proved that this gas contains the least caloric of any other, and that the inflammable gas, the hydrogen as it is called, contains the most caloric. Besides, it is proved that in several com-

combinations of oxygen, such as that with the nitrous gas, there is but a very small quantity of heat extricated. The oxygen inspired has little influence in the production of heat in animals and vegetables.

2. Oxygen can no longer be regarded as the generator of acids, hydrogen frequently performing its functions.

a. The acids, such as the fluoric, oxy-muriatic, iodic, hydro-sulphuric, hydro-telluric, and the prussic, contain no oxygen.

b. Substances which contain oxygen are not acids, but rather alkalis; such as potash, soda, the earths, the metallic oxides, &c.

c. The water, which upon this hypothesis contains 0·87 oxygen, is not acid.

3. The inflammable gas or hydrogen is to be found in all the combustible bodies. This is admitted with respect to animal and vegetable substances. It is also proved that it is found in the mineral combustible bodies, such as charcoal, the diamond, sulphur, phosphorus, the metals.

"The combustion of the metals, particularly that of zinc," I observed in 1781, in the *Journal de Physique*, "always suggested to me that inflammable air was one of their principles."

Davy regards this opinion as highly probable, and has assigned the quantities of hydrogen contained in several of these bodies.

4. This inflammable air ought therefore to be regarded as the principle which Stahl called *phlogiston:* this name ought to be continued.

5. This inflammable air of all the acidifiable bodies, the oils, sulphur, metals, is partly consumed when we burn these bodies, and when they pass to the state of acids.

6. The hydrogen so burnt furnishes water, which ought consequently to exist in the new compounds.

7. Berzelius and several chemists are of opinion that inflammable air, or hydrogen, contains a *metallic oxide,* which enters into the composition of *ammonium.*

8. They have the same opinion of azote: it contains a metallic oxide, which also enters into the formation of ammonium.

9. I have observed, as well as Priestley, Senebier, Berger, and others, that these gases, hydrogen and azote, exposed over water, or agitated with water, are decomposed, and pass to the state of pure air: the inflammable air loses its inflammability, and the azote becomes absorbable by the nitrous gas. On the other hand, pure air exposed over water is also decomposed: it is less absorbed by the nitrous gas than before.

We have seen that in respiration there is less carbonic acid produced than oxygen absorbed. This made Berthollet and Delaroche conclude that there had been a production of azote.

10. It

10. It is *probable* therefore that there is only one kind of air, viz. *pure air*, which may be modified into inflammable gas, and into azotic gas.

11. Caloric enters, as the principle of the nitric acid, into the combination of the nitrous gas and oxygen. It ought therefore to be one of the principles of the other acids, the oxides, alkalis, and earths. This is the *causticum* of Meyer.

12. The new theory has therefore only the merit of having confirmed the opinion of Jean Rey, and Hales, who had proved that in combustion, and in the calcination of metals, there was a combination of a portion of air which augmented the weight of the burnt body; a truth to which too little attention has since been paid. But it was wrong to reject the principle of inflammability which exists in combustible bodies, as well as the caloric which is combined in the new products. The new nomenclature, besides, is very imperfect.

13. It is probable, therefore, according to the present notions, that an acid, the sulphuric for instance, is composed,

a. Of a base, that of sulphur.
b. A portion of hydrogen.
c. ———— oxygen.
d. ———— caloric combined.
e. ———— water.

The same may be said of the oxides, earths, &c.

There are some exceptions, however: the oxymuriatic, fluoric, and iodic acids do not contain oxygen, but hydrogen.

14. But what are the bases of sulphur, phosphorus, &c.? This is a question in chemistry which remains to be solved.

Chemists have wished to regard all these bases as *simple elements*, or at least indecomposed, because art has not yet decomposed them. But I have constantly maintained that, supposing most of these bases had not yet been decomposed, every fact seems to prove that a great number of these bases, and perhaps the whole, are daily composed: they are produced like the principles said to be *immediate* of vegetables and animals.

a. The fluoric acid is found in the teeth: what is it that furnishes it, if it be not a new product?

b. Phosphorus is very abundant, in animal and vegetable substances, in the form of phosphoric acid. We may say the same of sulphur, charcoal, &c. What is it that furnishes them, if they are not new products?

c. Potash and soda are very abundant in vegetable and animal substances, and in nitre pits, the earth of which has been cleansed by repeated washings from all saline substances. What is it that furnishes them, if they are not new products?

d. The earths, particularly magnesia, so abundant in the lixiviums

viums from nitre pits, and among animals, present the same phænomena.

Now, potash, soda, and the earths are metallic oxides.

e. Metals themselves, such as iron and manganese, are very abundant in vegetables and animals. What is it that furnishes them, particularly manganese, if they are not new products?

15. The production of ammonium by exposing to the action of the Voltaic pile mercury with ammonia, may throw some light on the new production of these bases. The azotic and hydrogen gases seem to be the principal bases of ammonia. These two gases therefore concur, according to Berzelius, in the formation of a metallic substance, *ammonium*.

Caloric, the luminous fluid, and the Galvanic fluid, may also concur not only in the formation of ammonium, but of the other metals, the alkalis, earths, sulphur, phosphorus, carbon, &c.

16. All these fluids themselves are probably formed of *one sole primitive matter*. This was the opinion of the philosophers of antiquity, and it has been sanctioned by Newton, according to Sir H. Davy. This primitive matter is perhaps the *nebulous matter*, or *akasch*. It is probable that chemistry will decompose some of these bases.

17. The Galvanic action has the greatest influence in the principal phænomena of nature: such as fermentation; the inflammation of pyrites, the pyrophori, and the various chemical combinations, compositions, and decompositions; the various phænomena of the animal and vegetable œconomy; heat, irritability, excitability, sensibility; the electrical state of the terrestrial globe, &c.

18. Electricity and Galvanism therefore contributing to chemical combinations, their action is continual and never interrupted. The *electro-chemical* theory of Berzelius merits the whole attention of scientific men.

19. It is no more demonstrated that water is composed of hydrogen and oxygen, as Cavendish says, than that it may be converted into earth as maintained by Newton.

20. The action of the molecules of solid bodies depends rather on their figure than their mass.

M. Delametherie thus concludes his Sketch of the Progress of the Sciences, of which the above is an extract:

"Yet a little while, and these truths will be recognised like those to which I have already alluded; viz. the great quantity of caloric in the inflammable gas, the small quantity of caloric in pure air, the insufficiency of oxygen for producing the acids, the imperfection of the new chemical and mineralogical nomenclatures, the insufficiency of the new chemical theories, and the insufficiency of crystallography for making us acquainted with minerals."

IX. *Me-*

IX. *Memoir upon Palladium and Rhodium.* By M. Vauquelin*.

After detailing the history of the discovery of palladium and rhodium, M. Vauquelin proceeds:

§ I. The following is Dr. Wollaston's process for separating these two metals from the solution of platina: he dissolves in the nitro-muriatic acid two ounces and a half of platina ore, he precipitates the platina by sal ammoniac from a portion of this solution, corresponding to 1000 grains of ore; he plunges in the mother waters and the washings of the salt of platina, united, laminæ of zinc for precipitating the metals in question. But as there is in these mother waters, copper, and sometimes lead, which are precipitated by the zinc at the same time with the rhodium and palladium, M. Wollaston takes them up by means of aqua fortis, which does not attack the other metals.

He dissolves the residue in the nitro-muriatic acid, precipitates once more by sal ammoniac the little platina which is in it, adds to the solution, when thus precipitated, 20 grains of sea-salt, evaporating to dryness, and washes the residue with alcohol until the latter comes off colourless. By these means he dissolves the triple salt of palladium, and leaves that of rhodium.

Although M. Wollaston only operated upon 1000 grains of platina ore, and had at his disposal, of each of the metals, only six or seven grains at most, he nevertheless ascertained their chief properties; which does infinite honour to his sagacity, for it seemed almost incredible.

For my part, I confess that although I employed about sixty marcs (fifteen kilogrammes) of platina ore, I experienced many difficulties in separating exactly the palladium and rhodium from the platina and the other metals which are in this ore, and particularly in obtaining them very pure.

§ II. The first condition favourable to the solution of platina in the ore, and its precipitation, relates to the proportion of the acids which ought to compose the solvent: that which appeared to be the best is one part of nitric acid to two of muriatic acid: this aqua regia so composed, independently of its dissolving a greater quantity of platina, gives a saving in price of nearly cent. per cent., an object which ought not to be overlooked when large quantities of platina are to be dissolved.

The second condition refers to the concentration of the acids: the quantity of nitro-muriatic acid necessary for dissolving the platina, does not decrease merely in the ratio of its concentration,

* *Annales de Chimie*, tome lxxxviii. p. 167.

tion, but in a much larger ratio. Thus, I suppose that in a volume of aqua regia represented by two, there is enough of acid for dissolving a given quantity of platina: this quantity of acid reduced to half its volume, for instance, will then be much more than sufficient for producing the same effect.

Thus an aqua regia composed of two parts of muriatic acid at 22°, and one part of nitric acid at 34°, and which shall mark about 25° in the areometer, will only dissolve about one-eighth of its weight of platina: whereas the aqua regia composed of the same proportions of muriatic acid at 22°, and nitric acid at 44°, and which shall mark 28° and a half in the areometer, will dissolve one-fourth of its weight of platina. It will therefore require one half less of this last to dissolve the same quantity of platina; now one part of this acid costs less than two of the other. There is also this advantage, that the solution takes place much more speedily, and almost without the assistance of any other heat than that which is naturally developed during the operation.

We may easily conceive the reason for what is above set forth, when I observe that nitro-muriatic acid at 18° or 20° only will not attack crude platina at all, and will attack it but very slowly even at a boiling heat: now at this temperature a great part of the acid will be volatilized previous to acting upon the platina. This is the reason why the acid which we collect during the solution of the platina, even when reduced to the consistence of a thick syrup, does not attack the platina again: this is also the reason why we may with weak aqua regia dissolve gold mixed with platina, without the latter being attacked at all.

The third condition has for its object the state in which the solution of platina ought to be put, in order to be more completely precipitated by sal ammonia. It must be sufficiently reduced by evaporation, in order to congeal into a crystalline mass upon cooling; for, if there remains too great a quantity of acid, it retains much more triple salt in solution after muriate of ammonia has been added. In fact, if in a similar solution which precipitates no more by sal ammoniac, we put any alkali in order to saturate a part of the superabundant acid which it contains, we see a new quantity of salt precipitated: we know also that the ammoniacal muriate of platina is dissolved more abundantly in water sharpened with nitro-muriatic acid, than in pure water. I ascertained this fact, by putting into the liquors from which the ammoniacal muriate of platina had been separated, some pieces of iron, in order to precipitate the metals which exist in the ore of platina. The first effect produced was the precipitation of a considerable quantity of triple salt of platina

tina almost pure; but I was puzzled for a few minutes as to the true cause of this precipitation, by the sulphate of iron at the *minimum* producing the same effect with the metallic iron.

This suggested the idea, that in the solution of platina the metal might be in two states of oxidation, and that at the *minimum* was the only one precipitated by the sal ammoniac, while the other remaining in solution was precipitated in its turn by giving to the iron a portion of its oxygen. In order to verify this conjecture, I introduced into a vessel in which I had diluted yellow salt of platina with water, oxy-muriatic acid gas, with the intention of hyper-oxygenating this salt, if it was possible.

The solution took place speedily with a development of heat and change of colour, the liquid becoming red. A considerable quantity of gas was extricated, which I found to be azotic gas. But when I put iron into this solution, no salt was precipitated from it, but metallic platina; the sulphate of iron produced no effect on it; whereas the muriate of ammonia, and ammonia itself, precipitated from it a great quantity of yellow salt.

Thus the oxy-muriatic acid did not alter the state of oxidation of the platina in its triple salt, but decomposed the ammonia from it, and reduced it to the state of simple muriate: it will follow that the sulphate of iron has, like the metallic iron and the alkalis, the property of saturating a portion of the acidity retained by the salt of platina in solution. The sulphate of iron being decomposed by the muriatic acid, it is possible that the sulphuric acid, set free, has not under these circumstances the same solvent powers with the muriatic acid, over the salt of platina.

Lastly, the fourth condition for obtaining the salt of platina pure, consists in diluting in a proper quantity of water the solution of this metal, without which it will be very difficult to wash the precipitate: it will always remain mixed with iron, and other metals which exist in the same solution. It is better that a greater quantity of salt of platina should remain in solution, than that the matter precipitated should be mixed with foreign substances, because we always recover the platina in our ulterior operations. Ten parts of water to one of the highly concentrated solution, appear to me to be the proportion which succeeds best, taking care to employ, in order to precipitate, a solution of sal ammonia saturated cold.

The ammoniacal muriate of platina is not pure, except when it is of a citron-yellow colour, when it does not become brown upon drying, and when it pulverizes easily.

§ III. *Method of separating the Palladium from the Rhodium and other metallic Salts, which are united in the same solution.*—After having precipitated the platina by sal ammoniac from its nitro-muriatic solution, I put into the mother-waters and the washings, united together, pieces of iron, in order to precipitate the various metals which accompany the platina in its ore.

I treated the black precipitate which resulted from it by the nitric and muriatic acids, employed in succession cold. When these two acids ceased to act, I washed the residue and dried it. During this operation there were emitted various very acrid white vapours, which I ascertained, upon heating a portion of this residue in a retort, to be sub-muriate of mercury mixed with muriate of copper: the sublimate also contained globules of mercury, and a black substance which I took for osmium from its smell.

The nitric acid which had been used in washing the precipitate just mentioned, contained abundance of iron, copper, and a small quantity of palladium, although the operation had been performed in the cold way.

The muriatic acid employed after the nitric acid still contained abundance of iron, copper, palladium, and even platina and rhodium. This proves that a part of these three last metals is precipitated in the state of oxide by the iron, otherwise the muriatic acid would not have dissolved them: it seems to prove also that these metals in being precipitated combine with the iron and the copper, and prevent them from being attacked by the nitric acid even when employed in large quantities.

I afterwards treated this precipitate by the nitro-muriatic acid compounded with the common acids used in commerce.

Part of the matter only was dissolved, and there remained a great quantity, over which the acid seemed to have no action, although I had employed at least twelve parts.

Presuming that this acid was too weak to act with efficacy on the substance in question, I made a highly concentrated nitro-muriatic acid, of which I applied six parts to one of this residue.

On this occasion, the action was violent, and the solution was very abundant even in the cold way, which was announced by a brisk effervescence, a development of abundance of nitrous vapours, an elevation of temperature, and the colouring of the liquor. All the matter however was not yet dissolved; there remaining at least one-sixth, although the mixture had been long heated.

Lastly, I once more poured upon this substance four parts of the same nitro-muriatic acid: there was still a solution, but in less abundance than in the foregoing operation, the proportion being considered: I shall leave this residue for the present, but shall return to it ere long.

Although the substance treated as above three times with nitro-muriatic acid had been previously subjected to the successive

cessive action of great quantities of nitric and muriatic acids, each of the above three solutions in the nitro-muriatic acid still contained copper and iron; which proves that these metals in precipitating are united to the platina, and to the other metals mixed with it; and that it is by this union that they are protected from the action of the simple acids, and even from the first portions of nitro-muriatic acid.

I had already observed these phænomena in a previous operation; but as I had calcined the precipitate pretty strongly in order to agglutinate its parts, I thought that this had operated between the metals a commencement of union, which had formed an obstacle to the solution of the iron and copper. But the above result announces that this combination takes place at the moment of precipitation. One thing not less remarkable is the great quantity of iron which is precipitated in the state of oxide, with a portion of platina, palladium, and rhodium, probably in the same state.

The three solutions of the precipitate were joined, and evaporated into a syrupy consistence, in order to drive off the excess of acid: afterwards the liquor, sufficiently diluted with water, was precipitated by the muriate of ammonia, when a salt of platina of a pure yellow colour was obtained.

The liquor once more evaporated, almost to dryness, and the residue taken up by water, left a salt of a red colour, not very soluble in water, and which was still mostly formed of platina. We shall account for the colour of this salt by and by.

The following is the process which I adopt for separating the palladium from the rhodium, and the latter from the iron and copper with which it is always mixed.

I dilute with water the above liquor deprived of the greater part of the platina: I add muriatic acid when there is not enough: afterwards I gradually pour in ammonia, so as not to saturate entirely the excess of acid; I shake it; and instantly a great quantity of salt makes its appearance in the form of fine sparkling needles, and of a beautiful red colour.

I allow it to deposit: I try a portion of the clear liquor with some drops of ammonia: if it still yields salt as it did at first, I add fresh quantities of this alkali until it ceases to furnish any.

Then I allow the salt to deposit; I decant the liquor, and wash the precipitate, first with cold water and afterwards with hot water, which is not injurious, for it is of very difficult solution. If by accident I have exceeded the proper degree of precipitation, and there are deposited with the red salt some particles of iron or of rhodium, it may be easily cleared of it by digesting it a few minutes in water slightly acidified with mu-

 riatic

riatic acid: this salt is an ammoniacal muriate of palladium at the *minimum* of acid, the properties of which will be presently explained: in order to obtain the metal, it is sufficient to make it red hot.

Now, in order to obtain the rhodium, I evaporate sufficiently the liquors from which the palladium has been separated, in order that they may crystallize in a mass upon cooling: I allow the water to drain off the crystals, which are sometimes of two forms and two colours; some are in hexahedral laminæ, and of a fine ruby red; and the others less numerous, in square prisms of a yellowish green, which are the ammoniacal muriate of palladium. The water which comes from these crystals is also of a greenish-yellow colour, owing to the copper and the iron.

The crystals being drained, I pound them in a glass or porcelain mortar, and wash them with alcohol at 36°, which I leave on them for four-and-twenty hours, in a close flask, frequently shaking them. When the colour of the alcohol (a greenish yellow) ceases to heighten, it is decanted off, and fresh quantities are applied until the last portions are no longer coloured, or at least until they no longer present to the re-agents traces of copper or iron.

If some portions of palladium still continue with the rhodium, they will be dissolved in the last washings with the alcohol, and then the latter will give by spontaneous evaporation crystals in long square prisms of a greenish-yellow colour. Among these sometimes we may also remark some very minute crystals of a ruby red, which are ammoniacal muriate of rhodium dissolved by alcohol. The salt of rhodium is afterwards dried in the air: it is of a beautiful red; but as it might still contain some small portions of triple salt of platina, it may be dissolved in a small quantity of water sharpened with a little muriatic acid. The salt of platina, if there be any, will remain at the bottom of the liquor, and it may be separated by decantation or filtration. In order to procure the metallic rhodium, it is then sufficient to evaporate to dryness the solution of its triple salt, and to make it red hot in an earthen crucible: it yields a white metal breaking into a spongy mass, the properties of which will presently be shown.

The above process, which is simpler and more precise than Wollaston's, is founded, as we see, 1st, On the insolubility of the ammoniacal muriate of palladium, even in water slightly acid. 2dly, On the solubility in alcohol of the muriates of copper and iron, and the insolubility of the ammoniacal muriate of rhodium in the same agent.

§ IV. *Properties of the neutral ammoniacal Muriate of Palladium, or Submuriate of Palladium.* — This salt is of a very agreeable

agreeable red colour. In this respect it deserves the name of rhodium rather than palladium. When we examine this salt in the mass, it is formed of flexible brilliant and slender needles; when taken in the mass, they are spongy and soft to the touch.

Reduction. Twenty grammes of this salt dry, and made red hot in a forge in an earthen crucible, furnished eight grammes or 40 per cent. of metal of a dead silver white, the particles of which were united, but incompletely fused. This metal was perfectly ductile and malleable.

In order to observe more closely what passes during the decomposition by fire of this salt of palladium, I heated some by the blow-pipe, and I saw that it fuses, diminishes greatly in volume, and sends out vapours of muriate of ammonia and oxy-muriatic acid. It is the fusion and diminution in volume of this salt when heated, which, bringing more closely together the particles of the metal, render it susceptible of being laminated.

Solubility. The red salt of palladium is scarcely soluble in water; it merely communicates to it after a long contact a slight yellow tinge. It is easily dissolved in weak muriatic acid cold; but at a boiling heat it dissolves completely: the solution is of a yellowish brown.

The excess of muriatic acid in this solution being saturated with ammonia, the salt is precipitated with its red colour and all its other properties: if we add an excess of alkali, the liquor assumes a weak yellow colour.

If, instead of ammonia, potash be employed to saturate the excess of muriatic acid, the salt is precipitated in yellow flakes; but by adding ammonia afterwards they become still more yellow; which proves that they are combined once more with the ammonia in the state of a triple salt.

§ V. *Some Properties of Palladium.*—This metal has some resemblance to platina, in its colour, malleability, hardness and fusibility.

The fire of our furnaces does not fuse it completely: I could fuse it but imperfectly on a piece of lighted charcoal acted upon by a jet of oxygen gas*.

I have remarked that, when it is once fused, if we continue to heat it in the same way, it boils and burns away in very brilliant sparks. A portion of the metal which has escaped from the combustion is condensed on the surface of the piece of coal, in the form of very small grains.

Platina when fused in the same manner does not burn like palladium; which proves that the latter is more volatile and

* M. Chenevix says that palladium melts in a common furnace: it is probable, however, that what he subjected to this proof was not perfectly pure.

more combustible. I found the specific gravity of the laminated palladium to be twelve and a very small fraction.

§ VI. *Solution of Palladium by the nitro-muriatic Acid.*—One gramme of palladium put into six grammes of nitro-muriatic acid composed of equal parts was speedily attacked, even in the cold way, which was announced by the reddish-brown colour assumed by the liquor. At first, no nitrous gas was developed; but, by means of heat, plenty came over; which indicates that palladium requires a great quantity of oxygen for its solution.

The solution of palladium is of a reddish brown, which is deeper in proportion to the quantity of acid which it contains; for, in proportion as the latter evaporates, the colour diminishes, and the solution gives a salt of a fawn colour almost insoluble, and which merely makes the water yellow; but this solution becomes reddish brown on the addition of some drops of muriatic acid.

The neutral muriate of palladium is not very soluble in water; it is dissolved, on the contrary, abundantly when the water is sharpened with a little muriatic acid. This solution is not regularly crystallized.

The muriate of ammonia in liquor, put into the acid solution of palladium, forms no precipitate in it; but if the liquors are sufficiently thick, there is immediately formed a great quantity of crystals in needles of a greenish-yellow colour and very singular. When the crystallization takes place slowly, this salt presents the form of quadrilateral prisms or elongated hexagons: these are the ammoniacal muriate of palladium. If to the solution of this triple salt we add some drops of ammonia in order to saturate the excess of acid, a very brilliant crystalline red precipitate is formed, which is the ammoniacal submuriate of palladium above alluded to.

The solution of potash turns the colour of the muriate of palladium brown, and separates from it red and brilliant flakes, as if they were formed of laminæ: the liquor remains coloured, notwithstanding the excess of alkali; but heat affects the precipitation of the oxide of palladium, and the liquor loses its colour entirely: the alkaline carbonates produce the same effect.

The infusion of gall-nuts produces no change in the solution of palladium; the addition of some drops of ammonia to this mixture determines a greenish precipitate: the liquor continues of a yellow colour; which announces an action of ammonia on this combination. The muriate of tin at the *minimum* forms in the solution of palladium a black precipitate. The green sulphate of iron precipitates it in the metallic state.

The oxide of palladium, on being separated from the acid by means of potash, retains much water, at least it occupies a great

great deal of room: in this state it is of a reddish-brown colour: on drying it loses its volume, and takes on a very brilliant black colour.

The muriate of palladium does not form, therefore, triple salt with potash like the muriate of platina. The oxide of palladium, when well washed and dried as well as possible by a gentle heat, loses 20 per cent. and becomes metallic: it contains therefore abundance of oxygen.

§ VII. *Sulphuration of Palladium.*—One hundred parts of the triple red salt of palladium, when heated with as much sulphur, in a well covered crucible, furnished 52 parts of a blueish white sulphuret very hard, and presenting brilliant laminæ in its fracture.

Being aware, from anterior experiments, that 100 parts of this triple salt contain from 40 to 42 of metal, it was easy to know the quantity of sulphur which is combined with it in the above operation.

In fact, if 42 parts of metal (the result which I regard as the most probable) absorb ten of sulphur, it is evident that 100 parts of palladium will absorb about twenty-four in order to be sulphuretted completely.

The sulphuret of palladium, when put into a cupel, is perfectly fused at the heat at which assays of silver are made: when a certain quantity of sulphur was dissipated, the metal was fixed, increased in volume, and became rough on the surface. When all the sulphur was separated, the palladium was of a silver white; it was malleable under the hammer, and might afterwards pass through the rolling-press without being torn.

Sometimes the palladium obtained by the roasting of its sulphuret, presents at its surface spots of a blueish-green colour, which appear to be owing to a commencement of oxidation; for they disappear in the muriatic acid, and the latter becomes of a reddish colour. The same thing happens to it, when, by decomposing its triple salt, a sufficient degree of heat is not given to set the oxygen entirely free. The platina is not even united to the sulphur.

§ VIII. Rhodium. *Examination of the ammoniacal Muriate of Rhodium.*—After having separated the palladium, and evaporated the liquor containing the muriate of rhodium, in order to obtain this crystallized salt, the mother-water was decanted, the crystals drained, and washed several times with alcohol as above described. This liquid dissolves the green matter which exists among the crystals, and these last assume a lively ruby-red colour. This salt, in order to be redissolved, requires much more water than before, because the alcohol has taken

taken from it an excess of acid favourable to its solution and crystallization.

The solution of the muriate of rhodium separated from the copper and from the iron, and from the traces of palladium which might still exist in it, forms with ammonia a precipitate of a fawn-colour and nearly insoluble, which is an ammoniacal submuriate of rhodium, as evinced by the vapours of sal ammonia which it gives on being heated *. The liquor from which this submuriate has been separated preserves a deep fawn colour, notwithstanding the excess of ammonia; but upon heating it even without ebullition, it furnishes a new quantity of flaky precipitate of a clearer yellow than the first. Lastly, the liquor evaporated to dryness, and its residue taken up with water, still left a small quantity of matter similar to the foregoing. All these precipitates were ammoniacal submuriate of rhodium; which proves that this salt is a little soluble in water, and that an excess of ammonia seems to favour this solubility.

Solubility.—The ammoniacal muriate of rhodium is dissolved very easily in cold water; its solution is of a purple red colour, nearly like that of cochineal or recent strawberry juice: but this colour becomes brown upon the application of heat, and even spontaneously after some time.

This salt is in grains, crystalline, and very brilliant: it is not soft to the touch like the ammoniacal submuriate of palladium.

§ IX. *Decomposition of the ammoniacal Muriate of Rhodium by Potash.*—Two grammes of this salt dissolved in water, having been mixed with a solution of potash, produced a red precipitate, and emitted ammonia. In a few minutes the quantity of precipitate was diminished, and the supernatant liquor assumed a greenish-yellow colour. By the aid of heat, the precipitate which I have mentioned was speedily dissolved, the liquor became deeper in colour, and plenty of ammonia was given off: there was an excess of potash in the liquor.

The solution of muriate of rhodium in potash exposed to the air for some days produced yellow crystals: this is probably a triple salt of muriate of rhodium at the *minimum* of acid, and muriate of potash dissolved by an excess of alkali: at least it seems to be proved by this; viz. that by the excess of potash saturated by the muriatic acid, a yellowish white precipitate insoluble in water is formed: this is neutral muriate of potash and rhodium.

§ X. *Reduction of the ammoniacal Muriate of Rhodium.*—

* This name suits it better, as this salt furnishes 50 per cent. of metal on the application of heat, whereas before having been precipitated by ammonia it gives only 28.

Fifteen

Fifteen grammes of this salt made red hot in an earthen crucible furnished four grammes and two-tenths of white metal not fused, but all the parts of which were agglutinated and formed only one mass. In another experiment ten grammes of the same salt gave three weak grammes of metal: from these results it is evident that this salt contains between 28 and 29 per cent. of metal.

Being anxious to know if this metal could be united with sulphur, and, in this case, how much it required to pass to the state of a complete sulphuret, I mixed four grammes of the above salt with as much sulphur, and I heated this mixture for some minutes in a furnace. I obtained a well fused metal of a blueish-white colour, weighing one gramme $\frac{400}{1000}$. This result proves that the rhodium is combined with the sulphur; without which it would not have melted, and would have only yielded, according to the proportion above established, one gramme $\frac{12}{100}$ of metal; it had therefore acquired $\frac{300}{1000}$ of sulphur, which is in the ratio of 26 to $\frac{0}{0}$ of metal.

The sulphuret of rhodium, when subjected to a strong heat in contact with the air, exhales sulphurous acid, and exhibits considerable arborisations. After this operation, it is white, spongy, and brittle, and does not weigh more than one gramme $\frac{00}{1000}$.

Fusibility.—Rhodium of all the known metals seems to be the most infusible; in fact, one semigramme of this metal, coming from its submuriate, decomposed at a middling heat, and which had on that account a blackish colour, heated for a long time on a piece of charcoal upon which a stream of oxygen gas played, was not melted: its parts were merely agglutinated into a single mass, which was of a silvery-white colour. I repeated this operation several times on still smaller quantities, without fusing them completely. This metal, although fragile, is therefore more difficult to fuse than the pure platina and palladium, which melt very speedily in a fire kept up by oxygen gas. Hence it was impossible to determine the specific gravity of this metal.

We may say therefore that pure rhodium is a metal of a white colour, very little different from that of palladium, which is brittle, and more difficult to fuse than all the other metals.

§ XI. *Solubility of Rhodium in the Acids.*—One gramme of rhodium in fine powder subjected to the action of eight grammes of nitro-muriatic acid, in equal parts, was not sensibly attacked, and scarcely was the acid coloured. I then treated it with a very strong acid, but it was not dissolved. Since rhodium when it is pure is absolutely insoluble in the acids, we must suppose that it is alloyed in the ore of platina with other metals, which

which favour its solution when we treat the crude platina with the nitro-muriatic acid. For the same reason, it was impossible to study the properties of the simple salts of this metal.

I shall give in a subsequent memoir the method of separating the osmium and iridium from the foreign bodies which form the insoluble residue of the ore of platina, and I shall explain in the same way the properties which these two metals shall exhibit on examination.

X. *On the Influence of the Pressure of the Atmosphere on the Crystallization of Salts.* *By* Gay Lussac*.

Several chemists have laid it down as a principle, that we increase the solvent power of water by diminishing atmospherical pressure. This result is not however well ascertained; for we are acquainted with only one substance, the sulphate of soda, the aqueous solution of which does not crystallize in vacuo, although it gives abundance of crystals in the open air; and besides, the circumstances under which this phænomenon takes place have not been accurately noted. I propose therefore to examine, 1. The causes which concur in the crystallization of the sulphate of soda when we vary the pressure of the atmosphere; and, 2. What other substances which are soluble in water, act in the same manner.

In order to make the experiments which I am about to describe, a common barometrical tube may be used, into which the saline solutions are to be introduced when warm: this may be filled about three-fourths, and in order to form a vacuum, the solution must be boiled. When it is presumed that the vapour has completely displaced the air, the open end of the tube must be hermetically sealed, by exposing it to the blow-pipe, or by dipping it in melted sealing-wax. The vacuum obtained in this way may be regarded as perfect nearly to the degree of the elastic force of the vapour of the saline solution.

The temperature of the air being from 12° to 18°, I inclosed a solution of sulphate of soda at the boiling temperature, in a small tube as above described. After cooling, no crystal was formed, although it was shaken frequently. Having broken the extremity of the point of the tube to give access to the air, the liquid instantly became solid, with a very perceptible disengagement of heat. It happens sometimes, however, that the entrance of the air does not determine the crystallization; but if we then introduce a small crystal into the solution, or if we shake it, it begins to crystallize immediately. This circum-

* *Mémoires d'Arcueil,* tome iii. October 1813.

stance

stance proves that the solvent power of water does not depend solely on atmospherical pressure.

I poured mercury into five barometrical tubes to within three finger breadths of the top, taking care to remove all the small particles which stuck upon the sides, and I filled them up with a boiling and saturated solution of sulphate of soda: having instantly inverted them over a bath of mercury, the solution crystallized in each tube in proportion as it rose in its upper part.

I repeated this experiment upon five other tubes in which mercury had been boiled, but the solution did not crystallize in any. I introduced a small bubble of air occupying about the two hundredth part of the tube, and by shaking, or frequently even without this precaution, crystallization took place promptly. The same effect may be produced by using hydrogen, carbonic acid or nitrous gas instead of air. It would seem therefore that a very small quantity of any gas is sufficient to determine crystallization.

I introduced a concentrated and boiling solution of sulphate of soda into five barometrical tubes in which mercury had been boiled. Twenty-four hours afterwards no crystals were observed in any tube, although they had been shaken considerably in the interval. Nevertheless, after some violent concussions produced by suddenly dipping the tubes in the mercurial bath, I succeeded in crystallizing the salt in three of them, in a few minutes. It was always in the upper part of the tube, where the shaking had collected some very small air-bells, that crystallization commenced. The solution in the two other tubes not having undergone any change under the same circumstances, I operated the crystallization speedily by introducing into one a crystal of sulphate of soda, and into the other an air-bell. I ought to observe that I never succeeded in crystallizing the solution by shaking it in a small apparatus; but I ascribe this failure to my not being able to produce an agitation so violent as in the barometrical tubes. I did not succeed any better on vibrating the tubes by means of the bow of a violin, after having secured one of their extremities in a vice.

It seems from these experiments, that simple shaking does not produce crystallization unless there is air in the apparatus; but on the one hand I produced it by shaking barometrical tubes freed from air with the greatest care; and on the other hand, this precaution was almost always insufficient for the small apparatus, in which I found, on opening them under water, a volume of air sometimes amounting to one-thirtieth of the empty space in the tube. Besides, is it not very remarkable that the steam of water which is developed in the apparatus, and the tension of which

which is frequently equal to two centimetres of mercury, does not produce any effect, while a very small air-bell favours crystallization so powerfully? In short, it is not less evident already, that the solvent power of water does not increase as the pressure of the atmosphere diminishes; since a very small quantity of any gas produces crystallization: but I am about to show that it is entirely independent of it.

If we introduce a crystal of sulphate of soda into a hypersaturated solution contained in a barometrical tube, crystallization generally commences, instantly extending speedily through the whole mass, and the solution is afterwards at the same degree of saturation which it would have acquired in the air at the same temperature. Now I am convinced that crystals of a salt introduced into its solution, well saturated at a given temperature, lower it but infinitely little under its true point of saturation: consequently it is clear beyond dispute, that the solvent power of water does not depend on the pressure of the atmosphere. We may add, that if we put into a tube a little salt with its boiling and saturated solution, and afterwards make a vacuum as described, a perceptible quantity of salt will not be dissolved on applying heat.

We shall see presently that the crystallization of a solution of sulphate of soda may be prevented by allowing the latter to cool in vacuo; but we obtain the same result also by allowing it to cool, exposed to atmospherical pressure, provided that we cover the surface with a little spirits of turpentine. This method, which is also very efficacious for retarding the freezing of water, is very easily executed. Take a glass tube from one to two centimetres in diameter, closed at one end: pour in a boiling and saturated solution of sulphate of soda, and cover it instantly with spirit of turpentine. The solution will crystallize, but rarely by cooling and even by agitation: a current of electric fluid, or two platina wires communicating with the poles of a pile, will produce no effect. But a crystal or a glass ring dropped into it, will immediately bring on crystallization: an iron ring will do the same, but not so well as a glass one.

If by diminishing the pressure of the atmosphere the solvent power of water was increased, it would follow that, by augmenting the pressure at the surface of a saturated solution of sulphate of soda, the precipitation of a part of the salt would be determined; but this is not the case. I took a bent tube, the large branch of which was two metres long; and having introduced into the shortest a solution of sulphate of soda, saturated at the temperature of the air, I loaded it with a column of mercury two metres high, without any crystals being deposited even after several days.

As

As it cannot be doubted that the air acts in some manner or other upon the crystallization of the sulphate of soda, since the latter takes place almost constantly in the open air, whereas in vacuo it takes place under certain circumstances only; I have supposed, in order to account for this phænomenon, that water having the property of dissolving air, and having lost that which it held in solution by the heat which I had employed to dissolve the salt, it might have been possible, when it was cooled, for it to take up that which it had lost by heat, and that the absorption of this air had precipitated part of the sulphate of soda, for the same reason that one salt precipitates another from its solution.

In order to verify this conjecture, I introduced into a flask filled with water deprived of air by boiling it, and inverted over mercury, an air-bell occupying the fourteen hundredth part of the capacity of the vessel. In twelve hours the air-bell had diminished but little, and in four days it was not entirely absorbed. An absorption so slow cannot account for the crystallization of the sulphate of soda, when to a concentrated solution of this salt in vacuo we add a little air. I shall remark however, that since a crystal determines the crystallization, it may be possible that the absorption of the air, however small at first, produces the precipitation of a little salt, and that afterwards the crystallization might continue the same.

However the case may be, it seems probable that we may describe the property which the sulphate of soda also has of not crystallizing under certain circumstances, to the figure and arrangement of its molecules, which may be such that they strongly resist any change of state.

This property does not in my opinion differ in any way from that possessed by water of maintaining its fluidity below its true freezing point, under circumstances nearly similar, nor from the properties of several saline solutions, of remaining sometimes saturated, and of crystallizing as soon as we shake them, or introduce a foreign body. But supposing the figure of the molecules to be the chief cause of the foregoing phænomena, it is nevertheless difficult to conceive how the presence of the air acts in disturbing their equilibrium and favouring their junction.

I think I have demonstrated that the solvent power of water is totally independent of the pressure of the atmosphere; but if there remain any doubts, they would be soon removed by showing that there are very few saline solutions which possess the property of remaining hyper-saturated under certain circumstances. A solution of phosphate of soda saturated at the temperature of 70° did not crystallize upon being cooled in a barometrical tube, even after being slightly shaken: an air-bell did

did not produce crystallization; but after the introduction of a new quantity, the solution congealed in one mass. If we take it saturated at the degree of its ebullition, it will crystallize almost constantly in vacuo, as well as in the air.

A solution of alum saturated at the temperature of 40° did not crystallize in two small tubes, one of which was kept open: a slight agitation determined the crystallization in both.

Nitre in solution, weak or concentrated, crystallized constantly in the same way in vacuo, and in the open air: the case was the same with the solutions of barytes and of strontian slightly super-saturated; with the oxalic acid, the muriates of soda and ammonia, the nitrate of lead, and the sulphate of potash. It is to be remarked, that the saline solutions which crystallize with most reluctance in vacuo are precisely those which sometimes remain super-saturated in the air. We see from this, that the fact from which we set out, in order to establish as a principle, that the solvent power of water depends on the pressure of the atmosphere, is not general; but even supposing that this was the case, it would not be the less clearly demonstrated by the experiments which I have related, that the solvent power of water is totally independent of any pressure exercised on its surface.

XI. *Notices respecting New Books.*

THE First Part of the Philosophical Transactions for 1814 has appeared; the following are its contents:

1. A Synoptic Scale of Chemical Equivalents. By William Hyde Wollaston, M.D. Sec. R.S.—2. Methods of clearing Equations of quadratic, cubic, quadrato-cubic, and higher Surds. By William Allman, M.D. Communicated by the Right Hon. Sir Joseph Banks, K.B. P.R.S.—3. Analysis of a new Species of Copper Ore. By Thomas Thomson, M.D. F.R.S. L. and E.—4. The Bakerian Lecture: on some new Electro-chemical Phænomena. By William Thomas Brande, Esq. F.R.S. Prof. Chem. R.I.—5. An Account of some new Experiments on the fluoric Compounds; with some Observations on other Objects of Chemical Inquiry. By Sir H. Davy, LL.D. F.R.S. V.P.R.I. —6. Some Experiments and Observations on a new Substance which becomes a violet-coloured Gas by Heat. By Sir Humphry Davy, LL.D. F.R.S. V.P.R.I.—7. An Account of a Family having Hands and Feet with supernumerary Fingers and Toes. By Anthony Carlisle, Esq. F.R.S. In a Letter addressed to the Right Hon. Sir Joseph Banks, Bart. K.B. P.R.S.—8. Experiments and Observations on the Influence of the Nerves of the eighth

eighth Pair on the Secretions of the Stomach. By B. C. Brodie, Esq. F.R.S. Communicated by the Society for the Promotion of Animal Chemistry.—9. On a fossil human Skeleton from Guadaloupe. By Charles Konig, Esq. F.R.S. In a Letter addressed to the Right Hon. Sir Joseph Banks, Bart. K.B. P.R.S. —10. A new Method of deducing a first Approximation to the Orbit of a Comet from three Geocentric Observations. By James Ivory, A.M. Communicated by Henry Brougham, Esq. F.R.S.—11. On the Affections of Light transmitted through crystallized Bodies. By David Brewster, LL.D. F.R.S. Edin. and F.S.A. Edin. In a Letter to Sir Humphry Davy, LL.D. F.R.S. —12. On the Polarisation of Light by oblique Transmission through all Bodies, whether crystallized or uncrystallized. By David Brewster, LL.D. F.R.S. Edin. and F.S.A. Edin. In a Letter addressed to Taylor Combe, Esq. Sec. R.S.—13. Further Experiments on the Light of the Cassegrainian Telescope compared with that of the Gregorian. By Captain Henry Kater, Brigade Major. In a Letter addressed to the Right Hon. Sir Joseph Banks, Bart. K.B. P.R.S.—14. Astrononomical Observations relating to the sidereal Part of the Heavens, and its Connection with the nebulous Part; arranged for the Purpose of a critical Examination. By William Herschel, LL.D. F.R.S.

XII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 30. THE Right Hon. President in the Chair. Sir Everard Home furnished an Account of his various Experiments on Rabbits, &c. tending to elucidate the influence of the nerves on the secretions. The results, so far as could be ascertained from the action of living matter after having sustained a mechanical injury, were decidedly in favour of the opinion, that all secretions are effected by nervous influence.

Smithson Tenant, Esq. described A new Method of Double Distillation by means of Steam, whereby double the quantity of fresh-water may be raised from salt-water in a ship's kettle, that has hitherto been obtained. His process was illustrated by a drawing, showing how steam can be made to pass through water, and thus contribute to evaporize it, so that an additional quantity of fresh-water may be procured in the same time and with the same apparatus from sea-water at sea.

An Account of a Series of Observations on the Formation of human Bones, made by Mr. Housham, was communicated by J. Heaviside, Esq. The author having favourable opportunities

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for observing the origin and progress of the growth of bones, began with a child or fœtus only seven weeks old, and continued his observations at different times for several years. He inspected the process by means of a powerful microscope, from which he observed that the arteries secrete a mucilage which forms cartilage; that the cartilage shortly becomes tubular, with numerous spiculæ around it containing phosphate of lime; that in the course of time bony matter is completely deposited; and that, finally, the laminæ appear. Mr. H. described with great minuteness the various forms which the matter assumed during the entire process of ossification, and also the structure of the most perfect bones, their nerves, blood-vessels, membranes, periosteum, &c. Chemists have long known that if a piece of bone be digested in dilute muriatic or nitric acid, the gelatine and earthy matter are dissolved, and there remains only a firm cartilaginous substance retaining the figure of the bone: hence it was known that cartilage is a constituent of bone. Mr. H. has now proved that it is the nidus or rudiments of bone.

A paper was read on the Triple Salts called Prussiates. The author derived his facts chiefly from Proust's experiments, and after showing that these salts contain no prussic acid, proposed to designate them by a more correct appellation, according as they were the result, with a particular base, of the peculiar compound which he denominated *carhyoxic* acid, taking the first syllable of the names of its elementary principles, carbon, hydrogen, and oxygen, to which he added the termination *ic*.

The Society then adjourned till Thursday the 10th of November next.

PROCEEDINGS OF THE FRENCH INSTITUTE FOR THE YEAR 1813. BY M. CUVIER.

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

After detailing the discovery of iode by M. Courtois, and the subsequent experiments of Messrs. Clement and Desormes and Sir Humphry Davy, with which the readers of the Philosophical Magazine are already acquainted, the report proceeds:

Another manufacturer like M. Courtois, enlightened by chemical science, has made an observation which may become important to the arts. M. Tassaert having for some time constructed the floors of his soda furnaces with stone, he observed on taking them down a blue substance which is not to be found when bricks are used, and in which M. Vauquelin found almost all the principles and all the properties of ultramarine: it is to be hoped therefore that, by following up this indication, we may some day or other succeed in imitating nature in the formation of of this valuable colour. M. Pelletan jun. has remarked on this subject, that there is manifested, in the manufacture of soda, a blue more or less intense, which calcination does not destroy, and that this colour appears chiefly when iron is found in contact with soda not yet entirely freed from sulphuric acid.

Crude platina, as it is brought from Peru, is a body very much compounded. Besides pure platina, a noble metal, heavier than and equally unalterable with gold, it contains iron, copper and mercury; and the successive researches of Messrs. Wollaston, Tenant, Descostils, Fourcroy, and Vauquelin, also demonstrated ten years ago the presence of four metals, distinct from all those with which we were formerly acquainted: these have been named palladium, rhodium, osmium, and iridium.

M. Vauquelin has this year resumed the investigation of these substances, having read a memoir on the most convenient methods of obtaining palladium and rhodium in a state of purity*.

While M. Vauquelin was thus employed, M. Laugier, his colleague in the Museum of Natural History, was occupied with osmium, perhaps the most curious of all the metals found in crude platina, and the oxide of which is volatilized at the heat of boiling water, gives no colour to distilled water, but exhales a pungent odour, and acts so strongly upon the olfactory nerve as to affect the sense of smelling for several days. These properties, and others not less singular, made it a subject of regret with chemists that it was so difficult to obtain this metal in a considerable quantity; but M. Laugier has satisfied them to a certain extent. When platina is dissolved in the nitro-muriatic acid, there remains a black powder composed of iridium and osmium, and hitherto this powder alone had furnished osmium to the chemists: but M. Laugier, having perceived that the acid, which was used for dissolving the platina, and which is again separated from it by distillation, exhaled a strong smell of osmium, supposed that it contained this metal; and in fact he found that, by saturating the acid by caustic alkalis, but particularly by lime, and by distilling the mixture, a solution was obtained with little trouble, containing a considerable quantity of osmium, which formerly was entirely lost.

In our report for 1808 we mentioned the fortunate trials which had been made in the mines in the environs of Liege, to obtain on a large scale zinc in a malleable state, and we alluded to the advantage which might flow from our being able to substitute zinc for lead in roofing houses. It was also proposed to substitute it for tinned copper, tinned iron, and pewter used in kitchen and other utensils in daily use. But the Ministers of the

* See a preceding article in the present Number.

Interior and of War having consulted the Institute on this subject, the Committees of Chemistry and Medicine found that zinc is too soluble by the weakest acids, by grease, and even by pure water; and that the salts which it forms are very acrid, and in certain cases move the bowels too much to admit of this metal being used without danger. M. Sage also individually made experiments, which convinced him that distilled water when kept in zinc utensils assumed a very decided styptic taste; and that the juice of fruits cooked in these vessels dissolves a portion of them, and forms salts disagreeable to the taste: this is the more vexatious, because the mines in question contain no arsenic as others do, and in this respect there is nothing to apprehend. Of this M. Sage has furnished a new proof by a series of experiments which he laid before the Class.

Messrs. Vauquelin and Thenard have given an analysis of the mineral water of Provinz, from which it results that one litre contains

Carbonate of lime	0·554
Oxide of iron	0·076
Magnesia	0·035
Manganese	0·017
Silex	0·026
Sea salt	0·042
Carbonic acid 27 inches 8-10ths	

and an inappreciable quantity of muriate of lime and of a fatty substance; but the sulphuric acid, as has been supposed, does not exist in it at all.

M. Thenard has published the first volume of an elementary Treatise on Chemistry, in which this science, which daily makes such rapid progress, is exhibited under its present aspect. The author arranges the facts according to the degree of simplicity of the bodies to which they belong. After having spoken of imponderable agents, he treats of oxygen, and of the theory of combustion; and passes afterwards to combustible bodies, their combinations with each other, and those which they contract one by one with oxygen. These last are divided, according to their properties, into oxides and acids, and the fluoric and muriatic acids are arranged according to the ordinary ideas, which refer them to oxygenated bodies. At this point ends the first part of a work which the rapid progress of science has rendered necessary so soon after other excellent works on the same subject, and of which we cannot refrain from wishing to see the speedy termination by M. Thenard.

The method of positive and direct observation becomes daily more prevalent in geology, and more precise notions are hourly acquired as to the soils of various countries, the general laws of their

their stratification, and the organised bodies of which they contain the remains.

The stony beds which contain fresh-water shells only, so many of which Messrs. Cuvier and Brongniart have discovered in the environs of Paris, and which Messrs. Brongniart, Omalius de Halloy, Marcel de Serres, Daudebart de Férussac, &c. have recognised in other countries, have particularly excited the attention of naturalists, who have set about distinguishing the shells of fresh from those of salt water. Messrs. Marcel de Serres and Férussac have each given in a memoir upon this subject. The species alone, according to the former, may be offered in proof, and not the genera; for most of the genera have both fluviatile and marine species: the varieties even are not unworthy of notice; for the same species, according to the observations of the author, sometimes changes its form, so as to deceive those who have not observed the different transitions; and the difficulty increases when it is required to classify shells in the fossil state, when the epidermis, the hairs, and all the other fugitive characters have disappeared.

There are species, particularly among the *operculeæ*, which live both in salt and fresh water, and which are consequently more abundant at the mouths of rivers: and we may observe among fossils traces of this habitude; for our fresh-water beds contain in certain places a species of the *potumidus*, a genus which is also met with at the mouths of rivers.

M. Marcel de Serres visited on purpose the salt-water pits on the shores of the Mediterranean, in order to examine the shells which they contain: he there observed paludines similar to those which form extensive beds in the environs of Mentz, where several sea shells are also found. A geologist who had confounded these paludines with one of the *bulimi* of our fresh-water strata, concluded that the latter are sea shells as well as the others; but de Serres clears up this mistake, and shows that they are not only species, but different genera.

This observer has examined the limits of these migrations of animals and plants from salt to fresh water, and *vice versâ*. He ascertained that no animal and no plant can resist a brine of eight degrees: he has distinguished, both among animals and plants, the species which adhere to the shores of the sea on account of the sand, and which can exist in other sandy places also; those which are attracted by the salt alone, and which exist very well in salt pits or lakes at a distance from the sea; and, finally, those which require the sea as it is, and do not remove from it.

These observations prove that it is not always easy to decide

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if a shell is a salt- or a fresh-water one; but they do not weaken the fact of there being immense beds where there are only shells well known to be of fresh-water origin; and they even account for our also finding these shells dispersed in beds of marine formation.

M. de Serres places the lignites or bituminized woods among the fossils which are most frequently mixed with terrestrial and fresh-water shells; and this renders it probable that these woods have grown on the spot where they are now found, besides agreeing with all the other facts, which show that the surface of the present globe was dry, and peopled with animals and terrestrial vegetables, previous to the last irruption of the seas.

Two young and expert naturalists, Messrs. Desmarets and Leman, have found in the fresh-water strata of our neighbourhood, even shells of those small *entomostraceæ* which are called *cypris*, and even the seeds of the plant known by the name of *chara*. These seeds had been taken for shells, and the name of *gyrogonites* was given them.

The geological system of the environs of Paris, which formed the chief object of the observations and discoveries of Messrs. Brongniart and Cuvier, is now followed up with great zeal by several learned naturalists. Messrs. de Tristan and Bigot de Morogue have carefully described the districts adjoining the Loire; and M. Omalius de Halloy, mining engineer, in following up these inquiries, and those of our associate M. Desmarets, is occupied in laying down all the boundaries on a chart. The beds of this system deposited upon chalk represent an irregular and curvilinear trapezium, of which the southern side parallel to the Loire runs along this river on the south from Cosme to below Blois; the eastern side passes near the towns of Montargis, Nemours, Montereau, Villenoxe, Sézanne, Epernay, Laon, Crépy, and La Fère; the northern side near the towns of Chauny, Noyon, Compiègne, Clermont, Beaumont, Chaumont, and Gisors; lastly, the western side descends by Mantes, Houdan, Epernon, Auneau, and along the Loire to the vicinity of Vendôme, from which it rejoins the Loire at Blois. All this space is surrounded with chalk; and the chalk, in which M. de Halloy has recognised three very distinct modifications, is surrounded itself, except towards the sea, with a compact calcareous substance more ancient, which forms a great part of Berry, Burgundy, and Lorraine, extending to Vosges, and which reappears beyond the Black Forest in Franconia and Hesse. The formations of the system of Paris send out various ramifications over this chalk, while the agriculture, industry and resources of a district are frequently detailed by the geology of its soil.

soil. M. de Halloy visited all these places on foot, and was not disheartened either by the badness of the weather or of the roads.

M. Brongniart, a corresponding member of the Class, has visited a part of France equally interesting in a geological point of view, viz. the department of La Manche: and M. de Halloy, who followed his steps, confirmed and completed part of his observations. From the description given by M. Brongniart of the rocks of this country, and of their mutual position, it results, that what were denominated as granites properly so called, belonged to that other kind of rock called *syenite* by Werner, and characterized by the amphibole which enters into its composition, as well as from its formation being much more recent than that of the true granite. These systems of La Manche repose on schists and other rocks long posterior in formation to the granite: it even appears that in certain places they have under them calcareous matter containing fragments of organised bodies; a fact which would be analogous to that observed in Norway by M. de Buch, and from which we might conclude that there were also precipitations of crystallized rocks, after manifestation of life in the waters which anciently enveloped the globe.

M. Brongniart, who is occupied with a general Treatise of Geology, has presented the plan according to which he proposes to distribute the rocks, *i. e.* those aggregations of minerals which compose the crust of the present globe. Applying the principles at present recognised by all naturalists, he wishes the bases and the details of the whole of his method to rest on characters taken from the rocks themselves; and he rejects all those which might be taken from their natural position on the globe, which belongs to their history, but not to their systematic division: he separates the rocks, and leaves among the simple minerals the mineral substances which appear to be simple to the naked eye, and the heterogeneousness of which is manifested only by washing and other operations, which, without being regarded as chemical analysis, nevertheless alter the appearance and texture of these substances; such are the schists, argil, &c. Rocks so reduced, or, as M. Brongniart expresses himself, the *mixed rocks*, are subdivided into crystallized and aggregated: the former have their parts in proportions nearly equal, or rather one of these parts prevails over the others: in the former case the genera are established according to the essential substances, *i.e.* those which are constantly there: in the latter case, according to the base, *i. e.* the prevailing substance: and in both cases the number of component substances, and the structure resulting from their mode of union, serve to distinguish the species.

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The aggregate rocks are divided according as the cement which unites them is more or less apparent, and according to the nature of this cement and the grains which it agglutinates.

In this work, so important for serving as the basis for the History properly so called of Rocks, the author has throughout preserved the names given them by M. Haüy in the arrangement which he has made of them in the Museum of Natural History.

M. Brongniart has also represented to the Class the division which he thinks ought to be established between rocks considered with respect to the date of their formation, and to the remains of organized bodies which they contain, and which are the strongest marked indications of those æras. Underneath all the rest are the granitic strata, without organized bodies, and the most ancient with which we are acquainted: the soils which cover them contain but a small quantity of organic remains, and almost all are zoöphytes: a third series, that of the syenitic strata, yields none, as if their production had been suddenly interrupted: in the fourth series shells begin to appear, and chiefly those which are called *cornua Ammonis:* the fifth and sixth classes of strata are characterized by the gryphites and the cerites, which prevail among their shells: lastly, there are strata the distribution of which is so irregular that we cannot classify them in the order of time: these are trapp rocks on the one hand, and on the other those which result from the ejections of volcanos. With all these groups, strata of transport are mixed, productions of violent motions occasioned by revolutions, and which are sufficiently correct indicators of the moment at which each has commenced.

The well known phænomenon of the fall of the leaves in autumn is still the subject of some discussions with respect to its causes, and still gives rise to various observations. M. Carnot, member of the section of mechanics, but whose active mind embraces every department of science, having remarked that certain trees begin to throw off their leaves from top to bottom, and *vice versâ;* M. Palissot de Beauvois, member of the section of botany, has inquired into the cause of this difference. He found that in general the species in which the autumnal shoots consist of simple prolongations of the extremities of the branches, are first stripped at bottom, and that those in which this shoot takes place by small lateral branches, begin to throw off the leaves at top; or, in other words, that the last grown leaves are also the last which fall off. Duhamel, who had made a similar remark, was astonished that those leaves which ought to be most tender esisted the frost longest: this proves that it is not the frost which makes the leaves fall off, but that their fall is a necessary and

coordinate effect of the process of vegetation; and that either by the development of the shoot, or by an interior alteration prepared by nature, the petiole is detached when the progress of its nutrition has brought on the moment when the texture ought to be dissolved which served as its lien. Thus when a tree, from any given cause, perishes in the season of vegetation, its leaves preserve their adherence.

We know that several flowers open and shut at certain hours, and that heat and humidity influence this phænomenon. M. Desvaux, a botanist of Paris, has made some observations in this respect on the *mesembrianthemums*, plants in which these alternate movements are so remarkable, that their generic name has been taken from the circumstance; and he has found that the cause resides not in the corolla, as has been supposed, but in the calyx, which by closing forces the corolla to obey its contractions; so that, if we cut off the calyx, the corolla remains expanded at night as well as by day.

M. de Mirbel, our colleague, has this year presented us with two series of researches; the first on the seed, and on the membranes which cover it; the second on the *pericarp*, i.e. the receptacle in which the seed is lodged. He has inquired in the first place, to what extent the analogy established by Malpighi may be regarded as exact, between the tunicas of the fœtus in the womb, and those which envelop the seed of plants. The embryo composed of the plumule and of the radicle being considered as a fœtus, Malpighi thought he recognised in the *testa*, or external tunic, the representation of the *chorion*, and in the *tegmen*, or interior tunic, that of the *amnios*: the *perisperm* appeared to him to represent the liquor amnii in which the fœtus swims. M. de Mirbel finds, on the contrary, that originally the seed is only a mucilaginous and continuous cellular tissue, one part of which at first becomes the embryo, and the rest afterwards forms the perisperm and seminal tunics, without our being able to say that the embryo at any period floats in a liquid. The mucilaginous state of this tissue, and its transparency, may therefore have suggested the inaccurate comparison made by Malpighi.

M. de Mirbel, passing to the examination of the pericarp, succeeded in referring the forms of it to a general law, which, determining what is essential in this part of the vegetable, reduces almost to nothing the anomalies which it seems to present in certain families. The general type of every pericarpian capsule appeared to him to be represented by a small box flattened on the sides, and composed of two valves the union of which forms two rims or two sutures, one crooked and the other straight, and by which the vessels pass which go to these seeds, either from the body of the plant, from the style, or from the organ which

transmits

transmits their fecundating power. This disposition is evident in the husks of the leguminous plants, such as French beans, peas, &c. We perceive it also very distinctly in the kernels of almonds, cherries, &c. one of the sides of which exhibits a furrow and sometimes a channel which indicates the passage of the vessels. M. de Mirbel gives the name of *camare* to a simple capsule like the above. The plants just mentioned have only one for each flower. When there are several, their seminiferous or vascular sutures are always on the side of the ideal axis of the fruit; and if we represent them as soldered together, they form a single pericarpian box, divided into several compartments, and bearing the seeds along its central axis. It is thus that in one and the same family the *camares* are sometimes distinct, sometimes united according to the genera, as we see in the *ranunculi* and the *rutaceæ*: it is thus also that certain camares which are soldered at first are separated at the æra of maturity, as in the moss rose, euphorbium, the *hura crepitans*, &c.

These ideas being once admitted, we find that pericarps very different at first sight, are nevertheless only slight modifications of a common design: but as it also happens that very remote families have pericarps similar, it is rare that we can draw from this part characters proper for enabling us to group plants well.

This is not the case with the internal structure of the seeds, which differs much in the various groups, and very little in the interior of one and the same group; and this partly induced M. Mirbel to divide the family of the orange trees of M. de Jussieu into four families; viz. the *aurantiaceæ*, already very much circumscribed by M. Correa; the *olacineæ*, which comprehend the *olax*, the *fissila*, the *heisteria* and the *ximenia*; the *theaceæ*, in which are included the tea plant and the *camelia*; and the *ternstromiæ*, in which are included the *ternstromia* and the *fresiera*.

The family of the *olacineæ* does not comprehend the *ximenia ægyptaica*, of which M. Delille has properly made a new genus under the name of *Balanites*. This vegetable, which we do not as yet know how to classify, presented to M. Mirbel a character which is perhaps unique in vegetation. Every botanist knows the glandular body which is placed under the pistil of many flowers, and to which the name of *disk* or *nectary* has been given; it exists in the *balanites* in the form of a purse; the pistil is at first shut up in it entirely, and does not appear; but by growth it separates the sides of the purse, and becomes visible.

M. Henri de Cassini, the son of one of our members, whose botanical labours we noticed last year, impressed with the idea developed by sound naturalists, that a classification, to be correct, ought to be founded on the *ensemble* of the characters, has this year directed his attention to the stamens of the great family

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of plants known by the names of *compositæ*, *syngenesiæ*, and *synantheriæ;* in which he has discovered several peculiarities hitherto unknown. No botanist, for instance, had remarked the articulation which separates the thread in the vicinity of the anthera, a character which M. de Cassini has found to be much more constant than that of the union of the antheræ among themselves. He promises to give us his observations on the corolla, the ovary, the pericarp, and the seed; and as it cannot be doubted that he has paid the same attention to these inquiries as to his former communications, no family of plants will be so well described. Botany may expect much from such a student, if, after having minutely described a family so natural that we may almost regard it as a great genus, he should exercise his sagacity in these equivocal families, whose varied characters render their limits uncertain.

Vegetable physiology, like all the other branches of science, presents abundance of these difficult questions, of which nature does not furnish an evident solution, and which will long continue to be an object of discussion among botanists.

Such, among others, is the question respecting the existence of the sexes in the plants known by the name of the *cryptogamous*. Many botanists, disappointed by the difficulty of discovering their organs, concluded that these vegetables might exist without sexes, and propagate by bulbs or simple buds, as well as certain animals, such as the polypi, whose reproduction certainly takes place in this way. Others, on the contrary, struck with the complication of the apparatus for reproduction in the ferns, the mosses, &c. could not believe that a kind of propagation so simple as that of the buds could have rendered necessary organs so multiplied and various. They endeavoured therefore to discover the stamens, the pollen, the pistil, the seeds, the embryos, and all the agents of fecundation, which are so palpable in ordinary plants; but as the analogy of form quits them, although they are united as to the principle, they diverge in its applications. Thus, what one takes for the pollen, another regards as the seed, and *vice versa;* so that these *sexualists* (as they are called) have no fewer disputes among them than their common adversaries, or the *agamists*.

In former reports we have noticed these disputes. The present year has revived one respecting the great work of M. Desvaux, on the family of the *lycopodia*. We know that these plants (recently separated from the other mosses by botanists) carry in small capsules a yellowish dust very combustible, which is well known by the name of powder of lycopodium, and which is used for various purposes. Its resemblance to the powder of the anthers made M. de Beauvois regard it as a true pollen. According

According to some observers, however, it does not burst in water like pollen; and it is admitted by all, that when it is spread on the ground it grows, and produces lycopodia. But the former property is not of an essential nature; and M. de Beauvois ascribes the latter to small globules, which he has distinguished among this dust, and which he regards as small bulbs or eyes; so that, according to him, it is not the yellow dust which springs up, but these bulbs, which cannot be separated from them. As to the true seeds which the pollen would be destined to fecundate, M. de Beauvois finds them in other capsules, placed sometimes between and sometimes under the former, and containing only small round seeds, transparent and larger than those of the yellow powder. But these peculiar capsules have only been found in about one third of the various species of lycopodia, and they have been looked for in vain in the rest.

M. Desvaux agrees almost entirely with M. de Beauvois as to the facts, but denies the consequences: he sees nothing in the yellow dust but bulbs or eyes, or, as he calls them, *propagules*, which have no occasion for fecundation in order to germinate. The other seeds observed by M. de Beauvois are merely, he says, if we may judge from their small number, their transparency, and irregular figure, *abortive propagules*.

M. de Beauvois answers, by showing that all the definitions which the most learned botanists have given of the seed are applicable to these globules; and setting out from the principle that the existence of a seed presupposes that of the female sex, and that the existence of one sex implies that of another, he adheres to his first ideas.

His antagonist replies, that a nominal definition formed according to the received ideas cannot decide a process wherein these same ideas are disputed, and that the visible characters of structure, recognised in all the seeds, are far from being verified here, on account of the smallness of the object.

We find that the discussion now becomes metaphysical. The only method of ascertaining the point in the eyes of hesitating physiologists, would be to operate the fecundation of what we regard as pistils by means of what we regard as pollen; but who could flatter himself with making on organs so delicate, the experiment which has so plainly demonstrated the existence of the sexes in common plants?

M. Desvaux has given besides a methodical distribution of all the known lycopodia, adding some subdivisions to those established by M. de Beauvois in a preceding work on the same subject, and taking for his principal bases the existence of the two sorts of capsules, and the division of the capsules into compartments more or less numerous.

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M. Decandolle, of Montpelier, has made known some parasite mushrooms of a new genus, which he calls *rhizoctones*, or *death of the roots*, because they attach themselves to the roots of the plants, and speedily destroy them. M. Persoon had united, under the head *sclerotium*, the fleshy fungosities in the interior like truffles, but without those veins which give to the consistence of truffles a marbled appearance. Hedwig had separated from them the *erysiphes*, which exist on the surface of the leaves; but we might still observe in those which remained, characters sufficient to furnish two genera: one genus not essentially parasitic, growing on dunghills and among putrid vegetables, has neither roots nor fibres: another (and to this the *rhizoctones* belong) sends out simple or ramified filaments, grows on the roots of living plants, attacks them externally, and kills them by absorbing their nutrition. They are multiplied with rapidity by means of these filaments, which propagate them from one plant to another, and thus occasion contagious diseases, from which many of our nurserymen have suffered severely. We were well acquainted with one species only, which produces the disease too well known in the *Gatinois* by the name of *death of saffron*. Another, which M. Decandolle describes for the first time, exercises its ravages on lucern, the roots of which are lightly embraced by its threads, which are of a fine lake colour: the stalks, when thus attacked, become yellow, and die speedily: and as the champignon is propagated by radiation, we soon see in the lucern fields many circular spots thus deprived of vegetation. The author recommends planters to dig around the infected places deep ditches, to prevent the creeping filaments from spreading, taking care to throw the earth within the circle, in order not to extend the evil instead of checking it.

One of the most difficult points in botany consists in fixing accurately the limits of the species, not regarding as such the varieties of soil and climate; and the chief way to avoid this error, is not to admit among the characters of the species the peculiarities of organisation, the mutability of which has been ascertained beyond a doubt. M. Desvaux having applied this method to the rose trees, and perceiving that several of their pretended species differed only in characters which were various in the same individual, succeeded in reducing considerably the nominal species of this genus. He has shown, for example, that the common wild rose (*rosa canina*) presents twenty-one varieties, the differences of which might be expressed by descriptions, but which pass insensibly into each other; and that thirteen of these varieties have been improperly raised to the rank of species by certain authors: six other pretended species have also been displaced from this rank, and assigned to the rose of the alps: five

five to the hedge rose, &c. If the same rigorous severity were extended to the whole of natural history, it would be greatly simplified and elucidated; but naturalists, to attain this object, must renounce the vain honour of augmenting without end the list of known species. In the present state of science there would certainly be more real glory in diminishing this list.

M. Delille, a member of the Institute of Egypt, read to the Class a very interesting history of the plants, wild and cultivated, of that celebrated country. He intends that it shall form part of the great work on Egypt, to which so many men of talents and learning have contributed, and which is published with a magnificence proportioned to the grandeur of an enterprise of which it will be the most durable monument. The author distinguishes the plants peculiar to Egypt from those which are carried thither by the inundation of the Nile and the winds of the Desert, and from those which are common to other countries: he fixes the limits assigned to each species in this long and narrow valley, by the latitudes, by the quality, more or less saline and more or less sandy, of the soil: he makes known the variations produced by each soil on the plants which grow in several soils; and he carefully explains the species cultivated, and the attention which each requires on account of the climate peculiar to that country, and perhaps unique in its kind throughout the world. We have to regret that a work, essentially composed of details, cannot be more fully noticed consistently with the limits of this report.

M. Decandolle has published an elementary theory of botany, in which he explains all the varieties of form and combination of the organs, as well as the terms by which they are expressed, establishing the rules of every reasonable nomenclature, and giving a general theory of the methods of distribution, and particularly of that which is called *natural*, because it is founded on the essential relations of vegetables to each other. He enters on this head into various considerations, which are peculiar to himself, as to the value of these relations, and on the organs and conformations of organs in which they ought to be found: he suggests new views or differences in appearance very considerable between certain vegetables, and which nevertheless arise from the abortion or unnatural junction of some of their organs. Setting out from the species in which this abortion or junction is openly manifested, he proceeds to other species where these phænomena are not visible, and which they must take for granted from analogies similar to the hypothesis to which men of science have recourse when they are deserted by facts, in order to leave no vacancies in the chain of their developments. These methods might be dangerous in less adroit hands, but M. Decandolle

dolle has made a moderate and ingenious use of them. His work must be of great service, by introducing more of a philosophic spirit into a branch of natural history too much abandoned to routine, and which still reckons among its followers too many servile imitators.

M. de la Peyrouse, of Toulouse, has published an abridged account of the plants of the Pyrenees, in one vol. 8vo. This work, which was a desideratum in botany, is chiefly gathered from the numerous journeys made by the author in this interesting chain of mountains, and comprehends the abridged descriptions of all the species which have been observed there, either by himself or by his predecessors, arranged according to the system of Linnæus, with the names of the places where they grow, and the best drawings of them which were to be procured. The work is, upon the whole, an important addition to the French Flora, and an excellent guide for those who wish to visit the interesting mountains of the Pyrenees.

[To be continued.]

XIII. *Intelligence and Miscellaneous Articles.*

On Monday the 3d of January 1814 the Class of Physical and Mathematical Sciences of France held their first annual meeting. Chevalier Halle presided, and the following was the order of their proceedings:

After the announcement of the prizes for the year 1814, M. Biot read A discourse on the spirit of invention and inquiry, as connected with the sciences.

M. Delambre read a notice on the life and writings of M. Malus, chief director of the Polylechnic School, and of Count Lagrange, both deceased.

M. Palissot de Beauvois read a paper on the way in which trees lose their leaves in autumn.

The meeting was terminated by a notice of the life and writings of M. de Sausure, by M. Cuvier.

The Class proposes as the mathematical prize question to be decided in January 1816, the following subject: The theory of the propagation of waves on the surface of a ponderous fluid of an indefinite depth. The prize will be a gold medal, value 3000 francs. The papers to be transmitted before the 1st of October 1815. The result will be announced on the first Monday of January 1816.

The Class had for the second time proposed as the subject of an extraordinary prize, "The theory of the oscillations of elastic laminæ," which was to have been decided at the present meeting.

meeting. Two essays were received; that which was marked No. 2. was totally unconnected with the subject of discussion in the contemplation of the Class.

The essay No. 1. had for its device these words of Bacon: *Sed longe maximum progressibus scientiarum et novis pensis ac provinciis in iisdem suscipiendis obstaculum deprehenditur in desperatione hominum et suppositione impossibilis.* The analysis which the author of this memoir has employed to attain his fundamental equation, has been judged altogether inaccurate, and this equation even seemed in no manner to result from this analysis; but that part of the memoir which contains the comparison of the theory with the experiments of M. Chladni, being drawn up with accuracy, and leading in general to satisfactory results, the Class considered it worthy of honourable mention.

The same subject is again proposed on the same conditions: viz. a gold medal of the value of 3000 francs, to be awarded on the first Monday of January 1816.

The Class had proposed as the subject of a mathematical prize question, to be decided at the present sitting, the following: "Determine by calculation, and confirm by experiment, the manner in which electricity is distributed over the surface of electrical bodies, and considered both as insulated and as in presence of each other; for instance, on the surface of two electrified spheres, and in presence of each other. In order to simplify the problem, the Class only requires the examination of the cases in which electricity spread over every surface remains always of the same nature."

None of the papers transmitted in answer to the above having been deemed worthy of the prize, the question was withdrawn.

The Class has not been made acquainted this year with any work which merits the prize for Galvanism, as instituted by Bonaparte.

The medal given by Lalande "for the observation of most interest or the memoir of most utility to astronomy, which shall have appeared in the course of the year," has been adjudged to M. d'Aussy jun. as a mark of encouragement, and on account of an extensive work on the perturbations and elliptical elements of the planet Vesta. This young astronomer is also advantageously known from the elements which he has given of the orbits of several comets.

A literary and philosophical society has recently been established at Riga. The members meet twice a month to consult as to the best means of diffusing knowledge, and facilitating the study of political œconomy, natural history, chemistry, and commerce.

commerce. Their more immediate objects are the extirpation of idolatry among the inhabitants, and the establishment of precautions against the ravages of fire.

Mineral water of Roisdorff, a village about a league from the Rhine and four from Cologne. M. Petazzi has analysed this water, which rises in a well above 17 feet deep and 5 wide, in an alluvial soil mixed with blocks of trap, and gives the following as its contents. In four litres (about 8·4532 English pints) he found 2·3356 litres (or 160 English cubic inches) of carbonic gas; muriate of soda 4·266 grams, ditto of lime 0·337; sulphate of soda 1·163, ditto of lime 0·217; carbonate of soda 3·544, ditto of lime 0·326, ditto of magnesia 2·809, and silica 0·043. Total 12·705 grams, or 196·216 English grains of solid matter. Its specific gravity to distilled water is as 1·0089 to 1·0000. The analysis was made when the water was at $8\frac{1}{2}$ degrees of the centigrade thermometer, that of the air being 12°. The spring is within 80 feet of a well of pure water, and about 160 from a strong chalybeate one.

Literature and the Sciences have been cultivated in Poland notwithstanding the occurrences of the last year. Count Sierakowsky has published at Cracow a magnificent work on Architecture, in two volumes folio, one of which is filled with plates. It is written in the Polish language, and the periodical works published in Poland speak of it in the following terms: "Architecture has been studied by various Polish authors; but none of their works have been brought to a conclusion, because the printing of them has been constantly interrupted by the misfortunes of the country. A short time previous to the last partition of the kingdom, Count Stanislas Potocki had formed an Architectural Society, with the view of publishing a Polish work on architecture, and Count Sierakowsky was a member of that society; but at the period above alluded to the project was abandoned, and the society dissolved. The latter nobleman, however, persisted singly in collecting materials; and the work he has now published is the result of twelve years labour and personal sacrifices, having published it at his own expense. It is divided into three parts: the first part treats of beauty, the second of convenience, and the third of the construction of buildings public and private. The author, having formed his taste in Italy, has introduced into the second volume, which consists entirely of plates, drawings of the finest public buildings in Rome, such as the *Circus Maximus*, &c. In short, the work embraces every department of architecture from the cottage to the palace, and from the cistern and the ice-house to the aqueduct.

duct. The work is upon the whole the more meritorious, as having been executed amid all the calamities of war. The author was formerly Conservator of the Crown of Poland, Chevalier of the Order of Stanislas, and Rector of the University of Cracow.

At the last anniversary meeting of the Royal Bavarian Academy of Sciences at Munich, M. Bader read a Discourse on the Possibility of determining the Principles of Ethics by Reference to the Physical Sciences. M. de Sebrank read an ingenious Dissertation on the Influence of mental and bodily Temperament in furthering the Progress of the Sciences, and on scientific Occupations. M. Schlichtgerol, Secretary General to the Academy, recapitulated the proceedings of last year. The library, he observed, had been greatly augmented; the botanical garden enlarged, and enriched with many new plants; and a donation had been made of the literary collections of the late Professor Schræber of Erlangen.

The Philological and Philosophical Class of the above Academy proposes for the year 1815 the following as the subject of a prize question: "As the writings and doctrine of Plato have been hitherto the subject of numerous philological and philosophical inquiries, and their success depends in an especial manner on a correct knowledge of the authenticity and chronological order of the writings of this philosopher, the Academy wishes that the learned should turn their attention to these subjects." In consequence, all men of letters who are acquainted with the works of Plato are invited to state what treatises in his name are of doubtful authenticity, and in what order those are to be taken which are avowedly authentic. Memoirs on this subject must be transmitted on or before the 20th of March 1815. The prize is 50 ducats, and the successful work is to become the property of the Academy.

M. Schoen, Professor of Mathematics in the University of Wurtzburg, has recently published at Nuremberg a mathematical work entitled "A Complete Theory of Astronomy, with a Supplement concerning the Calendar; preceded by the Theory of Conic Sections and some Curves of the higher Orders; with four tables and eight engravings." In this work M. Schoen supposes the reader already acquainted wi h spherical trigonometry and the first principles of algebra. The treatise on conic sections and other curves, only contains what is necessary to the understanding of the science which forms the principal object of his work, and from which it is in other respects entirely distinct. The author does not follow the analytical method, thinking

thinking it is too pregnant with difficulties for beginners. He discusses in particular every curve of the second degree, and afterwards shows their analogy with each other. Among the curves of the higher orders, he treats in detail the cissoid and conchoid; and, passing to the transcendent curves, gives as examples the cycloid, epicycloid, and hypocycloid.

His astronomy commences by an introduction perhaps too short for beginners: a few pages are only devoted to the chief phænomena of the heavens, such as the diurnal motion, the apparent motion of the sun, the determination of the equinoctial points, &c. These observations lead to a description of the horizon, the equator, and the ecliptic, on which all spherical astronomy depends.

In theoretical astronomy, the author treats of eclipses at very great length, and so as fully to attain his object. The use of the eclipses of the moon in the determination of geographical distances, might perhaps have been better placed after the details which he subsequently gives on the subject of the figure of the earth.

M. Schoen thinks that the mean barometrical height ought to be smaller under the equator than in the temperate zones, considering that the gravity is there less. Nevertheless, according to the known laws of hydrostatics, the absolute weight of the air ought to be equal over the whole surface of the sea, abstraction being made of the temperature: thus, in order that an equilibrium may subsist, the column of mercury must necessarily be higher in places where the weight is not so intense.

The variations of the barometer, caused by the increase and diminution of the temperature at different heights, are explained in a novel manner which deserves the attention of men of science.

The laws of Kepler are only exhibited in a historical point of view, since they may be inferred from the preceding part of the work. M. Schoen gives an indirect solution of the problem of the mean anomaly and the true anomaly.

The article in which the author treats of comets would have been more complete, if he had given a method for calculating the orbit of these extraordinary bodies by means of their apparent course. Lalande, Laplace, or Olbers, could have furnished him with the requisite materials. The article on perturbations is not very extensive, but it is sufficient to give a very correct idea of this remarkable phænomenon. The Appendix contains the history of the Calendar, the comparison of the æra of Nabonosor with ours; and, finally, the ordinary principles which are necessary for the drawing up of our calendar. Upon the whole, the work is more adapted for those who have gone through a course of astronomy already than for beginners.

A great number of antique vases have recently been discovered at Canosa, in the kingdom of Naples, in a most magnificent ancient tomb. Two of these vases are peculiarly interesting to the antiquarian. One of them is of a most extraordinary size, and its equal has never been yet discovered. It exhibits thirty-six painted figures, besides many emblems in various compartments, probably alluding to the labours of Hercules. On the neck are two elegant cars drawn by four superb white horses, being the chariot of the Sun and of Aurora. At the bottom there is a tetrastyle temple of the Tuscan order, in which as if in a niche are two personages of a dignified appearance, who seem to be Pluto and Proserpine. Lower down there is a Hercules armed with his club, combating Cerberus, besides various other figures not yet found out.

This vase, although very fine, is nevertheless, with respect to design and delicacy of workmanship, inferior to the other, which is smaller, and presents twenty-eight figures also in different compartments. We there find likewise the cars above described, and the tetrastyle temple; but we also read in the architrave of the temple, in excellent Greek characters, *ΚΡΕΟΝΤΕΙΑ*. On the left hand there is another figure, under which is written *ΙΓΓΟΤΗΣ*; and on the right hand there is a female, over whose head we read *ΜΕΡΟΓΗ*. Higher up are the two letters *ΖΝ*.

We also see on the left an interesting image of Death, *ΕΙΔΩΛΟΝ ΛΗΤΟΥ* is written over it. This image is represented in company with another wrapped in a black cloak, and with a menacing visage. Upon the head of the latter is a kind of two-curled wig. In the middle of the inner compartment there is a figure representing Fury, *ΟΙΣΤΡΟΣ*, and on the two sides two other figures; that on the left representing a man (Jason) *ΙΑΣΩΝ*, who is about to throw himself down; the other is Medea, *ΜΗΔΕΙΑ*.

All the amateurs and antiquarians who have seen the above vases pronounce them to be the most valuable in Europe, on account of their size, the variety of the figures, and the delicacy of the design. The tomb in which they were found is hollowed out of the solid stone. In the inside there are bas-reliefs representing hunting-matches: there were also found inside two casques, a cuirass, two pieces of leg armour, and a kind of ornament for the cuirass; all of which were gilt. Vases were also found without any figures or ornaments. The number of figures on each of the two great vases amounts to fifty. A detailed account, with drawings, is about to be published.

A variety of antiquities have been recently discovered in the environs of Astracan, and particularly among the Tartar *steppes* in in Russia. They consist for the most part of mud walls, on which are placed statues of stone coarsely carved. They seem to have been executed by a Mongol nation, if we are to judge from the costume and workmanship. One of the most remarkable monuments of this kind is to be found near the village of Prischiboriskoi, in the vicinity of Actuba: it is nearly a hundred and fifty feet in circumference, and three fathoms in height. The water used for the walls is a compound of lime, pounded charcoal and sand, and it is as hard as stone. It seems as if this monument covered the grave of some prince; for there were found in it, as well as in some other tombs in that country, jewels, ornaments, armour, and vases. The ruins of the great city of Madschari are in good preservation, and form three groups. The middle one is the most considerable: it consists in elevated squares half a league in length. The foundations of the ancient houses are easily distinguished; some buildings in stone are exceedingly well preserved, but most of them have been constructed of brick; they are from four to nine toises in height, and of a pyramidal form. Tombs and mausoleums are found in various parts of these edifices: the wall is so highly finished, and in such a good style, that it is scarcely credible that it could have been done by a nomade race. It may be reasonably concluded that civilized nations have formerly inhabited those countries. These remains of antiquity will speedily disappear, without doubt; for the present colonists are dilapidating them for the purpose of building their houses.

The excavations of Pompeia are pursued with the most unremitting zeal by the Neapolitan Government. The most recent discoveries have been three new magnificent tombs adorned with sculpture, and a hall which is supposed to have been the court of justice, and which is decorated with a triple row of columns. Bronze stoves were also found in the same place, of a most beautiful form, with a Faun and a Bacchus two feet high and exquisitely formed. Lastly, a small bronze Gladiator, precisely similar to that which has been already so much admired, and which is in the attitude of supplicating life from the pectators. A short time ago, there was also found a beautiful silver vase which seemed to have been used in religious worship; it is covered with figures in relief, and resembles in form that of the Imperial Cabinet of France, which has been published by Caylus and Dom Martin. There is a third vase quite similar in the cabinet of the late Cardinal Borgia.

There has been discovered in the Cabinet of Medals at Milan, a Chinese work, published in 1750 by order of the Emperor

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Kien Long, containing drawings of upwards of nine hundred antique vases resembling those denominated Etruscan. Several appear to be of very remote antiquity.

M. Deschamps, an agriculturist and botanist of Lausanne, has announced to the Society of Agriculture, Natural History, and useful Arts, of Lyons, some interesting experiments on the culture of the tea plant of Japan, and which convinced him that it will succeed perfectly well in Europe, if care be taken to sow it in a proper soil and climate.

M. Deschamps accompanied his paper with directions to gather and prepare the plant for use. Having analysed it, he discovered that it contained neither tannin nor gallic acid, principles which common tea contains, and to which is ascribed the property of affecting the nerves, and occasioning tremulous sensations. The disagreeable taste which some persons find in the tea of Japan has been also corrected by M. Deschamps, by throwing boiling hot water over the leaves, pouring it off in two or three minutes, and then infusing them in boiling water in the usual way.

M. Vogel has ascertained by several experiments that sulphur exists in the bile and in the blood*. After adverting to the opinions of various chemists, M. Vogel thus describes his experiments: "Being convinced that if the bile contained albumen it must contain sulphur also, which is a combustible body that generally accompanies this substance, I introduced two kilogrammes of fresh ox bile into a large glass retort; in the neck of the retort I placed some slips of white paper saturated with a solution of acetate of lead, adding a tubulated bell glass furnished with a curved tube, which entered into a flask filled with a solution of acetate of lead, and boiled the bile. In a few minutes the paper at the neck of the retort became sensibly black, and was covered with a metallic coating like galena. In the flask containing the solution of acetate of lead, carbonate of lead was at first formed, and a little sulphuret of lead was afterwards deposited. The disengagement of carbonic acid, according to M. Thenard, must be ascribed to a decomposition of the bile, or rather to the carbonate of soda which it contains.

"The blood also contains sulphur in the state of hydrosulphuret of ammonia, according to Proust. We may convince ourselves of this, even without distillation; it being only necessary to cover a flask containing blood with paper saturated with acetate of lead, and expose it to a temperature of 25 or 30 of the centigrade thermometer: in a few days the paper becomes black,

* *Annales de Chimie*, tome lxxxvii. p. 215.

and

and sulphuret of lead is formed. The same simple experiment will demonstrate that sulphur exists in the bile and in urine also. The sulphur probably exists in the above substances in the state of sulphuretted hydrogen, if this gas be not formed by the heat, or rather by putrefaction. It is also probable that sulphur exists in all the animal humours: but I have not found it in cow's milk; at least I did not discover its presence by operating on a few quarts of this fluid."

MESSRS. GALL AND SPURZHEIM'S SYSTEM OF CRANIOLOGY.

Having noticed in a former number of the Philosophical Magazine the Lectures of Doctor Spurzheim on the Physiology of the Brain, it may be interesting to many of our readers to have a short account of his proceedings in that science. The Lectures, which commenced on Monday the 11th instant, began with a declaration of the general plan of the system, and a refutation of the several opinions of other philosophers respecting the nature of the mind. He then proceeded to give an account of the different organs of the brain, and the means of discovering the relative degree of their development severally in different individuals. In order to give the reader some idea of the general outlines of the system, I must observe that the brain has always been considered as the organ of thought; that the present system differs from the general notion of physiologists, in regarding the brain not as one organ, but as an assemblage of different organs which have different functions; and demonstrates that the mind varies according to the relative degree of strength of these different organs in different people. These organs (which are upwards of thirty in number) are the material conditions of distinct faculties. The faculties are divided into, first, those which belong to the intellect, or what the Germans call *Ghëist;* and secondly, those of the sentiment and propensities, which they call *Gemuth:* these latter are again divided into, first, sentiment, and secondly, propensities. We have not room to enter further into the detail of this system. We shall therefore end by observing, that however unwilling the public may be to adopt any new system at first, and however intimidated by the numerous impositions of pretenders to science; every philosophic mind who will attentively study the present system will (we are assured by those who have studied it) be amply repaid for his trouble—even should he remain unconvinced of its truth. We hope to give more detail in our next. Dr. Spurzheim has a very respectable class, and is attended by many of the most eminent and ingenious medical practitioners of the metropolis.

The *Bibliothèque Médicale,* for August 1813, published at

Paris, contains an account of a wonderful cure of obstructions in the liver occasioned by biliary calculi. The remedy employed consisted of a combination of ether with spirits of turpentine, the efficacy of which was first discovered and promulgated in the Gazette de Santé for 1774. It was afterwards tried with the most complete success by Guyton Morveau and several other French physicians. In the case alluded to in the Bibliothèque Médicale, fifteen calculi, each of the size of an olive, were voided during six months that the above solvent was administered, and the patient is now perfectly recovered. M. Guyton Morveau recommends a combination of ether and the yolk of eggs, as giving less pain than that of ether and turpentine, when the patient suffers much in the expulsion of the biliary calculi.

M. John, an eminent chemist of Berlin, has recently published an octavo volume containing accurate analyses of several mineral, vegetable, and animal substances, of which the following specimens, from their novelty, may be interesting to our readers:

The juice of Euphorbia Cyparissias:—It is composed of 77 parts of water; an indeterminate quantity of tartaric acid; resin 13·80; gum 2·75; extractive 2·75; albumen 1·37; caoutchouc 2·75; and a little fat oil; the earthy parts of the euphorbia are composed of carbonates, sulphates, and calcareous phosphates.

The analysis of the *Asclepias syriaca* furnished resin 26·50; an elastic substance 12·50; a glutinous vegetable substance 4; extractive matter 4; tartaric acid and albumen 53. The plant when incinerated gave carbonate of potash, phosphate of lime, phosphate of magnesia, silex, iron, and oxide of manganese.

M. John afterwards analysed a scarlet elastic substance which comes from the East, through Turkey, known by the name of *Caoutchouc of Thibet*. This brilliant globulous matter is used by the Russian ladies for bracelets, ear-rings, and rosaries*; and it is merely a red oil insensibly indurated and oxidated, the colouring matter of which is similar to that of gum lac.

The fruit of the *Rhus typhinum* contains gallic acid from the instant of its appearance: but, as it grows, acidulated tartrate of lime is formed; and as soon as the circulation of the juices has ceased, acetic acid is formed; which seems to indicate that the latter is produced by the decomposition of the tartaric acid.

Analysis of Rocou.—This substance as it reaches the chemist is already changed by fermentation. M. John procured some seeds, from which he obtained the following results: an aroma, an acid, resin combined with a colouring principle, vegetable mucilage, fibrine, coloured extractive, and a peculiar matter which

* In the trinket shops of London these red drops or beads are erroneously called *Russian berries*. EDIT.

resembles

resembles mucilage and extractive matter. This analysis explains the reason why an alkali is added to the rocou intended for dyeing. The alkali combines with the resin, and forms a soap soluble in water: the alkali acts also upon the colouring matter, and renders it more lively.

M. Wittman, of Vienna, has published an extensive work upon Botany, and its application to rural œconomy, with a dissertation on the terminology of plants, an account of the system of Linnæus, and of the methods of Jussieu and Lamarck, and an Appendix on the Cryptogamia.

A Botanical Garden has been established at Siniscropal in the Crimea, principally with a view to collect all the indigenous plants of Russia, and also to procure the most curious plants from all parts of the world. One hundred acres have been inclosed for the above purpose in the village of Niktreh, near Siniscropal.

M. Parmentier, the French chemist, and whose name has so frequently occurred in the pages of the Philosophical Magazine, as an indefatigable contributor to the *Annales de Chimie*, died at Paris on the 18th of December last.

Several learned Societies in the departments of France have offered prizes for the best memoirs, describing the most efficacious preventives of choke or fire damp in coal- and other mines.

VACCINATION.

By an Official Report on the State of Vaccination in Sweden, printed by order of the House of Commons, it appears that the Swedish Government, attentive to the inestimable advantage which the Inoculation of the Cow Pox seemed to promise, directed the College to examine Dr. Jenner's discovery with the greatest accuracy, for which the proper means were immediately afforded; and the College was ordered, after collecting the results, to present its Report to the King.

This Report, which fully confirmed the excellence of the Jennerian discovery, occasioned the salutary law which was first enacted in 1803, by which Vaccination was established throughout the kingdom; and the College was commanded to promote its adoption by all possible means. The King was pleased to appropriate 900 dollars spec. banco to be divided into premiums, which were to be distributed among such medical men as could exhibit the greatest number of vaccinated persons.

A particular regulation was made for the metropolis, (which ought to be adopted in Britain,) imposing a fine of three dollars on on any one who should fail to announce to the medical officer of the district the appearance of the contagion of the Small Pox; and in every such case the person infected was to be carried to the Small Pox Hospital, where every measure was adopted for his being properly nursed; and the same precautions have been continued to the present time.

It was long a question, Whether new-born children could be vaccinated with success, and whether the matter taken from them might be employed with as much security as if taken from adults?

This doubt has been altogether removed, and in the General Lying-in Hospital in Stockholm all the children are now vaccinated within nine days from the time of their birth; so that, by means of this progressive vaccination, fresh matter remains constantly in existence.

For the more effectual encouragement of the practice of Vaccination, the King has been graciously pleased to appoint rewards of two different kinds, pecuniary premiums and honorary medals. The latter are distributed, commonly in silver, but sometimes in gold, to those who have particularly distinguished themselves. In all cases, those who have deserved rewards are humbly pointed out to the King by the College of Health; and His Majesty has reserved to himself the right of assigning the proportions in which these rewards shall be distributed. It is also in the King's name, and with a certain degree of publicity, that these marks of his approbation are bestowed.

For the honour of the medical profession in Sweden, it must not be forgotten, that although Inoculation for the Small Pox was one of the most lucrative branches of their private practice, and has been entirely superseded by the simple process of Vaccination, no one individual of the profession has raised any obstacles against the Cow Pox; but every one has contributed to its advancement, by giving advice, information and assistance, to the utmost of his ability.

It may therefore be asserted, that the Small Pox, that equally disgusting and destructive pestilence, which for many ages continued annually to send out of the world an immense number of young children, is now, through the influence of Dr. Jenner's inestimable discovery, so perfectly extirpated in Sweden, that it never can become epidemic, even if at any time, notwithstanding all the orders and all the vigilance employed for its exclusion, the infection should make its appearance. Such, in the last twelve years, has been the effect of the King's wise and humane attention, of the unanimity and disinterestedness of the medical profession, of the patriotic zeal of the clergy, of the good examples so promptly exhibited by the upper classes, and of the progress of information and civilization in the lower.

On

On Friday, July 15, Mr. Sadler and his son ascended in a balloon from Burlington Gardens, Piccadilly. The following is the account drawn up by Mr. Sadler of his aërial excursion:

"After clearing the east wing of Burlington House, the ascent was slow, and it was only evidenced to me and my son by the apparent receding of the objects, for it was not we who seemed to rise, but every thing beneath us to retire. In a few minutes we were perpendicular with Leicester-square, and our prospect was grand; the whole of London, and its magnificent buildings, lay below us, with its surrounding fields, canals, and parks; the beautiful serpentine form of the river, with its rich shipping, docks, and bridges. We enjoyed this scenery for about fifteen minutes, and at a quarter before four o'clock entered a dense cloud, which completely shut us out from all sight of the earth. At this time we could sensibly perceive the balloon to be rising. When we had soared through this cloud, my son observed to me, that from the variegated colours reflected from the multitudinous congregation of vapours around us, and the effulgence of different lights, he could scarcely see to any great distance, nor make any distinct observations on the numberless forms around us, although, from the splendour of the balloon, I could easily discover that we had altered our course towards the south-east. From the intense cold, and a most violent pain in my ears, which I never experienced before, our height could not be less, in my calculation, than five miles. The late Right Honourable W. Windham, when he ascended with me 30 years ago, experienced a similar attack in his eyes, though we had not then ascended above two miles and a half; but from my best observations, calculated upon former experiments, we must have been about that height. My son soon afterwards found the same effect, though in a much slighter degree. By the various currents of air, and the renewed motion of the machine, I judged we were approaching the sea; and requesting my son to open the valve, we perceived ourselves rapidly descending. The clouds were so near the earth, that after lowering for a quarter of an hour, we heard the lowing of cattle, but could not discover *terra firma*. Shortly after, the clouds opened beneath us, and displayed the variegated fields and the Thames, which informed us that we had altered our direction again, and were returning from the south-east to the north-west, over East Tilbury, &c. We had a distinct view of the mouth of the River, Margate, Ramsgate, &c. with their coasts and shipping; but the clouds collecting, and rolling over each other, again inclosed us. After once more descending below the clouds, we saw an inviting hay-field at a considerable distance, and opening the valve again, a sufficient quantity of gas escaped for us to reach the proposed spot; and after

after throwing out the grappling irons, we came to the ground easily. We remained quiet till all the gas had escaped; by which time a number of spectators arrived, but not before we had secured every thing properly."

Dr. Pearson having delivered Three Courses of Lectures annually during the last 26 years on the Theory and Practice of Physic, on Therapeutics with the Materia Medica and Medical Botany, as well as on Chemistry, proposes in future to confine himself to the department of the Practice of Physic with the Laws of the Animal Œconomy. This Course will commence, as usual, in the first week of October, at No. 9, George-street, Hanover-square, at 9 in the Morning.

The Lectures in the other departments, viz. On Therapeutics with Materia Medica; on Clinical Practice with demonstrative Pathology; on Medical Jurisprudence; and a full Course of Chemistry, will be given at the Theatre in Windmill-street by the united labours of Dr. Roget, Dr. Harrison, and Dr. John Davy.

LIST OF PATENTS FOR NEW INVENTIONS.

To William Alfred Noble, of Riley Street, Chelsea, in the county of Middlesex, engineer, for his improved steam and fire engine, and a new means of connecting or joining steam or water pipes together.—23d March 1814.—2 months.

To Emanuel Heaton, of Birmingham, in the county of Warwick, gun-finisher, for his improvements to the locks and breeches of fire-arms, by rendering the pans of locks, and communication between the priming and loading of fire-arms, water proof.—23d March.—2 months.

To John Sparks Moline, of Leadenhall Street, London, leather merchant, for his improved method of tanning leather.—28th March.—6 months.

To Joseph du Dyer, of Boston, State of Massachusetts, one of the United States, but now residing in the Adelphi, in the county of Middlesex, merchant, who in consequence of a communication made to him by a certain foreigner residing abroad, is become possessed of a certain improvement in machinery for manufacturing nails of various kinds.—1st April.—6 months.

To George Smart, of Ordnance Wharf, Westminster Bridge, in the county of Surry, timber merchant, for his improvements in machinery for grinding corn and various other articles.—1st April.—2 months.

To James Wood, of New Compton Street, in the county of Middlesex, musical instrument maker, for his improvement on the

the German flute, applicable also to the clarionet and bassoon.—1st April.—2 months.

To John U. Rastrick, of Bridgnorth, in the county of Salop, civil engineer, for his steam engine on a new and improved construction.—1st April.—6 months.

To Isaac Mason, of Wellen Hall, in the county of Stafford, tea-tray maker, for his method of making stamped fronts for register stoves, ship stoves, and other stoves, fenders, tea-trays, and other trays, mouldings, and other articles in brass and other metals.—7th April.—6 months.

To Joseph Roberts, of Brownlow Street, Drury Lane, blind-maker, for his apparatus intended to be used for map rollers and carriage blinds and other similar objects.—7th April.—2 months.

To William Whitfield, of Birmingham, in the county of Warwick, scale-beam maker, for certain improvements in carriages.—9th April.—6 months.

To John Read, of the parish of Horsemonden, in the county of Kent, gardener, for his means of raising and conveying water, steam, gas, or any other fluid, by pipes of purified earth.—18th April.—2 months.

To Lewis Gompertz, of Kennington Oval, in the county of Surry, gentleman, for his improvements in carriages (and substitutes for wheels for carriages) and other machines.—27th April.—6 months.

To David Grant, of Picket Street, Strand, soda-water manufacturer, for his pump or apparatus for drawing off soda water, and other liquids impregnated with fixed air.—27th April.—2 months.

To John Bernard Logier, of Sackville Street, Dublin, professor of music, and music-seller, for his apparatus for facilitating the acquirement of proper execution on the piano forte.—28th April.—6 months.

To Joseph Price, of Gateshead, in the county of Durham, glass-maker, for his methods of making glass.—5th May.—2 months.

To John Vancouver, of Ickenham, in the county of Middlesex, esq. for his method of painting walls of apartments and other surfaces by the preparation, use, and application of certain materials for that purpose.—17th May.—15 months.

To Thomas Abree Pickering, of Hackney Terrace, Hackney, gentleman, for his method for preventing losses, and for the security of remittances (containing bank notes, post bills, country bankers' or other notes or bills payable to bearer in London, or the country) by coaches, so frequently occurring.—21st May.—6 months.

To

To William Moult, of Bedford Square, in the county of Middlesex, for his improved method of acting upon machinery.—23d May.—6 months.

To William Neville, of Birmingham, in the county of Warwick, merchant, and coach-founder, for his method of making hurdles, gates, palisades, virandas, balustrades, stair-case rails, espalier frames, and various other articles.—26th May.—6 mo.

To William Sellars, of Kemsey Elms, in the county of Worcester, engineer, for his method of spinning and laying of ropes, twine, line, thread, mohair, wool, cotton, and silk, by machinery.—7th June.—2 months.

To George Heywood, of Blackmoor Iron Works, near Stowbridge, in the parish of Kingswinford, in the county of Stafford, ironmonger, for his improved plan or method of turning rolls, and of rolling gun and pistol barrels previous to welding —7th June.—6 months.

To John Stubbs Jorden, of Birmingham, in the county of Warwick, copper-sash manufacturer, for his improved method of making the lights, and also other improvements in the construction of horticultural buildings.—7th June.—2 months.

To Grant Preston, of Burr Street, London Dock, in the county of Middlesex, brazier, for his concave cabin stoves.—7th June.

To John Buxton, of Great Pearl Street, Spital Fields, in the county of Middlesex, cotton manufacturer, for his improved method of twisting and laying cotton, silk, and various other articles.—7th June.—6 months.

Meteorological Observations made at Clapton in Hackney from the 21st of June to the 21st of July 1814.

June 21.—Cloudy, with a little rain.

June 22.—The same sort of weather.

June 23.—Wind still northerly. Weather variable.

June 24.—Cool and cloudy for the most part; with rain.

June 25.—Cool, cloudy day; fine and warmer evening. N.

June 26.—Cold wind and showers from N.

June 27.—Fair day, but much cloud. N.

June 28.—Fine warm day. Thermometer 73° in shade, at 2 P.M.

June 29.—Thermometer 76°. Fine warm day.

June 30.—Much *cumulostratus* of late. Thermometer today 75°, and fair.

July 1.—Cooler in the morning; very fine evening.

July 2.—Fair and warm day; abundance of *cirrus*, sometimes like a long diverging feather; in other places in angular flexures,

flexures, or in filiform bands, with *cumulus* below. In the evening *cumulostratus.* Fine moonlight night.

July 3.—Fine warm day. Thermometer 78°. *Cirrus* and *cumulus* prevailed.

July 4.—Warm day. *Cumulus, cirrus,* and *cumulostratus.* Towards evening the clouds increased, and by midnight a fine and gentle rain came down, with a mild atmosphere.

July 5.—Fine warm day. Thermometer 78°. *Cirri, cumuli, cirrostrati,* and *cumulostrati.* Fine evening.

July 6.—Fine and clear at sunrise; about half after five a breeze from SW. brought a white mist, which was thickest above, and quite obscured the sky. It cleared off, and the day became warm and fair, with *cumuli.* Thermometer about the same as yesterday. Fine evening, with some brilliant meteors at night.

July 7 to the 18th, weather generally warm, and with occasional rains, which produced a fruitful vegetation.

July 19.—Fair day; wind westerly; various clouds and *nimbi.*

July 20.—Cloudy and thick early. Warm day and fair; much *cumulostratus,* &c.

July 21.—Warm morning; a gale afternoon; various clouds, as in warm weather.

A friend of mine*, in discoursing with me lately on physiological subjects, mentioned the occurrence of increased irritability in persons at certain periods, in such a manner as would lead one to suspect that there were general and periodical causes of disorder, which more or less affected most people. I have frequently noticed this circumstance myself, and believe it to be the consequence of a state of the atmosphere, the peculiarities of which are at present little known. The particular kind and arrangement of the clouds seem to me to be one indication of this state of the air. As I have before discoursed largely on this circumstance, I need only recall the attention of your readers to the subject; as it is only by a multiplicity of observations that any thing certain in natural philosophy and physiology can be determined.

Clapton,
July 21, 1814.

THOMAS FORSTER.

* Dr. Spurzheim, (the colleague of Dr. Gall of Vienna,) who is now giving in London a Course of Lectures on the Physiology of the Brain.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For July 1814.

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	52	57	56	30·00	37	Cloudy
	28	57	66	57	29·90	46	Fair
	29	60	69	62	·94	67	Fair
	30	62	69	64	·97	80	Fair
July	1	57	66	54	·90	66	Fair
	2	57	66	54	·95	70	Fair
	3	60	76	58	30·02	64	Fair
	4	63	74	62	·04	57	Fair
	5	62	78	64	·07	60	Fair
	6	62	74	66	·08	62	Fair
	7	63	76	62	29·98	66	Fair
	8	63	66	64	·90	0	Rain
	9	64	66	63	·86	56	Showery
	10	63	66	66	·89	0	Rain
	11	64	71	60	·98	64	Fair
	12	62	72	60	30·15	50	Fair
	13	60	68	56	·05	55	Fair
	14	57	63	57	29·85	36	Cloudy
	15	56	66	57	·75	40	Showery
	16	57	63	54	·80	0	Rain
	17	54	66	56	·96	41	Fair
	18	57	68	58	·97	36	Showery
	19	58	68	58	·75	36	Showery
	20	60	69	60	·70	45	Fair
	21	62	70	62	·81	58	Fair
	22	63	74	63	·95	60	Fair
	23	63	75	64	30·20	65	Fair
	24	68	77	63	·14	71	Fair
	25	65	78	66	29·87	70	Fair
	26	66	80	67	·90	86	Fair

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

XIV. *Some Account of the Life and Writings of* Condorcet. *By M.* Lacroix*.

Jean Antoine Nicolas Caritat, Marquis de Condorcet, one of the most distinguished philosophers of the eighteenth century, commenced the study of the mathematics with very great success, never lost sight of them, but attached himself principally to the diffusion of knowledge in general, and to the discussion of the most important subjects in social œconomy. His father, descended from an ancient family in the Venaissin, resided in the Castle of Ribemont in Picardy. Here Condorcet was born on the 17th of September 1743, and came in 1758 to study philosophy at the college of Navarre: there he sustained a thesis in mathematics, in presence of Clairaut, D'Alembert, and Fontaine, who judged him worthy of one day taking his place among them. In 1762 he settled at Paris, with his old professor of philosophy Giraud de Keroudou, in order to give himself up entirely to the mathematics. Soon afterwards he connected himself with Fontaine, a geometrician endowed with great sagacity, but whose singular character and strange habits had prevented his progress. It was the peculiar doctrine of Fontaine that Condorcet proposed to develop and extend in his Essay on the Integral Calculus, the first of his works; but the Theory of the Equations of Condition, with which he commenced at the age of twenty, gave him a very distinguished rank, since we there find the demonstration of several important theorems, which Euler had met with by chance only, and the direct proof of which he regarded as being very difficult. The remainder of this work, containing only general ideas, which require to be fixed and even proved by applications, announced much sagacity, and a profound knowledge of his subject, but left too much to be done in the details to enable us to derive any advantage from it. He afterwards treated in the same spirit the Problem of the Three Bodies, of which he was the first to give equations at once general and completely symmetrical; but he did not stop to particularize them, in order to render them accessible to the methods of approximation, and contented himself with developing the spirit of these methods, and the precautions which their use requires: "My object (he says) is to give general principles, without entering into details which should facilitate to others routes which I have not the courage to pursue." These words, which escaped him, would seem to indicate a sort of scientific egotism: but such an interpretation would be quite unfair so far as Condorcet is concerned, who desired nothing so much

* *Magasin Encyclopédique* for November 1813, p. 54.

as the advancement of the sciences. The above passage is merely the frank expression of the impatience of a young man, whose ardent spirit was only to be satisfied with the contemplation of a grand whole. He subsequently ascertained that the state of science required another course. "Glory (he observes), which was formerly the prize of genius alone, can nowadays be only the prize of genius and labour united."

Judging severely of his first writings, which he had collected under the modest title of "Attempts at Analysis," he composed in his more mature age a new Treatise, in which the differential calculus and the integral calculus, embraced in the aggregate, were developed and applied, and in which the hypothesis of infinite simals was replaced by new, very exact and very ingenious considerations. The printing of this work, begun in 1786, was interrupted at the sixteenth sheet. The manuscript however was not lost; and it is a drawback upon the memory of Condorcet as well as upon science, that it has not been published; for it appears that he bestowed great attention upon it. In order to judge him with equity as to his other mathematical writings, we ought rather to consider what he has shown himself capable of doing than what he has done. Nevertheless, if we confine ourselves to the memoirs which he published in the Academical Collections of Paris, Turin, and Petersburgh, and which chiefly concern the application of the series to the resolution of every kind of differential equation, we shall find him always at the head of the most recent discoveries and of the most difficult theories, throwing out useful observations as to every thing which occupied the first geometricians of his time, pointing out to their inquiries new combinations, and presenting remarks worthy of their attention. Such is the employment of the equations with finite differences for determining the arbitrary functions contained in the integrals of the partial differential equations: such also is the integration of the equations with mixed differences, which he was the first to consider. In 1778 he shared a prize from the Academy of Berlin, on the theory of comets: at the end of some experiments on the resistance of fluids, made in conjunction with Bossut and D'Alembert, he gave formulæ for deducing the laws of phænomena from observations: lastly, he inserted some articles on the subject of transcendent mathematics in the supplements of both the old and new Encyclopædia.

The merit of these labours will appear still greater, when we consider that they were merely accessories in the career into which Condorcet was led by the desire of contributing in the most efficacious manner to the general improvement of the human mind. Elected a member of the Academy in 1769, and afterwards in 1771 assistant secretary to Grand-Jean de Fouchy,

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he endeavoured in the *éloges* which issued from his pen, to promulgate those reflections of a general interest, which have not only for their object to diminish the dryness of the matter, but to form the public opinion as to every thing which might have a tendency to improve the state of society. In these productions we may see that philosophic spirit which Condorcet had imbibed from the writings of Voltaire, and the friendship of D'Alembert.

While thus cultivating a talent for composition, he seized upon the species of magistracy which must without fail be exercised in the republic of letters by a learned man who acts as a kind of interpreter to the *élite* of the learned in an enlightened country, when he unites the charms of style and clearness of detail to grandeur of views, a magistracy which had been vacant since the retreat of Fontenelle from public life. The talents of these two celebrated men differ like the spirit of the several æras at which they entered upon public life. When Fontenelle undertook the History of the Academy, the cultivation of the sciences was still little diffused: fearing therefore to embark in details which would have been interesting to a small number of auditors, he dissembled under ingenious comparisons rather than elucidated the difficulties of the subject. To make friends every where to the new doctrines, by enabling the mass of the people to catch at certain points, to perceive in them some shade of the useful or agreeable, and to adorn their conversation with some brilliant sallies, these were his objects. He proposed also gradually to sap those prejudices from which his own excellent understanding had freed and guarded him; but his prudence and the period at which he lived permitted him to make indirect attacks only. When the Academy intrusted the office of Secretary to Condorcet, things were greatly altered. The progress of education had considerably increased the number of persons who were acquainted with the sciences: he could therefore explain the new discoveries in language which united elegance with precision. On the other hand, the love of knowledge, or at least the desire of showing it, had become as it were the national spirit, and public opinion was powerful enough to protect the writer who knew how to express useful truths in an eloquent manner. Nothing, therefore, prevented these truths being clearly developed as often as occasion presented; and Condorcet possessed an energy of character which would not allow any thing to escape him: this was to him, as it is to every friend of humanity, an imperious duty, as he has most eloquently depicted at the commencement of his *éloge* on Chancellor de l'Hôpital.

All the *éloges* which he composed have, as far as the subject would allow, two kinds of merit very remarkable: in the first place we find perfect analyses of the labours of the personage praised, prepared by summaries in which the objects and the progress of the sciences are rendered of easy attainment without any want of precision. Afterwards we find criticisms stamped with the authority of reason, at least in the eyes of those who knew how to defend themselves from the exaggerations of enthusiasm as well as from the spirit of detraction; and he loses no opportunity of raising his voice with ardour, and yet with decency, against the abuses and prejudices which attend the subject undar discussion. In praising Haller, Linnæus, Daniel Bernoulli, Vaucanson, Euler, D'Alembert, Bergman, Buffon, and Franklin, it was necessary that he should review all the sciences, and give an account of the greatest discoveries of the age: he had also occasion to speak of the honorary academicians, members chosen from among the great men in place. He knew how to mingle with their *éloge* judicious observations on their operations, and on the perfectibility of the social edifice: but he loved truth too much to consent to palliate the vices of an oppressive minister, and he preserved the most profound silence with respect to Vrillière, the odious dispenser of the *lettres de cachet* under the reign of Louis XV. As Secretary to the French Academy, D'Alembert has also made *éloges* which have merited and obtained reputation: but those of Condorcet are stronger in point of facts, while the style is graver and better supported; and in the *éloge* of his predecessor, he has traced the duties which he imposed upon himself, and the course which he proposed to hold.

His mind, constantly occupied with the great interests of truth and society, could no longer descend to the fatiguing and barren calculations inseparable from the researches into pure mathematics. In correspondence with Voltaire, and intimately connected with D'Alembert, he necessarily took the greater part in philosophical discussions, the greater were his means of obtaining success, by adding to the solidity of a judgement matured in the most abstract subjects, a great facility of expressing his ideas: thus we find him, in the "Letter from a Theologian to the Author of the Three Centuries," repelling in a happy strain of irony the ridiculous assertions of Sabathier de Castres.

Tired of hearing the "Thoughts of Pascal" abused without restriction, Voltaire had already hazarded upon this book, a monument at once of the weakness and strength of the human mind, some very sagacious and well formed reflections: nevertheless, but little more was as yet known as to the object of those

those thoughts, than what was divulged by the first editors. Père Desmolets repaired in a great measure their omissions: but all these pieces, thrown together almost as if by chance, losing much of their value, Condorcet knew how to class them in an order which, if it was not exactly that which the author himself would have followed, is at least very luminous; and since it had been permitted to the followers of Pascal to make a choice conformable to their own opinions among his Thoughts, it also belonged to a philosopher to present only those which agreed with his plan, in using as it were such pieces as were precisely in unison with the excellent *éloge* of their author which Condorcet composed. Some essential differences between various editions of some of the Thoughts, and that given by Condorcet, brought upon him an accusation of infidelity; but he has been completely exculpated by M. Renouard in his excellent edition of the Thoughts of Pascal, and such a crime would have greatly astonished those who knew the virtues and prudence of Condorcet.

Political œconomy, which its importance ranks among the highest branches of human knowledge, which requires aid from almost all the rest, and can only be founded upon the most wholesome morality, frequently occurred to the meditations of Condorcet; it interested him still more during the administration of his friend Turgot, whose intentions were so pure, and whose conduct was so upright, as to entitle him to the appellation of the most honourable man of his age. The fruit of this intimacy was a Life of Turgot, in which Condorcet analysed the operations of this statesman. It is not enough to say that Condorcet was an œconomist: but he was none of those who follow the footsteps of any master, for he acted upon his own opinions: and on this account our criminal laws, so long reproached for their barbarity, and our jurisprudence, which was so imperfect, were the subjects of various essays dictated by his philanthropy.

Questions connected with administration and with judiciary proceedings, like every thing which depends upon facts too complicated for the discovery of their causes, and for the attainment of their effects, cannot in general be resolved but according to probabilities. In the language of common life, we thus denominate the perceptions which our moral habitudes produce in our minds, or the slight and inexact appreciations which we make in conformity to those habitudes, and upon the particular facts which chance brings to our knowledge: but with respect to the geometrician, probabilities ought to be susceptible of measurement, or they are nothing. He ascends to the primitive or independent facts, and calculates their number if that be possible; or rather he sets out from precise observations, and there

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finds data for assigning by calculation the probability of every cause of which the aggregate facts may be the result. Condorcet embraced with ardour this kind of speculation, which offered food for his passion for the interests of society; and he brought to his aid the mathematics, the cultivation of which he had carried too far not to experience the necessity of always resorting to them. Such were the origin and object of several memoirs which he read to the Academy of Sciences, and of his "Essay on the Probability of the Decisions given by Plurality of Voices," which comprehends the judgements and elections, the form of the tribunals, and the minutiæ of all deliberative assemblies. The questions treated in this work are of incontrovertible importance: their connexion is very methodical; perhaps the solutions are not sufficiently profound, and geometricians might desire more elegant methods of calculation; but the preliminary discourse, which is the analysis of the entire work, is at the same time an excellent treatise on philosophy. While upon this subject, let us not omit the plan of his Social Mathematics, a table equally complete as it is luminous of the titles of all the chapters of a work embracing in the greatest detail the application of the mathematics to the universality of the objects of social œconomy. The execution of this plan would from its utility do honour to the accurate inquirer who should take charge of it, particularly if he knew how to add perspicuity to precision, and disdained the trappings of analysis wherever they were not strictly necessary.

Faithful to the views of utility which guided him in all his writings, Condorcet took for the subject of his discourse at his reception into the Academy in 1782, the advantages which the union of the moral and physical sciences might produce to society. Keeping pace with the spirit of the age in which he lived, he took part in the revolution: those who knew him best, never imagined that he embarked in it from motives of ambition; for they knew that, under the ancient regime, he was strongly attached to his personal independence; but if the perspective of the ameliorations which philosophy ought to produce on society warmed so many honourable minds in the early period of the revolution, could that effect be wanting in a man who concealed under a calm exterior an energy of mind so striking that his friends compared him to a *volcano covered with snow?* His work on the Provincial Assemblies, published in 1788, had in view to pave the way for those reforms of which the administration of the kingdom appeared to him to be then susceptible: speedily the rapid succession of events and the conflict of opinions made him sensible of the possibility of carrying these reforms further. He attached himself at first to the constitution of of 91, and published several political writings which caused him to be appointed one of the deputation from Paris to the Legislative Assembly on the 1st of October 1791. Afterwards as deputy from the department of the Aisne, he voted generally with the members who were called Girondists.

Condorcet might perhaps be reproached with having endeavoured to temporize with the anarchists: nevertheless, by watching his progress, and examining his discourses, we soon discover motives more than excusable for this conduct, which was at most but a political error flowing from his moderation and perseverance. He did not wish to quarrel with the opposite party, because it could boast of misguided men, who might be brought back to reason by prudent treatment; and he feared lest, by violently stopping the revolution, the force of the impulse might throw the public spirit too much behind. "He wished rather," he used to say, "to try to moderate them than to embroil himself with them." "I wish that every man would occupy himself a little less with his own interests, and a little more with those of the public."

Being soon convinced of the inutility of his efforts, he absented himself from the debates in the Convention, and was not at first among the number of the members proscribed on the 31st of May; but in a letter signed by the great majority of the deputies of his department, he called down the indignation of his constituents on the attempt which they alleged he had made against the national representation: he did not disguise his sentiments on the shadow of a consitution drawn up in 1793 by the orders of Robespierre; and on the denunciation made by Chabot of Condorcet's observations, the Convention ordered that he should be accused at the bar: on the 3d of October 1793 a decree of accusation was passed, and soon afterwards he was put *hors la loi*.

Compelled to conceal himself, he found an asylum for a long time in the house of a generous female, who had not hesitated to expose her life in order to save that of Condorcet, whom she did not then know: but when they began to pay domiciliary visits, "I must leave you," he said to her, "you are *hors la loi*." "But we are not," she replied, "beyond the pale of humanity." The danger becoming still more urgent, he tried to leave Paris in March 1794: he proposed to go to Fontenai; but when he presented himself at the door of the person who was to receive him, she was not at home, and he was constrained to pass two nights in the open air, one night concealed in a quarry, and the other under a tree in the fields. Worn out with fatigue and hunger, and grievously hurt in his feet, he entered an ale-house at Clamard-sous-Meudon. Struck with his uncouth appearance, a member of the revolutionary committee

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of the place thought him a suspicious character, and took him before the committee, who sent him the same evening to the prison of Bourg-la-Reine. When the revolutionary assassins came to interrogate him next day (28th of March 1794) he was no more. Sensible that inevitable death awaited him, he made use of the poison which he constantly carried about his person, to avoid the catastrophe which was prepared for him by the ferocious enemies of reason and humanity.

In those trying times, which would have entirely depressed or exasperated minds less firm, or even more occupied with their personal interest, he developed the noblest character: every moment was employed with thinking on the welfare of the human race, and he occupied himself in the improvement of the sciences, and the instruction of youth, with a zeal, a mildness of expression, and a firmness of principle, of which there are very few examples. Other men of letters embraced the revolution with as much warmth, and only changed their opinion when it attacked them; but then they spoke rather from sentiment than from the impulse of reason. When we read "The Sketch of a historical Picture of the Progress of the human Mind," we find nothing but the most active benevolence, and the most sincere wishes for the happiness of mankind. This work, in my opinion, is one of thoe whose perusal is indispensable at the conclusion of every course of education, in order to give connection to the various studies of which it consists. Some pages of the last section, being those in which the author seems to assign to the duration of human life an indefinite extent, as a consequence of the perfection of the human race, have been fiercely attacked by the critics: but this exaggerated conclusion, this spot in the sun's disk, if we may so call it, what is it in comparison with the merit of the rest of the work, which is a *chef-d'œuvre* of precision, learning and genius? While, deprived of all literary resources, Condorcet drew upon his memory alone for the materials of this finished performance, he composed a Treatise on Arithmetic, not less remarkable of its kind. Fearing that he might not see his daughter again, he traced out for her a plan of conduct the wisest and most sensible which goodness and philosophy combined could have dictated. At this period even he made verses for his benefactress, and addressed to his wife an epistle, in which is the following couplet:

> Ils m'ont dit: "Choisis d'être oppresseur ou victime:"
> J'embrassai le malheur, et leur laissai le crime.

This is not the occasion for discussing the great political questions which occupied the last days of Condorcet's life. We refer to the complete edition of his works, such readers as have not

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blamed indiscriminately every thing which took place at a certain period, and every thing which certain persons have written, and who think, besides, that philosophers ought to be awarded the right of separating their theories from the abuses to which they are subject, in the same way as the truly religious have a right to distinguish between the precepts of the gospel and the crimes of fanaticism. They will there find a great number of things which the rational part of the community in all ages ought to approve; and they will feel with the author, that "to say we are without affections and without prejudices in the midst of the greatest interests which can agitate mankind, is to boast of a virtue which human nature can never attain, it is to confess oneself either indifferent or a hypocrite."

If some of his political opinions seem to have varied with circumstances, his philosophy always remained the same; and although he has not drawn up a special Treatise, it would be possible by collecting the results scattered over his various writings, particularly in his Life of Voltaire, in the notes with which he has enriched the complete edition of the works of that great man, and in his reports upon public education presented to the Convention; it might be possible, I say, to compose a Syllabus which should embrace the most important questions in metaphysics and morals. The foundation of this philosophy is scepticism; but a graduated scepticism, which weighing with exactness the various probabilities of our opinions, shows the true bases upon which they rest, and how these probabilities, increasing with the repetition of observations and the frequency of phænomena, may approach certainty indefinitely in every thing connected with the knowledge and determinations truly essential to our existence and preservation.

In order to make known the character of Condorcet, and even in some measure his exterior deportment, which gives some idea of it, we shall quote a passage from the Correspondence of Grimm (tome ii. p. 430, first edition). "He is a very great genius, full of reason and philosophy; on his visage reside calmness and tranquillity; goodness sparkles in his eyes. He would be the worst man in the world, if he was not an honest man; for he would deceive every body by his physiognomy, which announces the gentlest and most beneficent qualities. But his character does not belie his figure, and his friends call him, *par excellence*, the good Condorcet. There reigns throughout his *éloges* a great deal of mind with much simplicity." To the above we may add, that timid and even much embarrassed in a numerous circle, he displayed a mild and sprightly turn among his friends, and those whom he admitted to be familiar with him. He seemed completely to forget the advantages given

him

him by the high consideration which he had so justly acquired by his talents and success: he knew how to descend to the level of those who were merely entering upon those studies in which he excelled. He never endeavoured to force his opinions upon persons whom the superiority of his talents or the influence of his character rendered his inferiors. "*I may deceive myself,*" he observed: "*and if I am wrong, my friends must not conceal it from myself or from others.*" In patronizing young men, he did not merely give them the advantage of his protection, he served them with zeal; and by means of a simple and modest tone of voice, a civility truly affectionate, by the pleasure and instruction which he blended in his familiar intercourse, he inspired them with a gratitude as gentle as it was sincere, and an attachment as durable as it was respectful.

We have already mentioned the friendship conceived for him by Turgot and D'Alembert: the latter made him one of his testamentary executors. He had many other friends equally celebrated for their talents and their virtues: among these was the Duke de la Rochefoucauld, and his respectable mother the Duchess d'Enville. Their attachment for Condorcet was so much the subject of animadversion at the commencement of the revolution, by those who surrounded them, that they gave up his acquaintance with regret, yielding rather to importunate and unremitting solicitations than to their own conviction. About this period, when men and even things were liable every moment to take opposite qualities, it was no longer possible to establish a character on the public voice. Condorcet, like all those who made a figure in this dreadful crisis, was subjected to the most contradictory opinions; but there remained to him, among men of intelligence and virtue, some faithful friends, who preserved for his memory a just respect, which will not be belied by an examination of facts, if it is directed by that spirit of moderation and impartiality which knows how to make allowances in every series of events for the caprices of fortune, and the errors which naturally occur in the wisest political combinations.

The character of Condorcet is impressed in the forms of his style. The enemy of all declamation, he rarely quits the tone of calm discussion: we ought not therefore to expect from his writings grand displays of eloquence, but always accuracy, frequently elevation, and also profound research. We might reproach him with negligence, which he would have no doubt corrected, if he had attached more importance to the form of his writings, which were almost always produced on the spur of unforeseen occasions. He is also accused of being occasionally obscure; and it must be confessed, that the man who could in his literary productions explain abstract propositions,

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and the most various technical details of science, with clearness, is not the same person when he writes upon the pure mathematics. Something similar has happened to D'Alembert; and perhaps both may be excused on the ground that profound geometricians like them, when writing on a science so familiar to themselves, have no idea of submitting to that plainness of language which the universality of readers requires. The manner in which Condorcet expresses himself as to style in the preface to his *Assemblées Provinciales* proves clearly that he knew what was wanting in this respect, and how it was to be attained. Nevertheless it cannot be denied that, by dwelling too much on abstract propositions, as if he had wished to give proofs of his *finesse*, his ideas sometimes lead as it were into a labyrinth, and to conclusions which are a little strange: but, once for all, these were mere negligences, which did not prevent Condorcet, who was endowed with the most fortunate qualities for uniting the culture of the sciences to that of letters, and spreading the charm of the former over the latter, from almost always making a splendid and useful application of his diversified talents.

The name of Condorcet was enrolled in the chief academies of Europe: he enjoyed a moderate affluence from the inheritance left him by his uncle, the Bishop of Lisieux: he occupied the place of inspector of the mint; but he resigned it during the second administration of Necker, whose financial opinions were directly opposed to those of Turgot and his friends. In 1786 Condorcet married Mademoiselle Sophie de Grouchy, by whom he had one daughter.

XV. *Account of some singular atmospherical Phænomena in the River St. Lawrence in the Beginning of July* 1814.

To Mr. Tilloch.

SIR,—I SEND for insertion in your excellent Journal the following narrative, drawn up by a British officer of engineers, of some remarkable occurrences in his passage from Cork to Quebec. Your philosophical readers will not fail to notice the coincidence between the phænomena described below, and those which were observed at St. Vincent's and other islands of the West Indies upwards of a year ago. I am, &c.

A CORRESPONDENT.

"*Remarks on board Ship in the River St. Lawrence, distant about* 20 *Miles from the Bay of Seven Islands above the Island of Anticosti,* 3*d July* 1814.

"Yesterday morning at 6 A.M. the weather dark and cloudy, with a few drops of rain falling; winds high and variable, chiefly from the eastward, and through the day carrying all sail. The sails however of very little use, from a very heavy swell of the sea from the westward, which rendered the pitching of the ship very great, and nearly endangered the carrying away the masts and yards; towards evening the swell abating; during the day the clouds appeared to be coming with great rapidity from the northward. Horizon and atmosphere thick and hazy; at night the darkness excessive, and the masts and rigging scarcely visible from deck. About 9 P.M. a sort of dust or ashes commenced falling, and continued during the night. Towards the morning the whole atmosphere appeared red and fiery to a wonderful degree, and the moon then at the full not visible, and the appearance through the cabin windows and crystal lights on the deck singular in the extreme, as if surrounded by a mass of fire. The sea sparkling much, and in a manner not usual in these latitudes.

"At half past seven in the morning, candles lighted in the cabin, and the hour by a watch at nine, scarcely visible; the flame of the candle burning of a bright blueish-white colour; and the fire in the cook house the same; the wind dying away to a dead calm. Towards noon to-day the atmosphere resumed something of its natural appearance, and the sun visible, but red and fiery, as in the winter season, as if seen through the darkened glass of a quadrant; and by degrees becoming more of a yellow colour. Weather hazy and sultry, a dead calm, and the sea scarcely agitated;—the sea covered with ashes, and a bucket of water taken up appeared nearly as black as writing ink, from the quantity of ashes which had fallen; they appeared as if those of burnt wood, and not of a heavy sandy nature; a strong smell perceptible in the air, and violent head-ache complained of by many on board.

"Not having a thermometer on board, the temperature could not be observed: it did not, although close and sultry at times, appear to be remarkable for the season of the year. Numbers of small birds flying about, seemingly much disturbed. The darkness at 8 A.M. to-day, as great as is usual in London in the month of December at the same hour.

"From the darkness during the night, the seamen were obliged to use lanterns with candles on deck to conduct the navigation of the ship.

"Longitude 65° 48′ west, and latitude 49° 49′ N.

"4th July.—This day the ashes falling in a small quantity; and the darkness last night excessive again, so much so that the hand could not be observed while touching the face; at half past 3 P.M. scarcely able to see the hour by a watch.

"The ashes collected on deck appear to be those of burnt wood, darker

darker and more heavy than the ashes from a tobacco pipe; that collected from the surface of the sea, when dried, resembling a cake of shoe blacking. Several ships in different quarters of the Gulph and River St. Lawrence observed the same appearance of darkness, which appears to have been pretty general, although not to the same degree.

"A frigate coming up, the Captain in his cabin had candles to breakfast at half past eight. No reason can as yet be assigned for this extraordinary phænomenon. It is conjectured by many to be in consequence of a volcano; but the ashes by no means resembled those thrown up by the volcano in St. Vincent, West Indies, some years since; some of which the writer of this has seen. Considerable fogs and hazy weather had prevailed for some time, during which it was supposed the Leopard man of war was wrecked on the Island of Anticosti."

XVI. *An Account of some new Experiments on the fluoric Compounds; with some Observations on other Objects of chemical Inquiry.* *By Sir* H. Davy, *LL.D. F.R.S. V.P.R.I.**

In this paper I shall offer to the Society a continuation of those researches, the details of which have been already honoured with a place in their Transactions; and I trust, that the experiments and observations which I have to communicate will be found to elucidate some important but obscure parts of chemical philosophy.

In the last paper which I had the honour of presenting to this body, I have given an account of a number of experiments made with a view of decomposing the fluoric acid: the most probable inference, from my results, was, that the pure liquid fluoric acid consists of hydrogen united to a substance which, from its strong powers of combination, has not as yet been procured in a separate form, but which is detached from hydrogen by metals, and which, in union with the basis of the boracic acid and silica, forms the fluo-boric and silicated fluoric gases.

All the new experiments that I have made on the fluoric compounds tend to confirm this idea; and the various attempts that I have made, since the last session, to decompose the principle in the fluoric acid separated at the negative surface in Voltaic combinations, have been unsuccessful.

I have found that fluate of lead, the substance formed by the action of a soluble salt of lead on fluate of potassa or fluate of ammonia, is immediately decomposed, when moist, by ammoniacal gas, and a white powder separates from it, which must contain oxygen, as it gives carbonic acid by being ignited with

* From the Philosophical Transactions for 1814, part i.

charcoal

charcoal; but dry fluate of lead may be fused in ammonia without undergoing the slightest alteration; and the presence of water, which may furnish oxygen to the lead and hydrogen to the fluoric principle, seems absolutely necessary for the production of oxidated bodies from the fluoric combinations.

I obtained analogous results by acting on silicated fluate of ammonia and fluo-borate of ammonia by chlorine: when the salts were moist, or when the gas was not free from vapour, silica and boracic acid were formed in small quantities; but when water was carefully excluded, these bodies did not appear; and the results were muriate of ammonia and silicated fluoric gas, or fluo-boric gas and azote.

I ignited two points of charcoal intensely in fluo-boric and silicated fluoric acid gases, pure, and mixed with oxygen; but no change, indicating a decomposition, took place; the only new product was a little inflammable gas, which was probably disengaged from the charcoal.

I passed pure liquid fluoric acid over charcoal ignited to whiteness, in a platinum tube; no carbonic acid was formed, and a very minute quantity of gas only was produced, which proved to be hydrogen.

I mentioned in my last communication to the Society, that I had made several experiments on the composition of the fluates: since that time I have repeated some of the processes, and I shall detail such of the results as appear to be most correct. In experiments on the fluates, it is very difficult to exclude sources of inaccuracy; glass vessels cannot be used, and even silver and platinum vessels are slightly acted upon by the pure fluoric combinations soluble in water.

Fluor spar decomposed by sulphuric acid increases more than three-fourths of its original weight; but to produce this result several successive distillations of it with fresh portions of acid are necessary, and the spar must be in very fine powder, and the result must be powdered after every operation.

In an experiment made in a crucible of platinum, in which very pure white Derbyshire spar was used, and sulphuric acid distilled in glass vessels, and in which the product was heated to whiteness in every operation, 100 grains became in the first operation 159·4 grains, which gained

in the second	6·9
in the third	4·2
in the fourth	1·9
in the fifth	1·6
in the sixth	0·9
in the seventh	0·3
in the eighth	–·0
Total increase	75·2

If

If this result be calculated upon, supposing the number representing calcium to be 40, as I have given it in my Elements of Chemical Philosophy, the number representing fluorine will be 34·2, and fluor spar must be supposed to be composed of 40 calcium and 34·2 fluorine.

Twenty-two grains of fused subcarbonate of potassa decomposed by diluted liquid fluoric acid, in an experiment made with great care, was found to afford 18·15 grains of dry fluate of potassa; and by a very careful analysis, it was found that this subcarbonate contained 31 per cent. of carbonic acid: now, if the remainder of the salt be supposed to be pure potassa, and the calculations be made on this idea, it will appear that fluate of potassa must consist of 125·9 of potassium and 54·74 of fluorine, and the number representing fluorine must be regarded as 32·6.

The 18·15 grains of fluate of potassa decomposed by sulphuric acid afforded 38·5 of acid sulphate of potash. Supposing acid sulphate of potassa to consist of two proportions of sulphuric acid 150, and one of potassa 90, they ought to have given 40 grains, and this loss is no more than might be expected in the process of decomposition and evaporation.

In some experiments that I made on the decomposition of the hydrates of potassa and soda, when decomposed by fluoric acid, the results indicated a number for fluorine a little lower. Thus 20 grains of hydrate of potassa were converted into 19·8 grains of fluate of potassa, and 20 grains of hydrate of soda into 19·6 of fluate of soda: but I do not place so much confidence in these results, as there always was great heat produced during the action of the acid upon the hydrates; and probably a minute quantity of the hydrates might have been dissipated at the beginning of the process.

It appears reasonable to conclude, as I have stated in my last paper, published in the Philosophical Transactions, that the number representing fluorine is less than half of that representing chlorine, about 33.

I endeavoured to ascertain the composition of fluate of ammonia, by adding together hydro-fluoric acid and solution of ammonia of known composition; and I found in this way, that 100 parts of solution of ammonia of specific gravity 9162 required for its saturation 52 grains of diluted fluoric acid of such a strength, that an equal portion produced exactly 32 grains of fluate of potassa. According to this experiment, fluate of ammonia must consist of 9·7 of fluorine to 22 of ammonia*; and if it be conceived that liquid fluoric acid consists of two pro-

* See Elements of Chemical Philosophy, page 268.

portions

portions of hydrogen to one of fluorine, the true composition of fluate of ammonia will be one proportion of fluoric acid 35, and two proportions of ammonia 64*.

The volatility of fluate of ammonia rendered it impossible to ascertain by evaporation the real quantity of solid salt formed: though the heat was never raised so high as that of boiling water, yet only 12·7 grains of solid fluate of ammonia could be procured.

Two hundred cubical inches of ammonia, which weigh 36 grains, condense one hundred cubical inches of silicated fluoric gas, weighing, under the same circumstances, 110·7; and if it be supposed that silicated fluate of ammonia contains one proportion of silicated fluoric acid and one of ammonia, then the number representing silicated fluoric gas will be 98·4, and it may be conceived to consist of two proportions of fluorine 66, and one of the siliceous basis 32·4.

According to the experiments of John Davy, 100 parts of silicated fluoric acid afford 61·4 of silica when decomposed by aqueous solution of ammonia: hence silica may be conceived to consist of 32·4 of basis and 29 of oxygen, and this is very near two proportions of oxygen.

I decomposed 20 grains of silicated fluate of ammonia by solution of ammonia, and I obtained 9·2 grains of silica. This result offers proportions very little different from those gained in the preceding calculations.

I have made some direct experiments with the hopes of determining the proportions of oxygen in silica; but they have been unsuccessful. I have ascertained, however, that it requires more than three parts of potassium to decompose one part of silica; which shows that this substance cannot contain much less than half its weight of oxygen.

I have endeavoured to separate the siliceous basis in a pure form, with the view of making synthetical experiments on its nature by combustion in oxygen; and my results, though not perfectly satisfactory, yet seem worthy of notice, and may lead to more successful attempts.

I decomposed silica by passing potassium in excess through it, in a heated tube of platinum: the result consisted chiefly of alkali containing a dark-coloured powder, the basis of silica diffused through it. I fused the whole mass with sulphur, which, in combining with the dry alkali, produced ignition. I attempted to detach the sulphuret of potassa by water: in this case the dark particles separated; but during their separation, and after, they acted upon the water of the solution producing

* That is, if ammonia be regarded as composed of one proportion of azote 26, and six of hydrogen.

gas,

gas, and, in attempting to collect them by the filter, I failed to procure sufficient for examination, for they were principally converted into silica.

I heated the substance procured in another experiment of this kind with hydrate of potassa; in this case there was a copious effervescence, and silica appeared to be reproduced and dissolved by the alkali.

I heated a portion of a similar result in strong lixivium of potassa; the solution gained a tint of olive, but there was scarcely any effervescence: from this it seems probable, that the inflammable basis of silica, like boron, is soluble in alkaline solutions without decomposing them.

Indeed this body, in its general characters, appears very analogous to boron. It appears to be neither volatile nor fusible; its oxide exerts, like boracic acid, a neutralizing power on the alkalies, though of a feebler kind, and forms, like boracic acid, vitreous bodies with the alkaline earths; and, like boron, the siliceous basis in combination with fluorine constitutes a powerful acid.

In my first views of the nature of the boracic and siliceous bases, I thought it probable that they would both appear as metals, if they could be entirely freed from oxygen; but it now seems more probable, that they form a class by themselves, offering a kind of link in the chain of natural bodies, when arranged according to their analogies, between charcoal, and sulphur and phosphorus.

It seems worthy of an experimental inquiry, whether the siliceous basis may not be obtained pure by heating the result procured from silica by potassium with pure sulphuric acid, which might possibly detach the potassa to form acid sulphate of potassa, without being decomposed by the inflammable basis.

I have made many new experiments with the hope of decomposing chlorine, but they have been all unavailing; nor have I been able to gain the slightest evidence of the existence of that oxygen which many persons still assert to be one of its elements.

I kept sulphuret of lead for some time in fusion in chlorine; the results were sulphurane (Dr. Thomson's liquor) and plumbane (muriate of lead); not an atom of sulphate of lead was formed in the experiment, though, if any oxygen had been present, this substance might have been expected to have been produced.

I heated plumbane (muriate of lead) in sulphurous acid gas, and likewise in carbonic acid gas, but no change was produced: now, if oxygen had existed either in chlorine, or in its combination with lead, there is every reason to believe, that the attractions of the substances concerned in these experiments would have been such as to have produced the insoluble and fixed salts of lead, the sulphate in the first case, and the carbonate in the second.

I shall not enter into any discussion upon the experiments in which water is said to be produced by the action of muriatic gas on ammonia: there is, I believe, no enlightened and candid person, who has witnessed the results of processes in which large quantities of muriate of ammonia, made by the combination of the gases in close vessels, have been distilled, without being satisfied, that there is no more moisture present, than the minute quantity which is known to exist in the compound vapours diffused through ammoniacal and muriatic acid gases, which cannot be considered either as essential to the existence of the gases, or as chemically combined with them*.

One of the first experiments that I made, with the hope of detecting oxygen in chlorine, was by acting upon it by ammonia, when I found that no water was formed, and that the results were merely muriate of ammonia and azote†; and the driest muriate of ammonia, I find, when heated with potassium, converts it into muriate of potassa; which result would be impossible on the hypothesis of oxymuriatic gas being a compound of oxygen; for, if there was a separation of water during the formation of the muriate, the same oxygen could not be supposed to be detached in water, and yet likewise to remain so as to form part of a neutral salt.

If water had been really formed during the action of chlorine on ammonia, the result would have been a most important one: it would have proved either that chlorine or azote was a compound, and contained oxygen, or that both contained this substance; but it would not have proved the existence of oxygen in chlorine, till it had been shown that the azote of the ammonia was unchanged in the operation.

Some authors continue to write and speak with scepticism on the subject, and demand stronger evidence of chlorine being undecompounded. These evidences it is impossible to give. It has resisted all attempts at decomposition. In this respect, it agrees with gold, and silver, and hydrogen, and oxygen. Persons may doubt, whether these are elementary bodies; but it is not philosophical to doubt, whether they have not been resolved into other forms of matter.

* Dr. Henry found it very difficult to free ammonia from the aqueous vapour existing in it by hydrate of potassa; and probably the hydrated muriatic vapour which I have detected in muriatic acid gas, by a freezing mixture, is not decomposable by muriate of lime.

† Philosophical Transactions for 1810.

By

By the same mode of reasoning, as that in which oxygen is conceived to exist in chlorine, any other species of matter might be supposed to form one of its constituent parts; and by multiplying words all the phænomena might be satisfactorily explained. Thus, in the simple view of the formation of muriatic acid, it is said one volume of chlorine combines with one of hydrogen, and they form two volumes of muriatic acid gas. In the hypothesis of chlorine containing oxygen, it is said, the oxygen of the chlorine combines with the hydrogen to form water, and this water unites to an unknown something, or dry muriatic acid, to produce a gaseous body. If it were asserted that chlorine contained azote, oxygen, and this unknown body, then it might be said, that in the action of hydrogen on chlorine, the azote, the oxygen and the chlorine, having all attractions for hydrogen, enter into union with it, and form a quadruple compound.

Professor Berzelius has lately adduced some arguments, which he conceives are in favour of chlorine being a compound of oxygen, from the laws of definite proportions; but I cannot regard these arguments of my learned and ingenious friend as possessing any weight. By transferring the definite proportions of oxygen to the metals, which he has given to chlorine, the explanation becomes a simple expression of facts; and there is no general canon with respect to the multiples of the proportions in which different bodies combine. Thus, azote follows peculiar laws in combining with every different body; it combines with three volumes of hydrogen, with half a volume of oxygen, with 1·2 and 1½ of the same body, and with four volumes of chlorine.

The chemists in the middle of the last century had an idea, that all inflammable bodies contained phlogiston or hydrogen. It was the glory of Lavoisier to lay the foundations for a sound logic in chemistry, by showing that the existence of this principle, or of other principles, should not be assumed where they could not be detected.

In all cases, in which bodies support combustion or form acids, oxygen has been supposed by the greater number of modern chemists to be present; but as there are many distinct species of inflammable bodies, so there may be many distinct species of matter which combine with them with so much energy, as to produce heat and light; and various bodies appear capable of forming acids: thus hydrogen enters into the composition of nearly as many acids as oxygen, and three bodies, namely, sulphuretted hydrogen, muriatic acid, and fluoric acid, which contain hydrogen, are not known to contain oxygen. The existence of oxygen in the atmosphere, and its action in the œconomy

of

of nature, and in the processes of the arts, have necessarily caused it to occupy a great portion of the attention of chemists; and being of such importance, and in constant operation, it is not extraordinary, that a greater number of phænomena should be attributed to it, than it really produces.

In the views that I have ventured to develop, neither oxygen, chlorine, or fluorine, are asserted to be elements; it is only asserted that, as yet, they have not been decomposed.

As the investigation of nature proceeds, it is not improbable, that other more subtile bodies belonging to this class will be discovered; and, perhaps, some of the characteristic differences of those substances, which apparently give the same products by analysis, may depend upon this circumstance.

The conjecture appears worth hazarding, whether the carbonaceous matter in the diamond may not be united to an extremely light and subtile principle of this kind, which has hitherto escaped detection, but which may be expelled, or newly combined, during its combustion in oxygen. That some chemical difference must exist between the hardest and most beautiful of the gems and charcoal, between a non-conductor and a conductor of electricity, it is scarcely possible, notwithstanding the elaborate experiments that have been made on the subject, to doubt: and it seems reasonable to expect, that a very refined or perfect chemistry will confirm the analogies of nature, and show that bodies cannot be exactly the same in composition or chemical nature, and yet totally different in all their physical properties.

XVII. *Official Report of a Fall of Aërolites near Grenade, seven Leagues to the N.N.W. of Toulouse, on the* 10*th of April* 1812. *By M.* D'Aubuisson, *Chief Engineer of Mines in France*.*

On the 10th of April 1812, the air was colder than it had been for a few days; the thermometer marked only 5° about eight in the evening, when the phænomenon took place: it had rained a great part of the day, and the sky was almost entirely covered with thick clouds.

At the above hour a brilliant light was seen in the atmosphere at Toulouse, and for several leagues around: this was followed by a very loud detonation. It was thought at first that the powder magazine of Toulouse had been blown up; and when it

* *Journal des Mines*, vol. xxxi. p. 419.—The interruption of our communications with France prevented our laying this paper sooner before our readers.—Edit.

was

was discovered that this was unfounded, the light and noise were ascribed to some extraordinary meteor; for the state of the atmosphere and the force of the explosion did not admit of the idea of its being a simple clap of thunder. A few days afterwards it was discovered that this phænomenon had been accompanied with a shower of stones two leagues W.N.W. of Grenade, in the commune of Burgave, (department of the Upper Garonne,) and of Camville and Verdun (department of the Tarv and Garonne). Some specimens were sent to the prefect of the Upper Garonne, and this magistrate appointed a commission composed of M. Saget, of the Academy of Sciences; Marqué-Victor, Professor of Physics; Carney, Professor of Mathematics; and myself, in order to proceed to the spot and collect the details of the phænomenon. We accordingly gained the following information:

Circumstances observed in the Atmosphere.

The light which spread over the atmosphere appeared all at once. Although the sun had set for an hour and a half, and the air was dark, the light was so brilliant that the Mayor of Grenade informed us that he could read the smallest characters in the streets of the town; and the Mayor of Camville compared it to the light of the sun; adding, that the town clock was as visible as at noon day, and that a pin might have been picked up from the streets.

The exact duration of this light was not remarked. Some persons estimated it at two minutes; others at one, and others still less; but the fact is, that the light was continuous, and not instantaneous like that of lightning.

The sky around being dark, the body which produced this light could not be seen. Scarcely had it disappeared in the place where the aërolites fell, when there were heard in the air three strong detonations similar to the report of large pieces of cannon: they succeeded each other rapidly, and almost without any interval.

Their noise was such that they were heard at Castres, twenty leagues from the spot where the stones fell. They were distinguished from each other in the neighbourhood of this spot alone; some persons informed us that they were of equal strength; others said that their intensity gradually diminished.

We attempted in vain to collect information as to the interval which elapsed between the light and the detonations, in order to obtain some data as to the distance and height at which they had taken place.

They were followed by a very loud noise, which some compared to that of several heavy carriages rolling at once upon the

 pavement;

pavement; others compared it to the sound of several drums; and others to a strong fire of musketry from the Spaniards having invaded the country.

The noise was heard not only where the stones fell, but also at Grenade and Toulouse; it was heard for nearly a quarter of an hour, according to some persons: but although this is of course exaggerated, I cannot help thinking that it lasted a minute at least.

It came from the N.E. and proceeded to the S.E. The peasants who gave us the most correct information, heard it pass over their heads towards Toulouse, and gradually cease in that direction.

After the rolling noise had passed over the ground situated between the farms of la Bordette and la Pradere, a sharp hissing noise was heard, which ended in considerable shocks, similar to grape shot striking the ground: these phænomena were produced by the fall of the aërolites.

Circumstances observed on the Ground.

I now give the information received, as to the aërolites which were collected or heard to fall.

1. The inhabitants at the little farm called la Bordette distinctly heard two aërolites fall; one to the northward in a field adjoining, which they have not yet found: the other was found about 50 paces to the south-east: the fragment which we have weighs three ounces, and the whole stone did not weigh six.

2. At the cottage called Paris (300 metres above Pemejan) the inhabitants were at the door listening to the rolling noise over their heads, when they heard the noise of a body which fell in front of them. The master of the house then went back through the house to shut the door of a stable, and when there he heard a second large body fall. The interval between the two must have been about 75 seconds. This fact is of importance.

3. At Pemejan, the inhabitants, equally alarmed at a stone which fell near them, took refuge in the house; when they heard a second hissing sound, followed by the noise of a body falling on the roof. Next day they found a tile broken, and a stone weighing about three ounces resting on the lath. Having carefully examined this spot, I found no contusion, nor any mark of fire, upon the wood of the roof. In the vicinity of the farm two stones were found which weighed a few ounces only.

4. At Richard, after the rolling noise, an explosion was heard in the air, and next day a stone weighing eight ounces was found.

5. At

5. At Pradere, there fell, about a pace from the house, with considerable noise, and more than a minute after the detonations, an aërolite weighing two pounds. It was not entirely sunk in the earth, and was not perceived until two days afterwards. A few seconds afterwards a smaller stone fell 40 paces in front of the house.

We did not learn for certain, notwithstanding some information to that effect, that any aërolites fell beyond the limits marked by the places which we have mentioned.

The quantity of the aërolites which actually fell must not be inferred from the small number which was collected. It was night when they fell, and most of the inhabitants were in bed: the ground on which they fell was partly in grass and partly ploughed up: into the latter many were no doubt sunk, so that it is more than probable, that a much more considerable quantity fell that what is mentioned above: in short, all that were collected fell close to the houses, and of course were easily discovered. The whole that fell, therefore, may fairly be estimated at upwards of 100; nay, perhaps at 1000.

Mineralogical Character of the Aërolites.

They were so like each other that the description of one will suffice. They were from three to eight ounces in weight, and one only weighed two pounds. They consist of a homogeneous paste of a stony nature, containing a very great quantity of small particles of iron in the metallic state, and very malleable. They do not affect any particular form. Their surface presents every where softened and rounded angles, nearly like those of a body which had begun to fuse. It is a very thin crust, like a coarse varnish or superficial coating; sometimes however it is thicker, but rarely exceeds a quarter of a line. It is of a blackish-brown colour.

The interior is of an ash gray. It becomes deeper, and exhibits spots of yellow ochre, when exposed to the contact of the air. The fracture is earthy with coarse grains, or rather granulous like freestone: it is rough to the touch.

With the exception of the metallic points, it is entirely dull: rasping makes it shine a little, in consequence of the ductility of the iron, as we shall soon see.

These aërolites are easily broken and easily pulverized. Their fall even upon soft earth has broken most of them. They are *semi-hard* (*i. e.* they scratch glass). The crust only is *hard*, and gives some sparks with steel.

The specific gravity of six specimens varied only from 3·66 to 3·71.

 They

They are absolutely opaque, sour, do not stick to the tongue, and do not sensibly absorb water in which they are plunged.

Thin fragments, when exposed to the blow-pipe, became black, and were covered with black globules at some points: their surface was in this state similar to the crust of entire aërolites.

The iron was in considerable quantities, being more than a third of their weight: it is not however very conspicuous to the naked eye, on account of the smallness of the particles; but they are evident when the specimen is rubbed or polished. The iron, which is very malleable, is then spread by the hard body, and forms a small scale or stud: like a piece of bright lead. This metallic aspect is particularly visible at the surface on the spot struck by the steel. The surfaces exposed by the lapidary's wheel present, on a gray ground, small metallic spots, and remind us of certain jaspers, containing dendrites of silver, which are found polished in mineralogical cabinets.

The great quantity of iron contained in these aërolites renders them of course very susceptible of the magnetic influence. But they exhibited no traces of polarity; and the phænomena of attraction only, but none of repulsion, were ever exhibited.

Observations.

I shall hazard no conjectures, but only remark that three facts conspicuously arise from the foregoing descriptions:

1. The space or district occupied by the aërolites of Grenade, is too limited to suppose that the point at which they were separated from each other was very high above the surface of our globe. They seem to have been fragments of a large mass which passed from N.W. to S.E., and which was broken, several times perhaps, at the time of the successive detonations which were heard in the atmosphere. The strength of the latter seems to indicate that they took place in a very dense medium, and consequently at a small height.

2. After their separation, the fragments of this mass must have undergone a heat capable of fusing their surface: for each on reaching the ground was entirely covered with a varnish, or blackish coating, evidently produced by fusion.

3. The light seen in the atmosphere seems to have been an effect of the deflagrations of the *aërolitic mass;* for it appeared suddenly like lightning, and was followed, or rather accompanied, by detonations, the interval between the light and the noise being undoubtedly the mere effect of distance. This light must have commenced by being very feeble, and must have gradually increased afterwards, if it had been produced by the arrival of a luminous meteor in our atmosphere.

XVIII. *De-*

XVIII. *Description of a practical Diagram for obtaining in a simple and easy Manner the Correction of the Lunar Distances as observed by a Sextant. By Lieut.* JAMES BULKELEY, *of the Royal Navy.*

To Mr. Tilloch.

SIR,—I TAKE the opportunity of sending you a description of a practical diagram, for obtaining in a simple and easy manner the correction of the lunar distances as observed by a sextant, which is submitted to your approbation and opinion for publication. I am, sir,

Your obedient servant,

Huntley-Hall, Cheadle, Staffordshire, June 6, 1814. JAMES BULKELEY.

Description. [Plate II. fig. 1.]

AB, a semicircle divided into 180 degrees or equal parts; AC, the semidiameter graduated as a line of sines; CD, a flat bar moveable round the centre of the instrument, and ending with a nonius or pointer to the semicircle, and divided like the semidiameter as a line of sines; EF, a bar sliding at right angles to the semidiameter, and on a pivot, to enable it to fall either to the right or left of the semidiameter, and graduated as a line of chords terminating on the semidiameter with a nonius. HG, a small bar moving at right angles to the moveable radius CD, and cutting with a silk thread the perpendicular bar EF, as at I.

Method of using the Instrument.

Suppose the *apparent* distance to be 104° 30′; the sun's *apparent* altitude 43° 20′; and the moon's *apparent* altitude 12° 30′; horizontal parallax 57′ 49″: to find the true distance. With the moveable radius CD set off the apparent distance on the graduated semicircle AB 104° 30′ as per figure. Then with the perpendicular EF set off on the semidiameter AC, which is divided as a line of sines, the moon's apparent altitude 12° 30′, as per figure; and on the radius CD with the sliding perpendicular CH set off the sun's apparent altitude 43° 20′, as per figure: then will the silk thread on GH cut on the perpendicular EF, (which is divided as a line of chords, but the degrees reckoned as minutes, and the minutes as seconds) a number, as 44′ 00″ at I per figure; which if multiplied by the horizontal parallax, and (as the perpendiculars cut to the right of the semidiameter) divided by 62, will give a correction to be subtracted from the apparent distance, to find the true, as 44′ 00″ × 57′ 49″ horizontal parallax; and ÷ 62 = 42′ 26″ — from apparent distance 104°

104° 30′, gives the true distance 103° 57′ 34″. But should the perpendiculars cut to the left of the semidiameter, the sum multiplied by the horizontal parallax must be divided by 52; and then the correction is to be *added* to the apparent distance, to find the true distance.

N. B. This instrument may be made either in brass, wood, or with care in card. I made a model in card, with a radius of about six inches, which gave the true distance within 10″ or 15″ of it when worked by other methods.

XIX. *Remarks on Mr.* Hume's *Gazometer and Blow-pipe. By a* Correspondent.

To Mr. Tilloch.

London, August 13, 1814.

Sir,—Upon reading in your Journal for the preceding month, a paper by Mr. Hume, wherein he describes a method for constructing an improved blow-pipe, out of some chemical apparatus, possessing the peculiar advantage of yielding a current of atmospheric air by inhalation; a suggestion occurred to me, which upon trial was verified, that too great exertion of the lungs would be required to render the current of air of sufficient duration to be useful in chemical or mineralogical experiments.

The Woulfe's bottle I used for this purpose contained about three pints; and though it required considerable exertion to draw up the water into the separating funnel, yet the stream of air continued only three-quarters of a minute; a time certainly insufficient to make scarcely any experiment.

Now I am inclined to think that the cylindric gazometer, which is almost indispensable to the chemist, and is frequently used for this purpose, answers the end proposed by Mr. Hume more completely than his contrivance; because, merely by raising the interior cylindrical vessel, as many cubic feet of atmospheric air may be obtained in a given time, as by inhalation we could obtain inches; and with this additional advantage, that the force of the current of air may be increased or decreased at pleasure. Nevertheless, I think that Mr. Hume deserves great credit for endeavouring to increase the powers of the philosophical chemist, without increasing his apparatus, the expense of which already deters many from the pursuit of so pleasing and useful a science.

I am, sir,

Your most obedient servant,

W. W.

XX. *Me-*

XX. *Memoir upon the compound and smooth or simple Eyes of Insects, and on the Manner in which these two Species of Eyes concur in Vision. By M.* Marcel de Serres, *Professor of the Sciences in the Imperial University*.*

Visus igitur quo insecta gaudent nulla penitus ratione cum nostris oculis aut cum camera obscura in qua rerum species, reflexionis ope, super charta aut panno albicante pinguuntur in comparationem venire potest.
Swammerdam, *Biblia Naturæ*, tom. i. p. 502.

Sight seems to be one of the most perfect of the senses of insects; and in this respect these animals are in the first line among those without vertebræ as birds are among those with vertebræ. Both enjoy a very long sight; and we know at how great a distance the maggot, like the bird of prey, perceives the object which it wishes to devour. Is the great quantity of air inhaled by these two orders of animals in the act of respiration, the cause of the acuteness of this sense, as it is of their muscular strength? or must we account for it from the great development of their retina and the largeness of their eyes? However difficult it may be to resolve a question abounding so much with difficulty, it is probable that insects and birds owe equally to the great abundance of air which they inhale, the acuteness of their senses, and the activity of their digestion; finally, the violence of their passions, if we may so express ourselves.

Far different from the vertebral animals in the structure of their eyes, insects enjoy the impression of external objects by means peculiar to themselves, and which furnish proofs that nature knows how to attain the same end by very opposite ways. In fact, the operation of vision is not performed by insects in the same way as with most animals by the action of the luminous rays, which, after passing through the pupil, collect on the retina, but rather by the shaking of the optic nerves occasioned by the light which passes through the cornea. These eyes are constructed so as to receive the images of objects by the simple shock of the rays which these objects reflect, and this method of feeling must necessarily be very acute. Besides, insects not having, like the vertebral animals, pupils susceptible of contracting, it would seem that vision ought to be very perfect with them, on account of the great number of rays which fall continually on the facets of their eyes.

Nevertheless, one of the great inconveniences which results from the organization of insects, is the kind of immoveability of the parts in which their eyes are fixed. But nature has remedied whatever is unfavourable in this organization, by rendering the eyes of insects very much complicated, and by multiplying their facets in such a way that one

* *Magasin Encyclopédique*, Feb. 1814.

and the same eye presents as many as fourteen thousand, as Hooke observed in the *libellulæ**. Nature has even sometimes multiplied the number of the eyes themselves, and this number seems to be always in the ratio of the size of these very eyes, and the immoveability of the parts in which they are situated. Thus, when the compound eyes are entirely wanting, the number of the flat or simple eyes is greatly increased, and certain species have thus even six pair of eyes. This is observed in some of the apteræ, many of the larvæ, and particularly in the spider. Indeed, the latter should be separated from the insects, as Messrs. Lamarck and Cuvier have already done; for their organization presents some very marked distinctions, and of the first importance. In fact, the spider like the scorpion has a kind of lungs, and consequently a real heart with blood-vessels; whereas in the real insect the organs of respiration are always ramified, and the vascular system, or at least what has been taken for it, never is. Sometimes, however, the eyes of insects are not situated in parts entirely immoveable, and certain species present a kind of neck, which Comparetti † seems to have been the first to observe, and which permits the head to execute a variety of movements. This neck is very conspicuous in some of the carnivorous species, which not being able to pursue their prey on account of the disproportion of their organs of movement, require the faculty of seeing to a great distance. This organization is very evident in the *mantis*, the *empusa*, and the *mantispa*, in which the head may be directed to one side, or backward, and may even be twisted almost round. It may be remarked in general, that the eyes are the more convex and the more salient, the more decidedly carnivorous the insect is, as the mobility of the parts in which they are situated, is always relative to the kind of life and to the habitudes of the insect. In short, we never saw the eyes of these animals present any kind of movement, and they always adhere to the parts where they are situated, and consequently are completely immoveable.

The sight is exercised by insects by two organs very different from those which perform this function in the vertebral animals and the *mollusci*: they exhibit even very striking differences from each other, as well in respect of their external conformation, as in their internal disposition. Some appear to be a large cluster of eyes united together, if we may so express ourselves, and others exhibit one only. The former have been called *compound eyes*, on account of their arrangement; and the latter

* Swammerdam, *Biblia Naturæ*, tom. i. p. 490, and *Collection Académique*, p. 323. Hooke has given the eyes of the *Libellulæ* in his *Micrographia*, plates xxiii and xxiv.

† *Dinamica Animale*, part i. p. 103 to 106.

smooth

smooth or simple eyes, on account of their simplicity. These two kinds of eyes concur to the same object, and are sometimes united on the same individual: thus, the *orthopteræ*, like the *hymenopteræ*, present at once both compound and simple eyes, a disposition which is also observed in various kinds of *hemipteræ*, *neuropteræ*, and *dipteræ*. The other classes present only a single kind of eye; but when there are only smooth eyes, their number is always considerable, probably in order to make up for their imperfection. In general, all winged insects have compound eyes, and this law does not seem to present any exception. In the *apteræ*, certain kinds present only compound ideas, such as the *cloportæ:* in truth, the latter seem formed by a collection of simple eyes. Other kinds have only simple eyes, such are the *juliæ* and the *scolopendræ*, which present a certain number of simple eyes; but all these animals appear to me so different from the insect tribe in point of organization, that I have thought it right to pass them over.

With respect to the larvæ, those of half-metamorphosed insects uniformly have eyes similar to those of the perfect insects; whereas the larvæ of insects wholly metamorphosed have only simple eyes, which vary much in their number and position. The caterpillar has six or eight on the sides of the head. The false caterpillars, or the larvæ of common flies, have two only, like those of bees and the *stratyomæ*. Finally, a great many completely metamorphosed larvæ have no eyes at all.

We may also remark, that the species which exhibit both compound and simple eyes (which has never been observed but in perfect insects) are those which have most need of seeing far, either because by rising high in the air it is requisite that they should distinguish their prey at a certain distance, or because, having large flights to take, they should direct themselves with safety. In short, the insects of a great flight, like birds of prey, have very delicate and very extensive powers of vision.

But in order to give an exact idea of the two kinds of eyes of insects, we shall describe each in particular, commencing with the compound eyes, the organization of which is most complicated, and terminating our description by the simple eyes. We shall afterwards endeavour to ascertain in what way the faculty of vision is executed among this order of animals, when we have followed up all the differences of organization which they present with respect to their eyes.

1. *Compound eyes.* The compound eyes are generally situated on the lateral or middle parts of the head, sometimes even completely at the base of it, and a few are placed near the antennæ, or more or less laterally and outside of these parts.

Placed

Placed in orbitary cavities, they are protected not only by the sides of these cavities, sufficiently hard to oppose the impression of external bodies, and by their external membranes, which are almost as hard as the shelly envelope which covers the body, but also internally by soft parts which seem to consist of tracheæ only. In the animals with vertebræ, the eyes are besides protected by several soft parts like the cartilages, the membranes of the eyebrows and eyelids, which conceal them under a thick veil, and defend them from external objects. These parts, called by Haller *tentamina oculi*, are totally wanting in insects; and besides, after knowing the conformation of their eyes, of what use could they be?

We must however observe that some insects present in their cornea, and in general on the furrows which separate the hexagonal facets of this membrane, fine hairs, more or less long, and more or less thick. These have been regarded by Swammerdam and other anatomists, as the eyelashes of the eyes of insects, although they do not seem to perform their office: in fact, they appear to be so little essential that they are wanting in the greater number; and when they are observed, they are always arranged on the lower part of the eye, a position which is far from being the most favourable to guard it against the admission of foreign bodies. Besides, scarcely any but the most glutinous bodies can adhere to the convex or polished surface of the eyes of insects; and when they do, the animal can easily remove them with its fore legs. This is frequently observable in bees and flies, which take a pleasure in repeating the operation.

The situation of the eyes, or rather their position, is very variable in the different classes. As this position has a great influence in vision, we shall describe it in the chief families.

The most external membrane of the compound eyes is hard and transparent: it might therefore either be called the *sclerotica* on account of its hardness, or *transparent cornea* from its transparency. This last denomination agrees with it perfectly; for, as Swammerdam remarks, it has the flexibility, the firmness, and transparency of horn. This cornea or sclerotica, convex externally and concave internally, is formed by an infinity of hexagonal facets arranged with the greatest regularity alongside of each other. These facets, divided or separated by furrows which always follow the direction of the cornea, exhibit in some insects, the *hymenopteræ* in particular, hairs which resemble down. As to the lines or furrows of the facets of the cornea, they are curved and a little folded on account of the spherical convexity of the cornea, which interweaves in different places the hexagonal facets with the lines which separate them.

them. In a word, the whole cornea is a true hexagonal net-work, the internal surface of which is divided into as many hexagonal facets as there are at the external surface.

Swammerdam thought that the cornea received some tracheæ, and that these constituted the hexagonal meshes of the cornea. He seemed to think also, that these tracheæ could in the eyes of the *nymphæ* serve for their expansion and unravelling. As to the first of these opinions, it does not seem to agree with the organization of eyes in general; for it is very rare that the tracheæ penetrate through the choroid and reach the cornea; and I only know one example, and that is the *libellula vulgaris.* The second fact, being only a consequence of the first, cannot be admitted, if it be true that the tracheæ rarely ever reach the cornea.

The form of the cornea necessarily determines that of the eye, and seems to have certain relations with the manner of living of the species. Sometimes this form varies in its proportions, and that in the species of one and the same genus; but it seems as if the *sphericity*, or the more or less angular form, of this same cornea is little subject to vary. In general, the cornea is the more spherical and the more projecting in proportion as the animal is carnivorous, or when the eye is concealed under a flap of the eyelid, as we see in the *lampyræ* and others, where the sphericity of the eyes is so considerable that it comprehends almost the whole head. We may also observe that the smaller the compound eyes are, the greater is the convexity of the cornea. It is not the same with simple eyes, which in general vary little in point of size.

In order to give a precise idea of these variations, we have described them in a certain number of families, in order that their importance may be known. The cornea is in general cased in a cavity in the hard parts of the head. This union is so complete that it is frequently impossible to separate them: we might even doubt that any separation was possible, were it not for a small ring externally which marks the line of adhesion. When the cornea is entirely separated from all the parts situated below its internal face, as if from its covering, it seems white and brilliant, which renders it similar to horn. This horn is very thick in certain species. It is transparent if we examine it externally; but when it is not freed from the other parts situated below, it presents either bands or stripes of various colours, or a marbled appearance. It seems even completely black in a great number of species: most of the *coleopteræ, hymenopteræ*, and *lepidopteræ*, present this arrangement.

To conclude: this black colour and marbled tinge do not by any means belong to its texture, but depend, as may be easily ascertained,

ascertained, on the difference in thickness and the various colours of the humour which adheres to it.

Under the cornea is seen a conduit or pipe, not very liquid, not soluble in water, and strongly adhering to this membrane. Its colour in general is between the darkest violet and the deepest black. When this coating presents this colour, which is most frequently the case, it is almost impossible to distinguish it from the humour of the choroid. This is not the case in the eyes the coating of which is red or green, or of both colours united. We then see very distinctly the coating with its variegated colours, and the humour of the choroid with its black tinge which never varies. This arrangement is very conspicuous in the *locusta gigantea, lilifolia*, the *libellula vulgaris*, and the greater number of the *tabani*. It is also very striking in the *gryllus lineola*, the eyes of which appear to be streaked with brown and green bands, and which is indebted for this singularity to the coating of the cornea being alternately brown and green, and that by nearly parallel bands.

It is therefore to the mixture of the tints of the tunic of the cornea that the variegated colours presented to us by the eyes of insects are owing, precisely in the same way as from its various degrees of thickness and colouring the variegated stripes and marble tint arise which appear at the exterior part of the eye, and which might have been thought peculiar to the cornea. Frequently, as a consequence of this arrangement, one and the same eye presents spots and stripes of various colours, or even one side of a colour totally different from the other.

The coating of the cornea, therefore, covers all the internal surface of this membrane. Its thickness and consistence, like its opacity, are very much subject to variation, as already observed: but it would seem that, in general, the more this tunic is opaque, the thicker and broader are the nervous threads which pass through it, in order perhaps that its opacity may not be an obstacle to vision.

We are under the necessity of anticipating a little upon our description, and of speaking here of the optic nerves, which, furnished by the retina, pass through the choroid and its humour, as does the coating of the cornea, in coming into correspondence with the facets of the latter membrane. This arrangement is not the same in the species which present bands or stripes, and in those which want them. If we carefully remove the cornea, and in such a manner as to remove very little of its coating, we observe, in the species in which the compound eye presents stripes externally, this coating to be composed of very distinct rays, one of which is blackish and the other much deeper, and so on alternately. These two colours are far from being uniform; and however slightly

slightly we look at them, we soon perceive an infinite number of polygons, the centre of which appears to be white. All these white points are the extremities of the nervous filaments coming from the expansion of the grand optic nerve*, or from the retina, and which have traversed the choroid and its coating. We may convince ourselves of this in the following way: the brain being laid bare, we may follow the optic nerve, and see it directed to the eye, where it spreads, and is divided into a multitude of filaments. But if we draw these filaments, the white points disappear completely, and there only remain on the coating, streaks variously coloured; which proves that these white points are the extremities of the nervous filiaments. We may recognise this arrangement in the great species, such as the *libellulæ*, the *truxalæ*, and the common cricket. In the species which have no streaks on the cornea, we also observe on the coating of the cornea, the furrows or ridges which form the facets of this membrane, and the coating, which is more or less deep, having the form of a polygon, which represents the facets of the cornea.

These retinæ, peculiar to each facet, are those which Swammerdam has called the pyramidal of the eye. These compound fibres, according to him, proceed to a membrane as to a common centre, and it is the circular trachea from which the filaments issue: he also observes correctly, that it is through its substance that the tracheæ pass which ascend along the pyramidal fibres. The figure of these fibres is hexagonal, and their upper extremity is broader than their lower, spreading out a little, as it would seem, when they get into the concavity of the cornea, and taking the form of this membrane.

To conclude: Swammerdam states that he never was able to ascertain whether these fibres were muscular or nervous, although it would have been easy to convince himself of it, since they may be followed to the brain. As to the transverse fibres which Swammerdam has described†, I never perceived them. I am led to think that this expert anatomist must have made a transverse section in the eye and the brain, and that he formed these fibres by the section which he made in the optic nerve. At least, it is certain that these fibres communicate with the brain, for Swammerdam himself admits it: he remarks even that we might compare them to a very considerable nerve which we observe in the furrow, which nerve derives its origin from the brain.

* We designate the nerve which proceeds to the compound eyes, under the name of grand optic nerve, in order to distinguish it from the optic nerves which proceed to the simple or smooth eyes.

† *Biblia Naturæ*, tab. xx. fig. 5.

In order to observe these white points, or the extremities of the nervous filaments which compose the particular retinæ of each facet, the cornea must be carefully removed, operating its section from the outside to the inside, and taking care not to remove the coating; for, however little we disturb the filaments, they are so contractile, that they close in upon the optic nerve, and no longer appear on the coating of the cornea. If, instead of dissecting the eye from the outside to the inside, we observe it by successively removing the internal parts, we can never discover this arrangement, even in the genera where it is most decided; such as the *gryllus*, the *mantis*, the *libellula*, and the *tabanus*. This depends on the contractibility of the optic nerves, which is sometimes so great as to draw the filaments even beyond the choroid.

Immediately under the optic filaments and the coating of the cornea we observe the coating of the choroid. This coating or varnish is a viscous substance, not liquid, soft, very clammy, and not very soluble in water. It is also strongly adherent to the membrane which it covers when the latter exists. Swammerdam remarks with great truth, that this opaque varnish stains the fingers like the common *pigmentum nigrum*. But this accurate observer seems to have confounded the coating of the cornea with the varnish of the choroid, when he says that the latter is variously coloured according to the species. On the contrary; the black colour and the opacity of the varnish of the choroid are established beyond contradiction. This colour even seems to be internally connected with the texture of the choroid, for it is impossible to discharge it by repeated macerations. Contrary to the opinion of Hooke*, Swammerdam thinks that there does not exist in the eyes of insects any humour properly so called; and in fact, neither the tunic of the cornea nor the varnish of the choroid deserves the name of humour, particularly if we compare them to what is understood by the term humour in the eyes of vertebral animals. Finally, the same anatomist regards the blackish varnish of the choroid, as the extremity of certain fibres placed immediately under the cornea, fibres which have been torn on removing this membrane. This explanation appears very improbable, since, when the tunic of the cornea is of a colour different from the varnish, there never exist any blackish fibres corresponding with this same cornea. Besides, how could the

* Hooke and Boyle were the first who maintained that air was necessary to combustion and respiration, and that those operations consume but a certain portion of it. Hooke even conjectured that air was fixed in nitre, and that combustion was a chemical process, *i. e.* the solution of the burning body in a fluid, or its union with this substance. The chemists of the present day hold no other language. *Micrographia*, pp. 45. 104, 105.

the colour of this varnish be always the same with that of the choroid? This single fact demonstrates, in my opinion, an analogy between the choroid and its varnish: it would even seem that the latter is produced by a kind of transudation which takes place through the meshes of this membrane itself.

If the varnish of the choroid be formed by a transudation of this membrane, it is evident that this varnish ought always to cover it, and this is precisely what is observed. But this varnish, as well as the choroid itself, does not always exist: it would seem even that all the species which shun the light are totally deprived of it. At least I have not observed it in the *blaps*, the *pedinus*, and most of the *tenebrios:* it is wanting beyond doubt in the *blattæ*, which, as is well known, are stupefied with the light of the sun. When the varnish and the choroid exist, it is not very difficult to separate this membrane from the varnish. It seems then to be most generally cellular; and in certain species, the longitudinal fibres of which it is composed are sufficiently decided to make it appear slightly streaked, on account of the tracheæ which are distributed over it: this is distinctly observable in the *truxalæ*. This arrangement has been well described by M. Cuvier in his Memoir on the Nutrition of Insects, p. 42, tab. i. fig. 3. This organisation might induce a belief that the choroid is formed by the prolongation or union of the small tracheæ furnished by the large circular trachea: at least, all the tracheæ which proceed to this membrane are totally lost in it; and as we cannot recover them, after they reach it, we ought to regard them as contributing to form the choroid. This membrane is therefore composed of a cellular texture tolerably close, on which there exists a collection of tracheæ furnished by the large circular trachea, and which are imbued with, or rather deeply penetrated by, a blackish varnish. The choroid is more or less black, but always opaque; and its sombre tint is as constant as its opacity. Finally, the longest macerations cannot make it lose its colour or opacity, as we have already observed.

The choroid of the compound eyes of insects has been regarded by Swammerdam as an uvea. It would seem, however, that it cannot be called an uvea, as it does not occupy the bottom of the eye of insects. However this may be, this membrane is attached by its circumference to the whole of the edge of the cornea, and consequently follows the contours of this same membrane. It is surrounded by a large circular trachea, in general furnished by the tracheal artery of Swammerdam, but varying as to its arrangement in the different genera. For instance, in the *gryllus* and *truxalis*, it is the third principal division of this same trachea situated in the head, which, when it arrives at the eye, becomes bifurcated, and the two tracheæ

 which

which result fasten on the edges of the eye: the latter form the large circular trachea, unite afterwards to the upper part, continuing after this junction in one single trachea, which, by joining to another, afterwards terminates at the base of the brain.

The large circular trachea furnishes an infinite number of very minute tracheæ, which soon becoming bifurcated, form very numerous isosceles triangles which rest on the circumference of the optic cone. These triangles formed by the tracheæ are divided as by a perpendicular, by the nervous filaments resulting from the expansion of the optic nerve. These filaments afterwards pass through the choroid and its opaque varnish, as well as the tunic of the cornea, and terminate below the facets of the last-mentioned membrane. This arrangement of the tracheæ and of the nervous filaments forms a handsome network, which is rendered very sensible by carrying the nerve inwards, and on the side of the brain. All these tracheæ afterwards continue, and terminate on the choroid. The genera which have no choroid also want the circular trachea.

In the genera which have vesicular tracheæ, like the *lamellicorn coleopteræ*, most of the *lepidopteræ* and *dipteræ*, as well as certain *orthopteræ*, like the *gryllus* and the *truxalis*, we observe considerably under the optic cone another circular trachea, but much smaller than that which surrounds the edge of the cornea. The latter turns around the optic nerve, and is surrounded itself by numerous air pouches, the use of which seems to be, to sustain the optic nerve and to keep it in its position.

The small circular trachea is wanting in all the genera which do not present pneumatic pouches or vesicular tracheæ: as it appears, however, essential for keeping the nerve in its position, it is replaced by the fibres of the adductor muscle of the mandibles, which on separating wholly surround the optic nerve, and prevent its being put out of place. We cannot say that the muscle in its contractions can act on the nerve by compressing it; for observation proves, that by placing the muscle in all the contractions of which it is capable, the nerve remains always in its natural position, since the contraction of the muscle is effected longitudinally only, and from front to rear; so that, whatever contraction it undergoes, it can never touch the optic nerve.

The optic nerve formed by the prolongation of the brain is, of all the nerves of the head, the largest and broadest, particularly if we measure it at the place where it spreads. It issues almost always from the lateral and upper surfaces of the brain; but its position with respect to the other nerves furnished by the brain is very variable. According to the species and position of the various parts situated in the head, it is either the third, the

the fourth, or the fifth pair of nerves furnished by this organ. The optic nerve at its origin is a little cylindrical, and, directing itself laterally, enters soon after its origin into the small circular trachea when it exists; and, when it does not exist, between the filaments of the adductor muscle of the mandible, which forms a kind of circular aperture for its passage. Gradually this nerve expands, and forms a cone, which has its base on the cornea and its summit on the brain. This expansion is greater or less according to circumstances. The *libellulæ*, the *lamellicorn coleopteræ*, most of the *lepidopteræ*, as well as the *gryllus*, the *truxalis*, and the *mantis*, present it almost the whole length of the cornea; whereas in the greater number of species in which the cornea is not spherical, this breadth is in general much less, and that in a very decided manner. It is from this expansion that a very considerable number of nervous filaments issue, which proceeding between the tracheæ furnished by the large circular trachea, form the network which we have already mentioned. These are the filaments which, traversing the choroid and its varnish, as well as the tunic of the cornea, go to form the particular retina of each facet, each penetrating into the hollow of one of these facets, in order to receive the impression of the light which they transmit to the brain. We see at the exterior of the compound eyes a black point which seems moveable, and the apparent mobility of which is owing to a cause which we shall explain when speaking of vision in general. In order to resume this description, we may observe that, according to the conformation of the compound eyes, the union of the small facets forms altogether the first membrane or transparent cornea, and that, besides, each of these facets may be considered as itself a cornea. The nervous filaments which pass through the tunic of the cornea are probably the particular retina of each facet. As to the tunic with which they are as it were surrounded, and which everywhere fringe the cornea, its chief use must be to diminish the impression of the luminous rays, an impression the stronger because it takes place in an immediate manner. The blackish varnish which covers the opaque membrane, situated under the tunic of the cornea, may with great probability be assimilated to the varnish of the choroid, as the membrane itself may be to the choroid. Finally, the expansion of the optic nerve applied under the choroid, must be considered with M. Cuvier as a true nervous membrane perfectly similar to that of the red-blooded animals.

After having given a general description of the compound eyes of insects, it only remains for us to describe some peculiarities of organization which different species present.

If we study the compound eye of certain *lamellicorn coleopteræ*, as, for example, that of the *nasicorn geotrupæ* and *silenæ*, we observe that its form is like a heart, and that it is divided into two in its upper part, by the portion of the cranium which supports the horn. The cornea is tolerably thick, and divided, as usual, into hexagonal facets: under this membrane we observe the mucous tunic, the optic filaments, the varnishing of the choroid, and that membrane itself, black like the tunic of the cornea.

The circular trachea exists, and the great optic nerve receives, besides, an infinity of other tracheæ, several of which accompany the small optic nerves, and reach the cornea by some subtile ramifications. The roots or principal trunks of the tracheæ are placed under the optic nerve, and are attached in the first instance to the tunic with which it is covered. They derive their origin from a considerable branch adhering below the principal trachea, and surrounded with other small similar tracheæ. The *geotrupæ*, flying only at sunset, present the same organization in their optic nerve with most of the *lucifugæ*. Their principal retina or great optic nerve approaches nearer to the cornea than in the species which fly about in open day. Thus the eyes of the giant *scarite* become after the death of the insect of a reddish white, and yet they are black when the insect is alive. This whitish colour is owing to an alteration in the tunic of the cornea; but as this alteration does not act upon the mucous varnish of the choroid, the latter remains black, of which we may be convinced on removing the cornea with its tunic. This appearance is also very common in an infinite number of *coleopteræ*, and even in several other families. In general the cornea is very thick in the eyes of the *coleopteræ:* a few only of the *hymenopteræ* (and in particular the *apis violacea*) present this membrane of an equal thickness. When the *coleopteræ* have vesicular tracheæ only, which takes place in almost all the *lamellicorn* insects, they want the circular trachea which generally surrounds the optic nerve. To conclude: the optic nerve is always surrounded with tracheæ even in a considerable number, and these tracheæ form most frequently several vesicles, which leave some interval between them, an interval generally very small. Nevertheless, in the species having the tunic of the cornea of a clear colour, instead of being black, we see on the exterior part of the eye a single point of the same colour corresponding to the aperture through which the optic nerve passes; or rather we distinguish several points, as we shall mention when we come to speak of the butterfly.

[To be continued.]

XXI. *Report made by Order of the Philomathic Society of Paris. By Drs.* Magendie *and* Blainville, *on the Subject of the new Operations and Instruments invented by Sir* William Adams *for the Cure of various Diseases of the Eye. Read and approved by the Society the 28th of May* 1814*.

We have been directed to give an account to the Society of an English work having for its title " Practical Observations on Ectropium, with a Description of a new Operation for the Cure of that Disease, and the Method of forming an artificial Pupil, &c. &c. By William Adams, Member of the Royal College of Surgeons of London, Oculist Extraordinary to His Royal Highness the Prince Regent, Oculist in Ordinary to their Royal Highnesses the Dukes of Kent and Sussex, and Surgeon to the West of England Infirmary at Exeter for the Treatment of Diseases of the Eyes." To these titles it is but fair to add, that Sir Wm. Adams enjoys not only in England, but throughout Europe, the highest reputation as an oculist.

The present work is divided into three chapters, each of which embraces an important point in the history of diseases of the eyes: we shall examine them in succession.

The first chapter treats of Ectropium. In order to understand what follows, we must observe that Ectropium is a disease which consists of an eversion of the eyelids, as a consequence of which these organs cease to cover the anterior part of the eye. This disease is not only hideous to the sight, but excessively severe upon the patient, chiefly on account of the excruciating pain occasioned by the contact of the air or a strong light, and particularly if the smallest solid body should touch the conjunctiva. There is besides a constant flow of tears down the cheek, and an habitual ophthalmia, which in a short time is followed by an opacity of the cornea, and soon afterwards by blindness.

Sir Wm. Adams chiefly confines himself to the Ectropium which affects the lower eyelid only, and which is caused by the primitive enlargement of its internal membrane. It has been proposed to cure this disease by removing with scissars the enlarged membrane, in order to reduce it to an equal length with the skin, nearly in the same way with tailors when they take in the lining of a coat the outside of which has shrunk. This mode of treatment is generally adopted; and Sir Wm. Adams having operated in this way on several persons affected with Ectropium, he found that the disease returned in the whole. He thought he had discovered the reason in the too considerable extent retained

* From the Proceedings of the Philomathic Society of Paris.

by the skin of the eyelid after the excision of the conjunctiva: he therefore endeavoured to remove a triangular stripe, comprising the whole thickness of the eyelid, including the tarsus cartilage. This stripe, so far as we can understand the author's description, ought to be nearly of the form of an isosceles triangle, the small side of which corresponds with the free edge of the eyelid. After removing it, Sir Wm. Adams brings the edges of the wound together by suture, and the union generally takes place in a few days. By following this process, the author is of opinion that a relapse will always be avoided, and he quotes several cases in support of his opinion. In all the patients treated in this way, the operation was attended with no accident whatever.

This process, which, so far as we know, has never been employed in France, appears to us very ingenious, and completely attains the object proposed by the author.

The second chapter of Sir Wm. Adams's work is not less interesting than the first: it treats of the operation necessary to form an artificial pupil. The author speaks in the first place of the circumstances which render this operation necessary, among which he particularly distinguishes the obliteration of the pupil, the partial opacity of the transparent cornea, the protrusion of the iris, &c. In this operation Sir Wm. Adams attacks the iris in its posterior side, according to Cheselden's process: he recommends that the opening should be made as large as possible, particularly if the operation is for an obliteration of the pupil with complete transparency of the cornea.

In cases where the transparency of the cornea is not extensive, with adhesion of the iris, the instrument should be introduced quite through the cornea, and attack the iris on the anterior side, so as to destroy the adhesions in the first place, and afterwards to make the aperture directly opposite the transparent portion of the cornea.

Sir Wm. Adams has made a most successful application of the property possessed by the extract of belladonna of dilating the pupil in cases where the opacity of the cornea is not extensive, and situated opposite the pupil, by introducing every morning between the eyelids a drop of extract of belladonna. A great dilatation of the pupil is produced, and the patient is restored to sight. A patient who underwent this treatment could read the smallest characters with ease: when the influence of the remedy ceased, the same person could not distinguish the largest objects. Here Sir Wm. Adams discusses the question, whether the continued application of belladonna might be productive of inconveniences? and he concludes in the negative.

One

One of our number, however*, must observe on this occasion, that he has seen animals poisoned when a venomous substance was brought in contact with the conjunctiva,—a circumstance which ought to dictate the greatest caution in the application of belladonna to this membrane. Sir Wm. Adams afterwards speaks of a process employed by the late Mr. Gibson, of Manchester, in cases where the central opacity of the cornea was very extensive, and in which the application of belladonna could have no effect. This surgeon made an incision in the cornea, one line from its junction with the sclerotica, and about three lines in length. After the evacuation of the humour, a small portion of the iris presented itself through the opening; and then Mr. Gibson with curved scissars removed the portion of the iris which had protruded into the wound in the cornea, so as to form an artificial pupil nearly circular. Sir Wm. Adams makes several objections to this process, among others that of producing a considerable opacity in the portion of the cornea which remained transparent. He substitutes another process, which consists in drawing the edge of the pupil through a small aperture made in the cornea, and leaving it in a state of strangulation: the portion of the iris which appears protruded is afterwards destroyed by the nitrate of silver. Fifteen cases, the details of which are very curious, terminate this chapter, and serve to prove the correctness of the author's doctrine.

The third chapter of Sir Wm. Adams's work treats of cataract. He gives in the first place his ideas as to the causes of this disease. He denies that there is any such thing as *scrophulous* cataract, but admits of a *venereal* one, the essential character of which is an opacity of the crystalline capsule, the crystalline retaining all its transparency. Authors have made no mention of this cataract; so that the discovery of it must be attributed to Sir Wm. Adams.

The author afterwards notices a great number of cases of cataract observed in new-born infants. On this occasion he observed, that if more than one child by the same mother were born with cataract, all who came after were affected, and even the cataracts were of a similar nature†. The author therefore maintains

* Most probably M. Magendie. This ingenious physiologist, in concert with M. Delille, has been for several years engaged in experiments on the effects of vegetable poisons on the animal œconomy. See the Reports of the Proceedings of the French Institute in the Philosophical Magazine *passim*.—Translator.

† On referring to the Author's work, we find his idea appears to be misunderstood; his words are: "In all cases of congenital cataract which have fallen under my notice, where more than one child has been affected, it has invariably occurred in succession".—Translator.

maintains that cataract is hereditary, and adduces several instances.

The method of treatment which Sir Wm. Adams seems to prefer, leads him to speak of the absorbent faculty of the anterior and posterior chambers of the eye, and of the solvent powers of the aqueous humour. He mentions several facts in support of his assertion, and among others the following: Mr. Cline, a celebrated English surgeon, operating upon a cataract by extraction, the point of his instrument broke, and remained in the anterior chamber, where it was seen to rust, dissolve, and finally disappear by the process of absorption.

Sir Wm. Adams afterwards describes the instruments which he uses for the various operations of the eye. As they do not differ much from those generally employed, we shall not enter upon any detail respecting them.

Our author proceeds as follows: In operating for solid cataract in adults and children, he employs the knife represented in the plates which accompany his work, as No. 4, being a kind of very narrow flat needle with sharp edges. The eye being fixed by the speculum, he inserts the instrument into the sclerotica, one line behind the iris: the flat part being parallel to this membrane, he makes it penetrate into the posterior chamber, and afterwards into the anterior chamber until the point be very close to the nasal edge of the pupil; then turning the instrument half round, which directs one of the edges of the knife towards the crystalline, he gives a thrust backward, so as to cut in halves the crystalline and the capsule: by various movements he afterwards cuts the two halves into several portions, taking particular care to detach the capsule and its adhesions from the ciliary processes. Afterwards quickly replacing the instrument as it was upon entering the eye, and acting with the flat side, he places the separated portions of the crystalline and of the capsule in the anterior chamber, where they are afterwards quickly absorbed.

Sir Wm. Adams thinks it highly important that the capsule and the crystalline should be divided at one and the same moment: "Not only," he observes, "do we thereby avoid a secondary cataract; but it is much easier to cut the capsule than when the crystalline had been previously extracted from its cavity. The horizontal section of the crystalline has the advantage of preventing the capsule from being detached too soon from its adhesion to the ciliary process, and the crystalline from revolving upon itself, and totally passing into the anterior chamber. This process has a great resemblance to that which has been sometimes practised in France, under the name of the process of *Broyement* (Bruising).

For

For the fluid cataract, the same instrument and nearly the same process of operation are requisite; with this difference, that we have only to attend to the crystalline capsule, which is generally opaque.

Sir Wm. Adams, for the operation of capsular cataract, prefers a needle which differs a little from that which has been described by Scarpa, and from that which we make use of for the operation by depression. The operating process consists in cutting the capsule into stripes, and subjecting it to the absorbent powers of the chamber of the eye. When the capsule is too thick, and it is difficult to tear it, Sir Wm. Adams contents himself with detaching it from its adhesions. The capsule contracts of itself, and occupies, until it is entirely absorbed, a point of the posterior or anterior chamber: as, on account of its weight, it occupies the lower part only, it does not oppose the passage of the rays of light. If the capsule adheres to the posterior face of the iris, Sir Wm. Adams, instead of dividing it into slips, and making it pass into the anterior chamber as Scarpa recommends, contents himself with separating it with great care from all its adhesions, and leaving it merely attached by a point in its circumference to the ciliary processes,—so that it may not move, but remain fixed to the posterior face of the iris, where it becomes absorbed more or less speedily. Sir Willliam Adams has seen some of these capsules, when placed in the anterior chamber, remain upwards of two years without occasioning any accident, or even injuring vision. In general it is very advantageous to separate it into several portions, for then its absorption is infinitely more speedy. Sir William Adams has a particular process for operating upon solid cataract in old persons: at this age the centre of the crystalline is so hard that the needle cannot make any impression upon it; in this case Sir William Adams makes use of an instrument a little stronger, with which he cuts the crystalline by vertical incisions, taking care to avoid any change in the situation of the part: the fragments are pushed into the anterior chamber: the place which they occupy is filled by the aqueous humour, which softens what remains of the crystalline, and admits of its being cut entirely by incisions in a second or a third operation, at the same time that it prevents the crystalline from approaching the iris. There ought not to be a long interval between the operations; for the nucleus of the crystalline might either be detached and press upon the iris, causing irritation, or, passing into the anterior chamber, where on account of its hardness it could not be absorbed, would require for its extraction the incision of the cornea. As to the capsular cataract with transparency of the crystalline, (a cataract which ought

to

to be named after Sir William Adams, since we are indebted to him for its discovery,) the method of treatment is the same as for solid cataract of adults and children. The last part of the work is also accompanied by a great many interesting and accurate observations. The whole concludes with an Appendix, in which the author suggests some doubts, which appear to be well founded, as to the appearances by which we distinguish the complication of amaurosis with cataract: here also he cites cases in support of his ideas.

The work of which we have now given an account to the Society, evinces in its author a conspicuous talent for observation, great experience, and much of that ingenious spirit which is ever fertile in resources, and so useful in the practical sciences. It is, in short, one of the best works ever published on the diseases of the eye. We are of opinion, therefore, that the Society owes its thanks to the author for his book; and that it will be an honour to the Society to enrol Sir Wm. Adams among the number of its foreign correspondents.

(Signed) MAGENDIE, D.M.S.
N. DE BLAINVILLE.*

XXII. *On some new Electro-chemical Phænomena.* By WILLIAM THOMAS BRANDE, *Esq. F.R.S. Prof. Chem. R.I.*†

§ I.

It has been ascertained by Sir H. Davy, that when compound bodies capable of transmitting electricity are submitted to the operation of the Voltaic pile, their proximate and ultimate elements are separated with uniform phænomena; that acids are attracted towards the positively electrified surface, and that alkaline and inflammable substances take an opposite direction, and collect at the negative pole.

Of the *ultimate* chemical elements of bodies, the greater number exhibit the last-mentioned character, and a few only appear to be attracted towards the positive extremity of the Voltaic instrument; and as bodies possessed of dissimilar electrical powers *attract* each other, it has been concluded, that the inherent electrical state of the former is positive, that of the latter negative.

These chemical effects were at first regarded as peculiar to the Voltaic pile, and were considered to depend upon the operation of a new agent, termed the Galvanic fluid, until Dr. Wollaston,

* This Report was read, and highly approved by the Society, at a General Meeting held on the 28th of May 1814, when Sir William Adams was unanimously elected one of its correspondent members.

† From the Philosophical Transactions for 1814, part i.

laston, in the year 1801 *, succeeded in imitating the effects by means of the electrical machine, and thus experimentally demonstrated the identity of common and Voltaic electricity.

More recent investigations, and especially the admirable researches of Sir H. Davy, have fully established the correctness of these views, and have shown that the different action of the Voltaic pile and the electrical machine depends chiefly upon the *quantity* of electricity in the former being great, while its *intensity* is inconsiderable, and *vice versâ*.

In the following lecture I shall have the honour of presenting the Royal Society with some new inquiries connected with these objects of research, and have much pleasure in adducing facts which throw further light upon this interesting department of chemical science, and which harmonize with the opinions of the able philosophers alluded to.

§ II.

When the flame of a candle is placed between two surfaces in opposite electrical states, the negative surface becomes most heated: this circumstance was considered by Mr. Cuthbertson as indicating the passage of electric fluid from the positive to the negative surface †.

Mr. Erman‡ has shown that certain substances are *unipolar* in regard to the electricty of the Voltaic pile; that is, that they are only susceptible of transmitting one kind of electricity. The insulated flames of wax, of oil, of spirit of wine, and of hydrogen gas, only conduct positive electricity; dry soap, on the contrary, and the flame of phosphorus, under the same circumstances, only transmit negative electricity.

Sir H. Davy§ considers the result of Mr. Cuthbertson's experiment to depend upon the *unipolarity* of the flame, which would render it positive, and cause it to be attracted towards the negative pole.

On perusing these statements, it occurred to me that they admitted of another explanation, and that the appearances might be connected with the chemical nature of the substances employed. I repeated Mr. Cuthbertson's experiment, and found that when the electrical machine was in weak action, the negative surface not only became hot sooner than the positive, but that the smoke and flame of the candle were visibly attracted towards it. I now removed the candle, and substituted the flame of phosphorus, when the appearances were exactly reversed; the positive surface now became considerably warmer

* Phil. Trans. 1801, p. 435. † Practical Electricity.

‡ *Annales de Chimie*, 1807, tome lxi. p. 113.

§ Elements of Chem. Philos. vol. i. p. 177.

than

than the negative, and the flame and smoke of the phosphorus were powerfully directed upon it. I conceived, therefore, that the flame of the candle was attracted by the negative pole, in consequence of the carbon and hydrogen in which it abounds, and that the rapid formation of acid matter during the combustion of the phosphorus was the cause of its attraction towards the positive pole: in short, that the appearances were consistent with the known laws of electro-chemical attraction.

To ascertain the correctness of this idea, it became necessary to examine the phænomena with greater precision, and to institute the more extended series of experiments, which, with their results, I shall proceed to detail.

The apparatus employed consisted of two insulated brass balls capable of being brought near to, or removed from, each other, with a small table between them, as represented in the annexed drawing. (Pl. II.)

When it was necessary to ascertain the relative temperatures of the balls with accuracy, I made use of the instrument, fig. 3, devised by Mr. Pepys, in which *a a* represent two thin spheres of brass containing delicate thermometers, the bulbs of which, as well as the interior of the spheres, were coated with lamp black, to render the increase of temperature more evident. One of the balls was connected with the negative, the other with the positive conductor of a small electrical machine of Mr. Nairne's construction, as represented in fig. 2, so that the apparatus was perfectly insulated.

§ III.

Exp. 1. A small stream of olefiant gas was burned between the balls. The flame was evidently attracted towards the negative side. The combustion was continued for one minute. At the commencement of the experiment, the mercury in the thermometers stood at 60° Fahrenheit; at its termination the positive thermometer indicated 62°, the negative 72°.

2. The experiment was repeated with sulphuretted hydrogen: the flame was now only slightly attracted by the negative ball, but the sulphureous acid vapour passed off towards the positive surface.

In this experiment the positive thermometer rose 3° and the negative thermometer 6° in one minute.

3. A very small flame of phosphuretted hydrogen was slightly bent towards the positive pole. In one minute it produced an effect upon the positive thermometer =5°, upon the negative =3°. When a larger flame was used, it appeared equally drawn towards the two electrical poles, but the acid vapour passed off in the direction of the positive ball.

4. The

4. The flame of arsenicated hydrogen was attracted by the negative surface: the fumes of white arsenic produced during the combustion were slightly attracted towards the positive pole.

5. The flame of hydrogen appeared weakly attracted by the negatively electrified ball; but on employing the apparatus with thermometers, the rise was nearly equal on both sides. In one experiment, made with much caution, the thermometers stood at its commencement at 56°. The gas was burned for two minutes: the negative thermometer rose to 62°, the positive to 61°.

In a second experiment, the combustion was continued for five minutes: the effect upon the negative thermometer was $=4°$, that upon the positive $=2{\cdot}5°$.

6. With a very small stream of carbonic oxide, the results were still less distinct. The tip of the flame appeared in some cases to be slightly inclined towards the positive ball, but one thermometer was not more affected than the other.

On substituting a larger current of the gas, which produced a flame between three and four inches in length, it was much more evidently directed to the positive surface, but the rise in the positive thermometer was less than might have been expected. In several cases where the combustion was continued for two minutes, the rise in the positive thermometer did not exceed that of the negative more than 2·5 to 3 degrees.

7. Sulphur, in a state of combustion, was placed between the oppositely electrified balls. The flame being extremely small, its direction could not be observed, but the sulphureous acid vapour was attracted by the positive ball.

8. The *flame* of the sulphuret of carbon (sulphuretted liquor of Lampadius) was attracted by the negative surface: the *acid fumes* which it throws off took an opposite direction.

9. When phosphorus in a state of vivid combustion was introduced into the electrical circuit, the flame, and the phosphoric acid produced, were powerfully directed towards the positive surface.

The acid vapour which arises from phosphorus, during its slow combustion in a warm atmosphere, is also attracted by the positively electrified ball.

10. A small stream of muriatic acid gas was allowed to pass into the atmosphere between the electrified spheres; it was immediately attracted to the positive pole. This effect is rendered more evident by the diffusion of a small quantity of ammonia through the atmosphere of the room in which the experiment is made.

The attraction of the muriatic acid is strikingly exhibited by coating the conductors with litmus paper, placing them at a distance

distance of about six inches asunder, and propelling a current of the gas through a small tube between them. If the electrical machine is not in too powerful action, the positive ball is instantly reddened, while the blue colour of the negative surface remains unaltered.

11. Nitrous acid gas exhibited the same appearances as muriatic acid.

12. Potassium in a state of combustion was placed between the electrified surfaces. Its flame, and the alkaline fumes it produces, were drawn to the negative conductor.

13. Ammonia afforded no very distinct results. I attempted to ascertain its electrical state by disengaging it through a small tube placed between the conductors, the atmosphere being slightly tainted with muriatic acid; but it was apparently equally attracted and repelled by the electrical surfaces. When the conductors were coated with turmeric paper, the negative appeared sooner reddened than the positive, but in a very short time the effect upon each became equal.

14. Benzoic acid evolved by gently heating benzoin betweeu the electrical poles, was attracted to the positive side; but when the balsam took fire, the flame and carbonaceous matter were immediately drawn in an opposite direction.

I heated some pure benzoic acid placed upon a silver capsule, between the balls; as long as the temperature was only sufficient to raise it in vapour, it took the positive direction: but the moment that the acid was inflamed, the carbonaceous fumes passed to the negative side.

15. Camphor during its combustion throws off a large quantity of charcoal, and, when burned between the opposite electrical surfaces, furnishes a good instance of the attraction of that substance by the negative pole, which soon becomes coated with it, the positive ball being much less soiled.

16. The resinous bodies in general exhibited the same appearances as camphor: when in a state of combustion, both the flame and smoke were repelled by the positive, and attracted by the negative pole.

17. Amber, on the contrary, presented phænomena nearly similar to those of benzoin. When brought into a state of fusion, its acid vapour was attracted by the positive ball; but when made to burn, an opposite effect was produced.

§ IV.

In the preceding detail of experiments, such only have been selected as were attended with marked results. The electrical energies of many other substances were examined and compared, but no new facts were ascertained, nor were any circumstances observed which

which interfere with the inferences suggested by the above statement.

To insure accuracy, each of the experiments was several times repeated in the presence and with the assistance of some of my chemical friends, and the results were uniform when proper precautions were attended to. It is especially necessary to operate in a quiet atmosphere, and to employ a feeble electrical power; for, if the machine is too strongly excited, the substances under examination are alternately attracted and repelled between the poles, especially if insulated, or non-conductors of electricity. The balls were usually withdrawn from each other to a distance of four inches, and the subject of experiment placed equidistant between them. The poles too were occasionally changed, by moving the connecting wires from one ball to the other, with a view to insure correctness by observing the change thus produced in the direction of the flame or vapour.

The experiments were sometimes varied, by using the Leyden jar with a sliding electrometer connected with its outer surface: the substance, the electricity of which was to be examined, was placed between the ball of the charged jar and that of the electrometer.

Regarding these experiments as connected with electro-chemical theory, they appear to furnish a more evident proof than has hitherto been offered, of the inherent electrical states of matter, which are decidedly exhibited by the attractions and repulsions between the opposite poles; and when connected with Dr. Wollaston's researches, to which I have elsewhere alluded, they amply demonstrate the identity in chemical powers of common and Voltaic electricity.

The attraction of acids by the positively electrified surface, and of alkalies and inflammables by that which is negatively electrified, is thus easily exhibited; and the theory which regards their mutual attractive energies, as dependent upon their opposite electrical states, confirmed by experiments not less decisive than those in which the Voltaic instrument was employed.

Of the former class, phosphorus in slow and in rapid combustion, and benzoic acid, furnish the most striking instances; and of the latter, the combustion of potassium and of camphor are excellent examples.

There are, however, some circumstances which appear difficult to explain, and which have not turned out as might have been expected *a priori*. The combustion, for instance, of carburetted hydrogen gives rise to the production of water and carbonic acid, but its flame is entirely and powerfully attracted by the negative surface; and carbonic oxide, which produces carbonic acid, is not

not very evidently attracted by either pole, unless the flame be of a very large size. It may, however, be conceived with regard to carburetted hydrogen, that the carbonic acid which is formed does actually pass off to the positive surface, and that the polarity of its flame is influenced rather by the combustible, than by the product of combustion: this idea is in some measure sanctioned by the appearances exhibited by the flame of the sulphuret of carbon, which is directed towards the negative ball, although the sulphureous acid visibly passes towards the positive side, and it is fair to infer that the carbonic acid accompanies it.

An attempt was made to detect the carbonic acid by means of caustic potash: for this purpose a piece of linen, moistened with the alkaline solution, was applied to each conductor, and a current of carbonic acid, issuing from a small tube, was directed between them. The linen was then put into dilute muriatic acid, and it was expected that the effervescence would be greatest in that removed from the positive pole: this generally appeared to be the case; but I cannot say that the results were satisfactorily distinct, nor indeed does the method admit of the accuracy required.

The experiments related in the second section of this lecture suggest an explanation of the phænomena alluded to, as observed by Mr. Erman, more consonant with the known laws of electricity than that which he has suggested. The flame, for instance, of oil, wax, &c. must be considered as consisting chiefly of those bodies in a state of vapour; and their natural electricities being positive, it is obvious that, when connected with the *positive* pole of the battery, and with a gold-leaf electrometer, the leaves will continue to diverge; but when applied to the *negative* pole, that electrical state will be annihilated by the inherent positive energy of the flame, and consequently the leaves of the negative electrometer will not diverge. On the other hand, the flame of phosphorus is negatively unipolar. Now it has been shown, that this flame (owing probably to the rapidity with which it is forming a powerful acid by combination with a large portion of oxygen) is attracted by the positively electrified surface, and consequently that it is itself negative, so that it would transmit negative electricity to the electrometer, but would annihilate the positive power, and thus appear as an insulator under the particular circumstances which Mr. Erman has described.

XXIII. *Ac-*

XXIII. *Account of a new Species of Insect of the Genus* Corynetes *of* Fabricius, *observed in an Egyptian Mummy at Grenoble.* *By M.* Champollion Figeac*.

The circumstances attending the discovery of this insect, hitherto unknown, are equally singular as the result is in itself interesting; no person, in fact, would expect to find a new species of insect in an Egyptian mummy.

The curiosities of this description, which various public and private cabinets of Europe contain, have been examined by many. Blumenbach and several other naturalists have studied the various kinds of embalming which they present, and have inquired into the process itself, combining their remarks with what Herodotus and Diodorus have written on the subject. Considered in an anthropological point of view, the mummies prepared by the Egyptians have served to prove that there existed certain differences of form or proportion between the individuals embalmed; *i. e.* the people of Egypt or the adjoining countries, and the inhabitants of Europe. Antiquaries, seeking only for traces of the arts among the ancients, have described the paintings which adorn the *envelopes* of the mummies, and their sycamore coffins. Finally, the discovery of several Egyptian manuscripts, hieroglyphical and alphabetical, inclosed in the wrappings of the mummies, having excited the curiosity of the learned, they have endeavoured, as well as they could without destroying the mummy, to search minutely all those which have reached Europe.

Similar motives induced me to study those which belong to the Cabinet of Antiquities of the Public Library of Grenoble.

One of them, remarkable for the richness of its ornaments, is still more so from the kind of embalment which it has received, and which, consisting merely of an injection into the vessels of a preservative liquid, has left every limb its natural form and flexibility. This was the mummy which I examined in the summer of the year 1810, aided by M. Champollion jun.

The instant it was removed from its case or coffin, and placed on a convenient table, we perceived that it had been visited before by means of an aperture made in the lower part of the dorsal spine, an aperture which occasioned the loss of several vertebræ and some of the ribs. We know, in fact, that the Arabs, in order to search for gold and talismans, carefully inspected all the mummies which they discovered, before selling them to be sent to Europe for the uses of pharmacy, or for the cabinets of the curious. The mummy in question seems to

* *Magasin Encyclopédique* for May 1814.

have

have been treated in this way; but the aperture had not penetrated to the forepart of the body: the cavities of the belly and stomach were untouched, and filled with a blackish earth, with which were mixed the fleshy parts, which had fallen into a fat and unctuous powder in consequence of the humidity which had penetrated them. The arms crossed over the breast were still in their place.

On examining the hands attentively, we perceived in the interstices of the fingers several dead *coleopteræ* of a rose colour in all its brilliancy, and two or three lines in length. This insect being new to us, we collected all that could be found, with the view of obtaining better information respecting it.

A short time afterwards an opportunity presented itself of showing them to M. Jurine, of Geneva, whose opinion convinced us that we did well in preserving these *coleopteræ*, which will furnish to the nomenclature of entomologists a new species of a genus by no means numerous. We subjoin M. Jurine's description.

"*Generic denomination.*—*Corynctes* of Fabricius. *Necrobia* of Olivier.

"*Specific ditto.*—Species non descript, which holds the middle rank between the *corynctes rufipes* and the *corynctes abdominalis*, but which is neither the one nor the other." M. de Jurine is desirous to name it the *corynctes glaber.*

Here we have a new species well determined, and the more interesting because the genus *corynctes* is new, and we know only two species, both of them exotic; while the insect observed at Grenoble, which is also exotic, will form the third.

There cannot be a doubt that it was brought to Europe with the mummy, since it was found in the inside of the body under several envelopes: but how and when did it take up its residence there? It will not be easy to answer this question. M. de Jurine, whom it has not escaped, is inclined to think that the larvæ of the insect have been introduced into the mummy since it came out of its sepulchral deposit; viz. when it fell into the hands of the Arabs half a century ago, and was sent direct from Cairo to Grenoble by M. de Mure, then French Consul in Egypt.

It may also be fairly conjectured, that the residence of the insect under the envelopes of the mummy is of a much older date, particularly when we consider that, the larvæ of the *corynctes* being carnivorous, the desiccation of the body when the Arabs removed it from the tomb, or the well, could not be very attractive to it, nor suit its voracious appetite. It is also natural to suppose, from the method of embalment practised on this mummy, by submitting the body to several operations made in the

the open air for 70 days, that in this interval the larvæ might have been lodged between the fingers of the hand while still fleshy, and wrapped up with them; and they might at first have entered the flesh, and afterwards been brought to maturity while the state of the body admitted. The insect must have afterwards attained its full growth, and ended its life in the state in which it was found. I think it right thus early to mention this discovery, for the benefit of those who study entomology. In the mean time I am preparing a more extensive work on mummies in general, their embalment, and the paintings with which their envelopes and cases are adorned.

XXIV. *Notices respecting New Books.*

Observations sur les Insectes, &c.—Observations upon Insects considered as ruminating Animals, and on the Functions of the various Parts of the intestinal Tube in this Order of Animals. By Marcel de Serres. 1 vol. 4to. Paris, 1814.

This work, which has been favoured with various marks of approbation from the French Institute, is written to prove that the organization of insects is by far too simple to admit of their operating a real rumination of their food. Some very eminent anatomists, however, among whom are Malpighi, Swammerdam, and Cuvier, are of opinion that there is an entire order among the insects, similar to that of the ruminating class of animals. M. Marcel de Serres, on the other hand, seems to have the merit of demonstrating by anatomical and physiological proofs of a palpable nature, that all the insects as yet known have an organization totally different from that which they would have if their food underwent a true rumination. But in order to enable our readers to decide upon the evidence which M. de Serres adduces, we shall follow him through his work, and detail the facts upon which he chiefly rests.

Rumination, according to M. de Serres, is an act of volition: we see, in fact, that animals which ruminate, *i. e.* which pass their food from one stomach to another, and finally to the mouth, suspend this rumination when they wish to retard their digestion, and ruminate afterwards when they are no longer pursued. Thus, these animals place in reserve, in their large and ample stomachs, such food as has not been masticated, and which for this reason ought to be again thrown up into the mouth to be remasticated at leisure. Rumination is therefore connected in this respect with the habitudes of the animal; and all the ruminating animals are herbivorous, and placable in their

 dispositions.

dispositions. Living upon food not very sapid, frequently disturbed in the act of eating, and being nearly all defenceless, they can nevertheless store up their food to chew it at another opportunity. Here we see the use of the great stomach of the ruminating animals, and rumination is a necessary consequence.

The author afterwards inquires, if the insects which have been regarded as ruminators had the same occasion for fear, and if it was necessary that they should have an opportunity of storing up a certain quantity of food to remasticate it in a time of rest and tranquillity? From an attentive observation of the habits of these insects he answers in the negative, nor do their habits announce that ruminating would be of any advantage to them. These animals having a very decided voracity, the complication which is observable in their digestive organs seems to be relative to this activity in digestion only. Nature has done every thing to accelerate this digestion, and the peculiar means and apparatus which she has given for this purpose have deceived most anatomists. But in order to render this perfectly evident, let us examine the structure of the intestinal tube in the species of insects which have generally been regarded as ruminating.

An entire order of insects, the *orthopteræ*, has been regarded as subjecting the food to a real rumination. They present next after the œsophagus a stomach which is sometimes placed on the same line with this viscus, and sometimes a little on one side. The stomach communicates with another organ armed almost always with hard and coriaceous scales: this organ has been assimilated to the *bonnet* of the true ruminants; but it is remarkable that its strong and muscular valvules, and the scales or sharp teeth with which it is armed, render its organization very different from that of the bonnet of ruminating animals. These teeth with which it is provided, and the thick and muscular membranes which enter into its composition, make it on the contrary resemble the crop of birds, its action being that of triturating the food in a more perfect manner. Under the gizzard we meet with organs which do not vary in their position, but which differ in number. Sometimes very extensive, and in that case being two in number only, these organs have been assimilated to the *feuillet* and the maw of the ruminating animals, and consequently they have been regarded as true stomachs. Observation proves that, in whatever state digestion is, these pretended ventricles never contain any food. We see them, on the contrary, filled with a peculiar humour, which does not appear to be diminished after a long fast. Besides, how could those organs, if they were real ventricles, diminish in size until they became capillary, and multiply so as that some individuals present no less than forty? It is in fact difficult to conceive, that

that the alimentary paste can re-ascend into the mouth, after being divided into so many different portions in order to occupy the interior of these multifarious stomachs. In the second place, these organs often present at their extremities a great number of very minute capillary vessels, which indicate that a secretion is taking place: these vessels contribute in an evident manner to increase the liquid which we observe in these ventricles. This liquid is so abundant, that it fills the whole capacity of these viscera: it proves that they by no means exercise the function of stomachs, for where could the alimentary paste be lodged?

If these organs do not immediately assist digestion, what is their use, and of what nature is the humour which they contain? These are questions which the author thinks he has amply resolved.

All the insects which have been considered as ruminating, display, as already observed, a great voracity: consequently nature has made the process of digestion with them equally prompt and easy. As the teeth of insects triturate their food but imperfectly, this defect is supplied by rough and cutting scales, with which the internal membrane of the gizzard is armed. These scales, put in motion by a muscular tunic of great contractibility, exercise an action so much the greater, as they approach closer in the contractions of this organ, and they are besides very multifarious.

The gizzard is not the only organ which contributes to promote digestion; and the two viscera, erroneously assimilated to the first stomach, the maw of ruminating animals, have not the smallest share in it. They do not act by themselves, but rather by the humour which they secrete. This humour, on account of the disposition of the vesicles which contain it, as well as of the gizzard, may ascend into the stomach, and act in this way upon the alimentary paste. But in order to exercise an efficacious action on the food, this humour must have solvent properties; and analysis indicates that it is sufficiently analogous to the bile. At least this alkaline humour contains a resinous matter, perhaps the yellow matter, soda, albumen, and a great quantity of water. This composition, partly similar to that of the bile, announces that this animal fluid must have a great action on the food; which agrees perfectly with the physiological facts related by the author in his work.

Thus these two organs, which are assimilated to the first stomach and to the maw of the ruminating animals, never being filled with alimentary paste, do not seem to exercise the same functions with the real stomachs. Their excessive multiplication in a number of individuals is another proof of it; for it would be difficult to conceive that the alimentary paste was obliged to separate into thirty or forty portions, in order to be better elaborated.

These facts seem to prove, therefore, that those organs can by no means be assimilated to stomachs, being far from fulfilling their functions: the abundant humour which they contain induces a supposition that they are a kind of vesicles destined to secrete a species of bile, which supplies the place of the bile in insects, the pancreatic juice, and even the saliva which we observe in animals of a superior order. The small vessels which exist at the upper extremity of these vesicles, would have no other use than to pump up the humour which bathes all the parts of the bodies of insects, and thus to collect the materials of the fluid secreted in these biliary vesicles.

The author then proceeds to detail a variety of interesting experiments in illustration of his ideas. By putting these vessels into coloured fluids, without allowing the biliary vesicles to receive any impression from them, he saw the vessels absorb the liquor in which they were plunged, and after a certain time the vesicles themselves received the colour. It still remained to ascertain how the secretion of the bile took place when these small vessels no longer existed. In order to verify this point, the author put the hepatic organs in coloured liquors, and he saw that the absorption equally took place by the membranes of these same viscera. Thus, whether the vesicles have or have not secretory vessels, they nevertheless suck up from the humour which bathes all the parts of the body, the materials of their secretions. What also proves that these organs do not in any way perform the functions of the stomach, is, that we frequently observe them filled with an abundant humour in individuals which have died after a long fast. In short, according to M. de Serres, the true use of these pouches cannot be mistaken, since the individuals who undergo a complete fast perish the more speedily, the more considerable the quantity of the biliary humour. The bile being, as is well known, an irritating humour, its action is most energetic on the membranes of the stomach when it acts upon them in a more direct manner.

But M. de Serres does not confine himself to making us acquainted with the uses of the various parts of the intestinal canal of insects: he also gives an anatomical description of this canal, considered in the greater number of families. He has also given us some very valuable details on the structure of this canal, on the membranes of which it is composed, and finally upon the nerves and tracheæ which proceed to it. When an opportunity offers of drawing general conclusions, he does not neglect it. He remarks for instance, that the extent of the tongue and the

development

development of its villous coats are more considerable in the herbivorous animals, on account of the tastelessness of their food, whereas the prehensile organs are more developed in the carnivorous animals. In the same way he observes with respect to the alimentary canal, that the more the animal lives upon vegetables the more extensive is this canal. Thus, according to his observations, the connection between the proportions of the intestinal tube and the species of food is the same in the masticating insects as in the animals with vertebræ.

Fully sensible of the importance of the determination of all these minutiæ to the study of animals in a general point of view; M. de Serres has lost no opportunity of generalizing the facts which he observed, and always with that rigour which the present state of the sciences demands. The following are the theorems with which he concludes his work:

"1. That there do not exist any insects which operate a true rumination; and that the *orthopteræ*, which had been erroneously supposed to ruminate, cannot possibly from their structure throw up their food into their mouth.

"2. That the gizzard of insects, assimilated improperly to the bonnet of the ruminating animals, cannot under any circumstances bring the food up to the stomach, either on account of the arrangement of its valvules, or from the relative weakness of its muscular power when compared with the resistance of its scaly membrane.

"3. That the viscera hitherto considered as third and fourth stomachs are hepatic vessels, or kinds of vesicles which serve as a reservoir to the biliary fluid; those organs having, under certain circumstances, secretory vessels destined to suck up the materials of the humour which they are to elaborate, and in other cases operating of themselves the secretion for which they are destined.

"4. That in the very voracious species there are several orders or rows of hepatic vessels, and in those which have the most, the number of rows of these vessels never exceeds three.

"5. That the upper hepatic vessels are always more developed than the lower, and their extent in the inverse ratio of their number.

"6. That when there is only a single order of hepatic vessels, these vessels are merely simple elongated tubes, almost capillary, and floating freely in the interior of the body.

"7. That the development of the hepatic vessels is always relative to the extent and complication of the gizzard; these two kinds of organs being themselves in proportion with the quantity and species of food used by the insects.

"8. That

"8. That in general, when the gizzard is scaly, the internal membrane of the stomach cannot be classed with the three kinds of tunics which enter into the structure of the intestinal canal of insects, this membrane then approaching the nature of the fibrous membranes.

"9. That the gizzard being simply muscular, *i. e.* deprived of the scaly or coriaceous membrane, the internal tunic of the stomach is always mucous."

The author observes in the last place, that the presence of the biliary vessels, and of the gizzard, is in proportion to the organs of manducation, or to the quantity and description of food. Thus, the relation which exists between the organs of manducation and the digestive apparatus, seems to prove that, in the classification of insects, we ought not to pay much attention to the organs of the mouth, except in those insects which operate a real mastication of their food. This observation is the more important, because it may lead to a more natural classification of insects; an order of animals whose organization is peculiar, and in which analogy, which directs us in the study of the higher classes of animals, can be of little use to us.

The drawings with which the work is illustrated do great credit to the skill of M. de Serres, and tend greatly to elucidate his subject. Physiologists, and those who prosecute the study of general anatomy, will find this work a valuable assistant in their pursuits.

Bibliotheca Spenceriana.

Every collector of rare books is more or less acquainted with the bibliographical labours of Mr. Dibdin. His publications, like the works on which he treats, are objects to be coveted, and hardly appear before they are out of print. His last and most splendid work, *Bibliotheca Spenceriana*, in three volumes*, large imperial octavo, published at eight guineas, has risen to sixteen or twenty, and will soon be unattainable at any price. The object of the work is to make the public acquainted with some of the *principal treasures* of Earl Spencer's library, the richest private collection in Europe; and is exclusively confined to an account of *books printed in the fifteenth century*, and to some of the more important *first editions* in the *sixteenth century*. The author remarks in his preface, that "such a work, if executed with copiousness and precision, cannot fail to be interesting to the bibliographer, to the scholar, and to the lover of ancient literature and the fine arts." It is but justice to

* A fourth volume is in the press.

say

say that the work has been so executed; and that its value has been greatly enhanced by the *fac similes* and *embellishments* with which many of the descriptions of scarce works have been illustrated. The FIRST volume is confined to *block books, theology,* and *ancient classics*: the SECOND contains *ancient classics*: the THIRD, *collections; lexicography; miscellaneous authors.* The FOURTH will contain *more miscellaneous authors; books printed in the Italian language; books printed by William Caxton; books printed in the Abbey of St. Alban's; books printed by Wynkyn de Worde, and by Pynson; supplement; emendations; index of authors, and of editions described; of printers, and of editions, &c.*

This work, from the press of Mr. Bulmer, may be considered as a most interesting specimen of typography. To notice that it has been printed with great care, and that the press-work is well executed, would be no more than saying that it came from the Shakspeare press. The singular nature of the materials, the numerous *fac similes* (some of them embracing three or four colours), and the diversity of letter and arrangement often required in the same page—all required a felicity of execution more to be desired than hoped for. But Mr. BULMER has vanquished every difficulty, and produced a work which will deservedly be ranked among the most singular curiosities of that wonderful art which it illustrates.

XXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

To correct an inaccuracy in our last Report, we insert the following letter:

"Tower, August 13, 1814.

"SIR,—I HAVE read in the last number of your Journal, the abstract which you have given to the public of the contents of a paper of mine lately read before the Royal Society, which abstract I find to be remarkably erroneous. I conceive therefore that, as you must naturally wish to correct any wrong information which you may have accidentally given, you will be glad to receive from me the means of so doing; for which reason I point out to you the following errors.

"The first is in the title given to my paper: it is not 'On the triple salts called prussiates,' but 'On the salts called triple prussiates, and on acids formed by the union of certain bodies with the elements of the prussic acid.'

"The next is in the assertion that 'I derived my facts chiefly from Proust's experiments;' whereas they are deduced almost entirely

entirely from a numerous series of experiments devised and executed by myself. The last error is in the name *carhyoxic*, and in the explanation attached to it. You state that by this name I proposed to designate the acid contained in the before-mentioned salts: but the real name which I proposed for it was the ferruretted chyazic acid, the word *chyazic* being composed of the first letters of the elements *c*arbon, *hy*drogen, and *az*ote, with the syllable *ic* added; and the word ferruretted implying the iron which it contains, and at the same time distinguishing it from the sulphuretted chyazic acid also discovered by me, and of which a particular account is given in the paper from which your abstract was made.

"I am, sir,

"Your most obedient servant,

To Mr. Tilloch. "R. PORRETT Jun."

PROCEEDINGS OF THE FRENCH INSTITUTE FOR THE YEAR 1813. BY M. CUVIER.

[Concluded from p. 63.]

Two years ago we noticed the experiments of M. Lechenault on the deleterious effects of the juice known at Java by the name of *upas*, when introduced into wounds, as well as those of Messrs. Delille and Magendie, which tend to prove that it is essentially on the spinal marrow that this poison acts.

Having been frequently witnesses of the frightful rapidity of its action, Messrs. Delille and Magendie were tempted to doubt that it could have been transported so quickly into the marrow by the tortuous and intricate way of the lymphatics; and they inquired if we ought not to admit, at least in certain cases, the absorbent faculty generally ascribed to the veins when we were not so well acquainted with all the ramifications of the lymphatic system. In order to come to some conclusion in this respect, they applied the upas to parts which adhered to the animal body by blood-vessels only: for example, they cut off all the mesentery adhering to the intestine of a goose, leaving only the arteries and veins; and after having placed the upas in the interior of this goose, they cut it and tied both ends: nay, what appears still more conclusive, they cut a thigh leaving the vein and artery only entire, and afterwards applied poison to the foot: finally, in order to remove even the objection of invisible lymphatic vessels, which might have belonged to the texture of these two blood-vessels, they removed a segment of both, after having supplied their place with quills, so that there was no longer any communication between the member and the animal, than by the

the blood which circulated from the one to the other. In all the experiments, convulsions and death came on as promptly as if the upas had been applied to the entire animal. Some however will still object, perhaps, that when the upas was introduced into the intestine, it might always be supposed that there remained some concealed lymphatic vessel; that, when it was applied to the foot, it was inserted into a wound, from which it could penetrate into the blood by open veins and arteries; and that this is by no means what is meant when we admit the venous absorption, for in that case we mean an action attributed to the veins in their natural state, and by their organic pores. What is still more remarkable in the experiments of Messrs. Magendie and Delille, is, that the blood of an animal already poisoned, and ready to die, when transfused into the veins of another animal, does not kill the latter, and scarcely occasions to it any inconvenience.

M. Magendie has made another very interesting application of this action of certain substances, when introduced into the blood.

We know that an emetic injected into the veins of an animal makes it vomit in a few minutes, whilst it requires a whole hour when an emetic is swallowed to produce the same effect: and we instantly conclude, that this convulsive movement does not depend on the immediate action of this remedy on the coats of the stomach. Observations made on the viscus itself, during the operation of vomiting, have led some physiologists still further. They perceived that the coats of the stomach underwent very little agitation; and hence they concluded also, that it is not in the irritation of these coats that the immediate cause resides of the expulsion of the contents of the stomach. Their opinions, however, were but feebly supported, and have almost fallen into oblivion since Lieutaud and Haller introduced one directly contrary.

M. Magendie, wishing to ascertain the truth, employed the convenient method of injections; and having first made an opening in the abdomen, he ascertained by the touch, that during vomiting the stomach itself remains in a state of inertia, but that at every successive retching it is violently compressed by the contractiou of the diaphragm and the muscles of the lower belly: besides, the long inspirations which precede every vomit introduce a sufficiency of air into the stomach to prevent its capacity from diminishing, notwithstanding the quantity of matter which it ejects. If we open the abdomen wide enough to let out the stomach, the nauseæ continue; but they become impotent, because the muscles no longer compress the viscus: when we replace the stomach, the vomiting immediately begins.

Compression

Compression is not sufficient of itself, however; for, if we compress with our hands a stomach displaced as above in a dog into whose veins no emetic has been injected, we can very well expel its contents without producing thereby a true vomiting, because there are neither nauseæ nor inspirations attending this kind of convulsion: but if we pull the stomach instead of compressing it, and if we extend the pulling to the œsophagus, the nauseæ and all the other symptoms of vomiting appear, without there being any occasion for an emetic. Thus, vomiting would result from the compression exercised on the stomach by a convulsive contraction of the muscles which surround the belly; and this contraction itself may be excited by an irritation of the œsophagus.

It being of importance to know what muscles chiefly acted, what nerves put them in motion, and by what causes they were irritated, M. Magendie in the first place cut or removed the abdominal muscles, without much diminishing the activity of the vomiting: on the contrary, when we take from the diaphragm a great part of its strength by the section of the phrenic nerves, there are nothing but small retchings at long intervals, and the vomiting rarely takes place notwithstanding the abdominal contractions. Thus, the part acted by the diaphragm in this compression is by far the greatest. When we thus destroy at once the action of the diaphragm and that of the muscles, the vomiting no longer takes place, even if we make the animal swallow substances eminently and promptly emetic, such as corrosive sublimate. Finally, and this seems to form an almost marvellous completion of all his experiments, M. Magendie entirely removed the stomach: he substituted for it a bladder, which he attached permanently to the base of the œsophagus, by making it communicate with this conduit by a solid tube; and after again sowing up the abdomen, he injected some emetic into the veins: the animal had nauseæ, made inspirations, and ejected a coloured liquid, (with which the bladder had been partly filled,) quite as well as it could have done, if, with a natural stomach, an emetic had been administered in the common way.

Thus, an emetic does not cause vomiting by irritating the fibres of the stomach, nor even the nerves, but by acting by means of absorption and circulation on the nervous system, and by exciting an action which is reflected specifically on the œsophagus and diaphragm, so as to make them exert various movements, among which there are some, the definitive result of which is the compression of the stomach: this does not prevent there being vomitings produced by the immediate irritation of the nerves of some of these parts, or by any given nervous irritation which

which would be propagated so as to affect the system nearly like an emetic.

It remains to M. Magendie to distinguish with more precision the part acted by the œsophagus and the diaphragm in the act of vomiting, and to examine the phænomena of this movement in birds and other animals which have no diaphragm.

To these experiments with antimony considered physiologically, M. Magendie added some others upon its medical or deleterious action; and he ascertained by many observations made upon human beings, and by several experiments upon animals, that the tartrite of this metal, taken in large doses, is of itself a deadly poison; but that almost always its first effect is a vomiting, which rejects the greater part before any mischief has been done: in this way many suicides are disappointed in their melancholy intentions.

M. Magendie has also communicated to the Class a series of experiments relative to the use of the epiglottis. This cartilage, placed at the root of the tongue in men and quadrupeds, is generally regarded as destined by nature to prevent the food from falling into the wind-pipe. Birds and reptiles have no epiglottis, and yet they experience no inconvenience from this privation; but their glottis is preserved by other means, such as the teeth or excrescences with which they are most frequently bristled, so that no valid objection can be taken to the commonly received opinion. Certain individuals who have been deprived of the epiglottis by accident, and who have nevertheless swallowed as well as before, gave rise to very strong objections; and some anatomists have even concluded, that the epiglottis is intended to assist the voice only, and not the act of deglutition.

M. Magendie having removed the epiglottis from some dogs, ascertained that their deglutition was not impaired: he also ascertained by an immediate inspection, that the glottis contracts completely at the instant of deglutition, so that nothing can pass it even if the epiglottis did not exist: finally, by cutting the nerves which proceed to the contractor muscles of the glottis, he saw that the latter remained open and admitted the food, notwithstanding the presence of the epiglottis which he had allowed to remain.

It is difficult to withhold our confidence from experiments which agree so well with each other and with known facts: it is for physiologists, however, to inquire what may be the true use of an organ too distinctly developed, and too constant in a whole class of living beings, not to have an essential and particular destination.

M. Magendie was led by his researches to examine the particular distribution of the laryngal and recurrent nerves in the various

various muscles of the larynx, and this part of his labours adds some precision to this interesting point in anatomy.

While much has been doing to extract sugar from beet-root and other saccharine vegetables, M. Marsan, professor of medicine at Padua, has drawn up a memoir upon another plant, more resembling the sugar-cane in its botanical characters, and in the quantity and quality of the sugar which it yields, than any hitherto discovered. It is a large gramineous plant from the South of Africa, described for the first time in 1775 by Peter Arduino, under the name of *holcus cafer*, and well characterized by its velvet down and globular seeds. It is now cultivating in various parts of Italy, Bavaria, and Hungary.

Indigenous coffee seems hitherto to have been less easily obtained in Europe than sugar: the torrefaction of many seeds and roots has been attempted with a view to procure substitutes, but the liquor produced by them had nothing of true coffee but its blackness and bitterness.

M. Levrat, a physician at Chatillon sur Chalaronne, thinks that the seed of the yellow water flag of our marshes (*iris pseudacorus*) is that which most approaches the coffee berry, after drying it by heat and freeing it from the friable shell which envelops it: it is then torrefied, and infused like coffee: he has at least ascertained that the seeds of the *iris* may certainly be used with effect to obtain the febrifuge properties of coffee, and thus serve indirectly as a substitute for the bark. This last discovery would be the more important, as, from the circumstance of the *iris* growing in all marshes, it would relieve nature from the reproach of having placed her remedy so far away from the diseases, as in the case of bark.

Since the custom has ceased of destroying whole swarms of bees in order to get their honey, various methods have been devised for removing the bees into another hive without risk of being stung. M. Chambon, a physician at Paris, has proposed a simple and easy way: this consists in having hives which can be opened at top, placing them on a glass furnished with a metallic plate, under which the smoke may be safely introduced: if an empty hive is then placed over the upper aperture, the smoke will force the bees to ascend into it. The same gentleman has made some experiments to ascertain if it was advantageous to cover sheep with cloths, as the ancients did with much boasted profit. M. Chambon did not find, however, that the wool was increased either in value or beauty, so as to indemnify him for the expense of the cloths.

M. Chambon also read a memoir on the dangers to which anatomists are liable in their dissections, and on the means of preventing and remedying them: these are sometimes very formidable;

midable; but happily they are of very rare occurrence; and the remedies and preservatives belong to that class which medicine adopts against contagion and venomous wounds.

M. Orfila, a young Spanish physician, has presented to the Institute an extensive work on poisons, considered with respect to medicine and medical jurisprudence. We have only perused the first volume, which treats of the poisons of mercury, arsenic, antimony, and copper. The author has detailed many experiments on the differences which the presence of various aliments acting as reagents occasion: in the operation of poisons differences which may, in certain cases, disguise their properties, and prevent us from ascertaining them: he has pointed out all the precautions necessary for coroners, lawyers, and medical men, when the ends of justice are to be attained. He has particularly endeavoured, with the greatest care, to verify all the known methods of arresting the deleterious effects of these poisons, and to find new remedies where the old have failed. Thus, according to M. Orfila, the only antidote against corrosive sublimate is albumen or white of eggs diluted in water; and against verdigrise, common lump sugar, a result to which theory never would have led us.

M. Pictet, faithful to the interests of science, has this year communicated much valuable information connected with medicine and surgery: one of the cases he has reported would most assuredly have been regarded as a miracle, in those times when pious credulity ascribed every event to the immediate interposition of the Deity: this was the case of a man whose thorax was completely transfixed by the shaft of a gig*: another case was one in which hydrophobia seems to have been completely cured in India by repeated bleeding. The importance of this discovery was enhanced to us in France, by the circumstance of Baron Percy's having, but a few days before we knew of it, read to the Institute an account of a most dreadful occurrence at Bar sur Ornaine, where nearly twenty persons were bit by a mad wolf, and upon whom it is proposed to try the above remedy. Another case which M. Pictet communicated from Geneva did not terminate quite so happily as the first two. A soldier who laboured under all the symptoms of croup underwent tracheotomy without obtaining any relief.

M. Pictet has also communicated to us an interesting account of the plague which raged in the Russian port of Odessa, as furnished him by his nephew M. Charles Pictet, who contributed to stop its ravages.

* This and the case subsequently alluded to, must have been taken from the English Journals; they are no doubt familiar to all our readers.—Translator.

 M. Portal

M. Portal has published a most important work "upon the Nature and Treatment of Diseases of the Liver," in which he has given the result of his long experience on the affections of an organ, whose great influence in health and disease is so well expressed in the epigraph chosen by the author: *Quanto magis ad sanitatem prodest, tanto et deterius in morbis afficitur.*

Our truly respectable colleague M. Penon, who, notwithstanding a delicate temperament, and a youth which did not promise him a good old age, has preserved by care alone the faculties of his body and mind entire, long beyond the term allotted to the generality of men, has bequeathed us the secrets, the efficacy of which he so happily proved: his "Offerings to old men of some means of prolonging life," is a code of longevity dictated by science and experience; but in order to reap from it the same advantages with the author, we must enjoy, like him, a tranquil situation in life, the mild exercise of the understanding, and the consciousness of a well spent life.

It is by no means astonishing that the natural history of the animals of the deep should be so scanty as it is. Traversing at pleasure the profound element in which they escape from human observation, and even when they are caught, they are of most difficult description. M. Cuvier has presented to the Class some inquiries on such fish as have been neglected, or have multiplied beyond example in the catalogue of naturalists. One of them, remarkable for its large size, and very much known in Italy by the names of *umbra*, or *fegaro*, in Provence and in Languedoc by that of *poisson royal* (royal fish), was much better known at Paris, formerly, by the name of *maigre*. It has even given rise to some popular proverbs; nowadays, for causes of which we are ignorant, it has become rare in the Channel, and it is brought but seldom to the capital. The naturalists of the sixteenth century have described it very minutely; and Duhamel in the eighteenth has also treated of it at length. Nevertheless, our systematic authors have either given it as new, or confounded it with smaller and more common species. In addition to its exterior description, M. Cuvier has given its anatomy, and chiefly of the air-bladder, rendered curious by the ramified productions placed along its two sides.

Another species, which has been six times reproduced in the works of naturalists as so many distinct species, is a small fish of the Mediterranean, which its red colour and general form have procured the denomination of *king of the mullets*, (*mullus imberbis* Linn.) but which is more like the perch than the mullet.

M. Noel de la Moriniere, who has been occupied for several years with a treatise on the useful fishes, has presented to the Class

Class a memoir nearly of the same nature with the two former, in which he gives the history of a species very much neglected by naturalists, although so abundant at certain seasons in the Gulph of Gascony, that the fishermen of the Isle-Dieu alone take upwards of 14000 annually, weighing from 30 to 80 pounds each. It is the *germon*, or *grande oreille* of the French, or the *ala longa* of the Sardinian fishermen, (*scomber alalonga*, Gmelin,) so called because the principal character which distinguishes it from the tunny (*scomber thynnus*) consists of pectoral fins of great length and pointed. Commerson having found near Madagascar a fish which bears the same character, applied to it the name of *germon*, and was followed by Lacepede, so that the germon of Europe is now designated more particularly by the name of *ala-longa*. It remains to be shown if the germon of Europe and that of Madagascar are of different species: the distance makes this to be presumed; and Geoffroi Saint-Hilaire has ascertained the fact, by comparing the second drawing left by Commerson, with the description of the first given by M. Noel, and a drawing left by Pere Plumier. It would be desirable, however, to see this result confirmed by an actual comparison of the two fishes.

M. Cuvier has also described to the Class, a fish very little known, recently found in the Gulph of Genoa, upwards of four feet long, of the form of the blade of a cutlass, and remarkable for an elevated crest, surmounted by a kind of long horn which it has on its head, and by ventral fins excessively small, placed under the pectoral fins. There existed before but one description of it very incomplete by the late M. Giorna, naturalist, of Turin, who had given the name of *lophote* to the genus, and had dedicated the species to M. Lacepede, as an homage which all naturalists owe him.

M. Huber, of Geneva, the son of the author of a most interesting work on bees, and himself the author of a work on ants, filled with most curious traits of the instinct of these little animals, has presented to the Class a memoir on the singular industry of a small caterpillar which he calls *chenille à hamac*, from the way in which it suspends itself to pass through its chrysalis state. It lives in the inside of the leaves of some fruit-trees, and it is in the month of August that it ceases to eat, and weaves its hammock. Five hours are sufficient for constructing it: two cords stretched between the curled edges of a concave leaf are the chief supports: it is suspended by silken threads, and two others fixed to the bottom of the leaf serve it as a kind of anchor. It is of a cylindrical form. M. Huber, not contented with attentively following and describing the successive operations of the little architect who constructs this complicated

 edifice,

edifice, tried to ascertain how far these operations are connected with the instinct of the caterpillar, and may be varied by it according to circumstances. A caterpillar which he took away from the construction it was making, began it again so long as there remained any silky matter. If he placed it on a construction made by another, it continued it in general from the point where the other had left off; but if the latter was far advanced, it preferred beginning *de novo*. The butterfly which issues from the caterpillar seems to be the *phalæna clerkella* of Linnæus, and one of its enemies is the *ichneumon ramicornis*.

Our colleague M. de la Billardiere has observed a remarkable fact relative to the instinct of wild bees, or those large velvet bees which form their nests under the turf, in stones, &c. He found towards the end of autumn in a nest of the species called *apis sylvarum*, by Kirby, an old female and a working bee whose wings were glued together with brown wax, so as to prevent them from flying; and he thinks this was a precaution taken by the other bees to constrain these two individuals to remain in the nest, and to take care of the larvæ which next year were to replenish the population of the colony.

M. Olivier, member of the Class, has made on the insects which are enemies to corn, experiments which belong equally to agriculture and to zoology: he has hitherto treated of only those species which attack corn in the ear. M. Olivier describes nine of those, all belonging to the order of two-winged insects; but he describes at the same time three other insects, enemies of the above, and which of course stop their mischief.

One of the most important questions in the anatomy of insects concerns the use of a large vessel which the whole of this class carry along the back, and which undergoes movements of dilatation and contraction like those of the heart and arteries. Malpighi and Swammerdam have given it the name of heart; but it is evident from the observations of Lyonnet and several others, that no branches issue from it; and M. Cuvier seems to have proved by many experiments, that insects have no circulation. M. Marcel de Serres has taken up this subject again: he ascertained by innumerable observations made upon the largest insects in the south of France, and assisted by the most delicate instruments, that the dorsal vessel gives out no ramifications; that there exists in the body no other contractile vessel, and in general no system of blood-vessels. Insects from which the dorsal vessel is removed live for several hours, whilst scorpions and spiders, which have a real heart, perish speedily if it be destroyed. The contractions of the dorsal vessel are chiefly owing to the muscles of the back placed along its sides, but the tracheæ and the nerves exert a sensible influence on them. The humour which

which it contains appeared most frequently of a colour similar to that of the greasy matter which always fills a part of the body: it is not very liquid, particularly in the voracious larvæ. The diameter of the vessel is more equal in the larvæ, in which the fat is diffused more equally; and the inequalities of its various parts are proportioned to those of the fat in the corresponding parts of bodies. The nerves and tracheæ abound more in the dorsal vessel of the larvæ than in that of the perfect insect: its contractions are stronger, but less frequent. From these and other facts, the author thinks it may be concluded, that the function of the dorsal vessel is to produce fatty matter; and in order thereto, that it absorbs a part of the nutritive liquor diffused through the cavity of the body by the sides of the intestines; and that it makes it afterwards transude through the scales of the adipose substance, where the fat receives its final elaboration.

M. de Serres has interwoven some valuable observations on the varieties of structure of the tracheæ in different families of insects, among which we ought particularly to remark those which concern the mechanism of the vesicular tracheæ: he concludes by a detail of all the anatomical characters of the divisions which he thinks ought to be established among the articulated animals, and particularly insects. We regret that this interesting elucidation of comparative anatomy is too long for insertion in the present sketch*.

M. Montegre, a physician at Paris, has made some curious observations on the habitudes of the *lumbrici* or earth-worms, and some new remarks on their anatomy. These animals are hermaphrodites, each being productive of young: nevertheless, there is no copula, or this seems to take place without any intromission of parts, and merely by the excitement of the movements necessary for fecundation. This takes place chiefly in June and July. The worms unite by means of a swelling at the anterior part of their body, and by which they adhere firmly to each other. The young worms first show themselves in white organs placed in front, on both sides of the stomach, and slide between the intestines and external muscles along a reservoir situated in the thick part of the tail, where they are found full of life. The *lumbrici* exhibited nothing to our observer which could induce him to ascribe to them the faculty of being affected by light or sound; but he was convinced that they did not confine themselves to the earth alone, for he found in ther intestines the remains of animals and plants.

* The whole of M. Serres' valuable paper will be found in the pages of the Philosophical Magazine.– EDIT.

 XXVI. *In-*

XXVI. *Intelligence and Miscellaneous Articles.*

COUNT RUMFORD.

With regret we announce the death of that distinguished friend of science and of mankind, Benjamin Count Rumford. This lamentable event is thus mentioned in the French papers:

"Paris, August 23, 1814.

"Count Rumford, member of the French Institute, and of the Royal Society of London, died in the night between Sunday and Monday, at his country seat at Auteuil, in consequence of a nervous fever. This celebrated man dedicated his whole life to the study of the sciences, and always to the interests of humanity. He has left several works, which cannot but make his memory be cherished. He was in the 60th year of his age. His remains were interred yesterday morning at Auteuil."

MUNGO PARK.

When Mr. James Grey Jackson returned to this country from Morocco, some six or seven years since, he reported to the Earl of Moira and to Sir Joseph Banks, on the authority of an Arabic letter, which was written from Kabra, the port of Timbuctoo, by a liberated negro, to his former master, a Moorish merchant, then residing at Mogadore, that a small vessel or boat had anchored before Kabra, in the river (the Nile el Abeede), and that this boat or vessel had hoisted a white flag; that there were seen in the boat, by the inhabitants of Kabra, three Christians, one of whom was described as a very tall man, who stood up in the boat, which remained before the port of Kabra until night; but the next morning nothing more was seen of it. This boat had, as Mr. Jackson reported, no communication with the shore, but it was presumed that it had passed on to the eastward, towards Houssa.

With the foregoing report of Mr. Jackson, let the following letter from Mr. Court of Mogadore, to Mr. Mitford of the Audit Office, written not four months ago, be compared, and it will appear not unreasonable to entertain a hope that Mr. Park and two of his companions are still alive.

"Mogadore, May 15, 1814.

"A Moor, arrived this day from Houssa, informed me that with a large caravan, with which he left Timbuctoo, there were three Christians, who have been many years in the interior of Africa; that he travelled in company with them six days, and separated from them about 70 days since, the caravan taking the route to Tuart, and himself, with a part of it, to Totta; that the

the caravan would probably go near Tunis or Algiers. That the three persons appeared above the class of sailors, and were not slaves, but free to go with the caravan as the other travellers were: they had with them some papers, writings, and drawings.

"These people, the Moor says, went up the Nile or Niger in a small vessel, and were stopped; that originally their number was much greater, but they had died at Houssa, or elsewhere; and that the king or principal man at Houssa had put the three Christians under the protection of a person of note in the caravan, with directions to take them to Timbuctoo, and from thence to send them by some caravan, so that they might reach their own country.

"This account is extraordinary, as all inquiry about Mr. Park has long ceased here. The man could have no possible motive for inventing such a story; and if he did invent it, it is remarkable that he should invent a story so nearly describing, in many particulars, Mr. Park and his companions."

EARTHQUAKE.

Extract of a Letter from St. Michael's, dated the 3d of April.—"Since the alarming eruption in 1810, which shook the island to its base, we enjoyed perfect tranquillity, with the hope of its long continuance, till last week, when our fears returned with increased force, occasioned by an unusual heaving of the sea, without a breath of wind.—This was immediately followed by a rumbling noise, not unlike the report of cannon, and a strong suffocating smell of sulphur. This happened at five in the afternoon. In about a quarter of an hour the whole island appeared to be in motion; several vessels riding at anchor a short distance from the shore were dashed to pieces in an instant, and the earth opening, not a vestige of them was to be seen. On the opposite side of the island, near the village of Sylve Arbour, the ground opened in three several places, and the discharges of water issued forth from the largest aperture with great violence, the discharges continuing till midnight, when they entirely ceased. On examining the spot next morning with a friend, we found a considerable quantity of wood partly burnt, bones of animals, and heads of fishes. While we were examining these objects, a friar came up, and, having saluted us, told us that he had examined the spot, and had found, to his great surprise, a wax-cloth containing a fragment of what appeared to have been a chart, but which was so much injured by the water that it was impossible to decipher it; but on showing it to us at his house, the word "*Colum*" was still legible. As it is known by historical record, that Christopher Columbus, having been overtaken by a storm which threatened the destruction of himself and his ship, committed the account of his voyage to America to the deep, in the distant hope of reaching some habitable shore, it is conjectured here among the learned, that the wax cloth discovered by the friar is the same that Columbus is said to have put into a cask and given to the deep, when he and his crew were momentarily expecting to perish."

It appears from some recent experiments of M. Gehlen, that the best method of obtaining formic acid in abundance, is to procure in the first place a formiate of copper. For this purpose, the expressed juice of ants is to be supersaturated by carbonate of potash, and sulphate of iron at the maximum is to be poured into the liquor. The yellow liquid being filtered and evaporated to the consistence of syrup, was then distilled with a sufficient quantity of sulphuric acid. The liquor which had passed into the receiver was very acid, without containing sulphureous acid. It was saturated by means of carbonate of copper, and the solution furnished by evaporation fine blue crystals of formiate of copper. This salt was made use of for a comparative examination with the acetate of copper.

Thirteen ounces of this formiate of copper were distilled in a retort with eight ounces 310 grains of sulphuric acid of the specific gravity of 1·864. By the action of the sulphuric acid, there were obtained at a second rectification six ounces 410 grains of a distilled product, free from sulphuric acid, which possessed the following properties:

A sour and pungent odour, remaining always liquid even at a very low artificial temperature.

Its specific gravity at a temperature of 16° R. is 1116·8, whereas that of the concentrated acetic acid is only 1070·9.

200 grains of carbonate of soda deprived of water require for their saturation 1352 grains of formic acid, diluted in three parts water, and left by evaporation a residue of 228 grains in weight; whereas a similar quantity of carbonate of soda absorbed 1072 grains of acetic acid, and left 290 grains of dry residue.

The formic acid distilled with its weight of alcohol presents the same phænomena with those remarked in the preparation of acetic ether, with the exception of a very decided smell of peach kernels.

Rectified formic ether has a strong but agreeable smell, like that of peach kernels. The taste is the same, with a flavour of ants. The spirit of ants sold in the shops has also this smell, but M. Gehlen did not discover the slightest traces of prussic acid.

M. Bucholz, the German chemist, has published in Tromsdorff's Journal an analysis of the substance called Benzoin. He

He began by digesting it in common alcohol cold. Twenty-five drachms, when several times treated by alcohol, left half a drachm of woody and sandy particles. The alcoholic liquors were then mixed in a glass retort with 48 ounces of water: the alcoholic part was afterwards distilled off at a very gentle heat. The aqueous residue in the retort was filtered through bibulous paper, which retained a few flakes of a soft resin. The filtered liquid deposited on cooling two drachms and ten grains of benzoic acid in a pulverulent state. To the bottom of the vessel there adhered a thin coating of soft resin or balsam. The evaporation of the decanted liquor being continued, benzoic acid and resin were still separated: the mother water gave 16 grains of a resinous matter without any trace of extractive matter. The residue of the resin of benzoin which fringed the sides of the retort was redissolved in alcohol, and, after adding water as before, distilled: the filtered liquor again deposited benzoic acid. The weight of the acid separated by the preceding experiments was three drachms. The alcoholic product of the distillation reddened turnsole paper, a proof that some of the acid was volatilized in the alcohol. In fact, four ounces of this alcohol left to spontaneous evaporation in a cup gave one grain of benzoic acid.

The viscous matter like balsam of Peru was treated by boiling water, filtered, and evaporated. A peculiarly bitter aromatic taste distinguished the residue, which was soluble in boiling water and alcohol, but not in ether. The viscous matter which resisted the boiling water was very soluble in alcohol and ether. This last substance is *sui generis*. The purified resin of benzoin is of a brownish-red colour, pale like gumlac in tears, transparent, brittle, and gives a reddish-white dust. It is entirely soluble in alcohol and ether, but not in spirits of turpentine.

M. Bucholz obtained the following products from the 25 drachms employed:

	Drachms.	Grains.
Resin of benzoin	20	50
Benzoic acid	3	7
Substance similar to balsam of Peru	0	25
Particular aromatic principle soluble in water and alcohol	0	8
Ligneous particles and impurities	0	30
	25	

M. Bucholz then procured the benzoic acid in the following way: He poured four parts of alcohol upon one of pulverized benzoin in a matrass. After digesting a few days, the alcoholic liquor was filtered. It was then introduced into a large glass retort

retort with 12 parts of distilled water. The milky mixture was distilled until the whole of the alcohol had passed into the receiver. The boiling aqueous liquor, which swims above the resinous matter, was decanted from the cucurbite. The filtered liquor was put into earthen pots to effect the crystallization. The resinous deposit may be again dissolved in three parts of alcohol, and the liquor distilled as above, with a sufficient quantity of water. The two aqueous liquors put together are to be evaporated to an eighth part of their volume. When all the acid has been separated by crystallization, it may be purified and freed from the last particle of balsam in the following way. Dissolve it in 50 or 60 parts of boiling water in a tin or glazed earthenware vessel, and boil it a quarter of an hour with an equal weight of fresh pulverized charcoal. The filtered liquid crystallizes the benzoic acid upon cooling, in very fine white needles.

M. Monheim, a German chemist, having published an analysis of the sulphurous waters of Aix la Chapelle, in which he thought he discovered sulphuretted azotic gas in great abundance; M. Berzelius communicated his doubts on this subject to M. Monheim, who thereupon repeated his experiments. By adopting the improved process recommended by M. Berzelius, sulphur was constantly deposited; and M. Monheim admits "that in the mineral waters of Aix la Chapelle the sulphur is combined with hydrogen alone; and that if the sulphuretted hydrogen gas is not decomposed by the nitrous acid, or by the sulphurous acid, the phænomenon must be ascribed to the great quantity of azotic gas with which it is mixed."—It results from M. Monheim's last analysis, that the gas which is extricated from the sulphurous waters of Aix la Chapelle is composed of

Azotic gas	51·25 cubic inches.
Carbonic acid gas	28·26
Sulphuretted hydrogen gas	20·49
	100·00

M. Hildebrandt has recently made some curious experiments on the preservation of the flesh of animals in the gases. In a receiver of the capacity of three cubic inches, filled with very pure sulphurous acid gas, he introduced through mercury a piece of fresh beef: in a few minutes the meat had absorbed almost the whole gas, and the mercury filled the capacity of the receiver, except some air-bells, which were probably owing to the atmospheric air. The meat soon lost its natural red colour, and assumed that of boiled meat: it did not undergo any other apparent

parent alterations, and the air in the bell-glass preserved its volume. At the end of seventy-six days, during which time the temperature had varied from 0 to 10° Reaumur, the meat had scarcely acquired any smell of sulphurous acid: it was harder and drier than roasted meat. After leaving it four days in the open air, it then became more compact without being putrefied, and did not perfectly change colour: it merely lost the weak smell of acid without acquiring any other.

A piece of ox beef was treated in the same way in the fluoric acid gas, and the results were in every respect similar: the phænomena were only less visible, because the acid attacked the glass, and a thin coating of mercury was deposited on the meat.

Beef deposited in a receiver filled with ammoniacal gas exhibited alterations completely different: the absorption of the elastic fluid had taken place in it totally; the meat assumed a fine red colour nearly as in the nitrous gas, and preserved this fresh appearance for seventy-six days: it was much softer than in the foregoing experiment, without smell, and having the colour and consistence of fresh meat. When exposed four days to the open air it did not putrefy; it lost its red colour, however, became brown, dried up, and was covered with a kind of varnish.

LECTURES.

Dr. Clarke's and Mr. Clarke's Winter Courses of Lectures on Midwifery, and the Diseases of Women and Children, will commence on Tuesday, October the 4th. The Lectures are read at the house of Mr. Clarke, No. 10, Saville Row, every Morning from a Quarter past Ten to a Quarter past Eleven, for the convenience of Students attending the Hospitals. For particulars apply to Dr. Clarke, New Burlington Street; or to Mr. Clarke, at the Lecture Room, No. 10, Saville Row, Burlington Gardens.

Mr. Taunton will commence his Lectures on Anatomy, Physiology, Pathology, and Surgery, on Saturday, October the 8th, at the Theatre of Anatomy, Bartlett's Court, Holborn.

In this Course of Lectures it is proposed to take a comprehensive view of the structure and œconomy of the living body, and to consider the causes, symptoms, nature, and *treatment of surgical diseases*, with the mode of performing the different surgical operations; forming a complete course of anatomical and physiological instruction for the medical or surgical student, the artist, the professional or private gentleman.

An ample field for professional edification will be afforded by the opportunity which pupils may have of attending the clinical and other practice of both the City and Finsbury Dispensaries.

The

The Autumnal Course will commence on Saturday, October the 8th, 1814, at Eight o'clock in the Evening *precisely*, and be continued every Tuesday, Thursday, and Saturday, at the same hour.

Particulars may be had on applying to Mr. Taunton, Greville Street, Hatton Garden.

Mr. Brookes will commence his Autumnal Course of Lectures on Anatomy, Physiology, and Surgery, at the Theatre of Anatomy, Blenheim Street, Great Marlborough Street, on Saturday, the 1st of October, at Two o'clock.

In these Lectures the structure of the human body will be demonstrated on recent subjects, and further illustrated by preparations, and the functions of the different organs will be explained.

The surgical operations are performed, and every part of surgery so elucidated as may best tend to complete the operating surgeon.

The art of injecting, and of making anatomical preparations, will be taught practically.

Gentlemen zealous in the pursuit of zoology will meet with uncommon opportunities of prosecuting their researches in comparative anatomy.

Surgeons in the Army and Navy may be assisted in renewing their anatomical knowledge, and every possible attention will be paid to their accommodation as well as instruction.

Anatomical *converzationes* will be held weekly, when the different subjects treated of will be discussed familiarly, and the students' views forwarded. To these none but pupils can be admitted.

Spacious apartments, thoroughly ventilated, and replete with every convenience, are open all the morning, for the purposes of dissecting and injecting, where Mr. Brookes attends to direct the students, and demonstrate the various parts as they appear on dissection.

An extensive Museum, containing preparations illustrative of every part of the human body, and its diseases, appertains to this Theatre, to which students will have occasional admittance. Gentlemen inclined to support this School by contributing preternatural or morbid parts, subjects in Natural History, &c. (individually of little value to the possessors) may have the pleasure of seeing them preserved, arranged, and registered, with the names of the donors.

Terms.	£.	s.
For a Course of Lectures, including the Dissections	5	5
For a Perpetual Pupil to the Lectures and Dissections	10	10

The

The inconveniences usually attending anatomical investigations are counteracted by an antiseptic process, the result of experiments made by Mr. Brookes on human subjects, at Paris, in the year 1782, the account of which was delivered to the Royal Society, and read on the 17th of June 1784. This method has since been so far improved, that the florid colour of the muscles is preserved, and even heightened. Pupils may be accommodated in the House. Gentlemen established in practice, desirous of renewing their anatomical knowledge, may be accommodated with an Apartment to dissect in privately.

St. Thomas's and Guy's Hospitals.—The Winter Course of Lectures at these adjoining Hospitals will commence in the first week in October: viz.

At St. Thomas's.—Anatomy and the Operations of Surgery, by Mr. Astley Cooper and Mr. Henry Cline.

Principles and Practice of Surgery by Mr. Astley Cooper.

At Guy's.—Practice of Medicine, by Dr. Babington and Dr. Curry.—Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen.—Experimental Philosophy, by Mr. Allen.—Theory of Medicine, and Materia Medica, by Dr. Curry and Dr. Cholmeley.—Midwifery, and Diseases of Women and Children, by Dr. Haighton.—Physiology, or Laws of the Animal Œconomy, by Dr. Haighton.—Structure and Diseases of the Teeth, by Mr. Fox.

N. B. These several Lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a *complete Course of Medical and Chirurgical Instruction.* Terms and other Particulars may be learnt at the respective Hospitals.

Dr. Clutterbuck will begin his Autumn Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, on Wednesday, October 5, 1814, at Ten o'clock in the Morning, at his House, No. 1, in the Crescent, New Bridge Street, Blackfriars.

LIST OF PATENTS FOR NEW INVENTIONS.

To Thomas Tindall, of Green Street, in the parish of Scarborough, in the county of York, gentleman, for his improvements on the steam-engine with appendages thereof.—18th June 1814.—2 months.

To John Maberly, of Welbeck Street, Cavendish Square, coach-maker, and John Barrow, of York Court, East Street, Mary-le-bone, smith, for their method of securing carriage glasses.—25th June.—6 months.

To

To William Francis Hamilton, of Asylum Buildings, Westminster Road, in the county of Surry, engineer, for his improvements in the making and preparation of soda water, and other liquids impregnated with carbonic acid gas.—28th June. —6 months.

Meteorological Observations made at Clapton in Hackney from the 21st to the 31st of July 1814.

July 21.—Warm day. Cloudy morning, there being large but lofty *cumuli* in quantities. Fair evening.

July 22.—Hot hazy day, with *cumuli*, &c. The Thermometer came up to 80°. Fine evening, with red haze; a falling star left a narrow brass coloured train behind for half a second.

July 23.—Hot day, but clear and dry, with easterly wind; a few very small *cumuli* at midday.

July 24.—Hot and dry SE. wind, and clear sky. In the evenining *cirrus* scattered aloft. A few falling stars.

July 25.—Clear hot day again; wind SW. Therm. 80°. Barom. 29·98. *Cumuli. Cirrus* in the evening.

July 26.—Fair hot day; a little *cumulus* and *cirrus* in the evening.

July 27.—Fair hot day; *cumulus* and *cirrus.*

July 28.—A shower early, the day very hot. Therm. 88° in the shade; at night, rain and lightning, and Thermometer 75° at 11 P.M.—This night was everywhere noticed to be peculiarly hot and close.

July 29.—Before sunrise the great black mountainous *cumulostratus* below rows of *cirrostratus*, &c. had a beautiful effect after the rain. The Thermometer, which continued above 70° all last night, was 65° at sunrise. Afterwards it became cooler, with much cloud and wind. Mr. Sadler's balloon went from London to Chelmsford in less than an hour.

July 30.—Fair day, with *cumuli* and *cirri.* Gale from SW. and fine moon-light evening.

July 31.—Clear and thin clouds in the morning. Through the day there was a haziness, but it was fair with *cirri* and *cumuli.* At night *cirrostratus* and *cirrocumulus* by moonlight had a fine mottled appearance. The *cirrocumulus* appeared the most elevated, and had that clear and definite appearance which I noticed so often to accompany fine warm weather in summer, and which is quite different from the loose and scattered features between many common showers, as well as from those dense, compact and closely collected aggregates which precede thunder storms.

Meteoro-

Meteorological Observations made during a Journey in Wales, from the 3d to the 25th of August 1814.

Aug. 3.—(At Bath.)—Fair, and showers at intervals; fine evening.

Aug. 4.—Fair with *cumuli*, and other clouds; fine evening. SW.

Aug. 5.—Rainy morning; clear, and flying clouds afterwards. Showers again at night. SW.

Aug. 6.—Showers from SW. Fine evening with flimsy cirrocumulative *cirrostratus*.

Aug. 7.—(At Clifton.)—Cloudy, and a little rain early, then fair; hard shower in the evening. SW.

Aug. 8.—(At Ross.)—Showers and fair intervals. SW.

Aug. 9.—(At Buwlch.)—Showers as before from SW. I noticed this evening the loose cumulous clouds which supply the showers from beneath, sailing against and involving the tops of the hills as they passed along: they seemed white, and not like the scud which follows showers.

Aug. 10.—(At Aberystwith.)—Clouds and showers all day.

Aug. 11.—Rainy and fair by turms.

Aug. 12.—(At Dolgelly.)—Showery day. W.

Aug. 13.—Showery as usual. The clouds constantly on the mountain tops.

Aug. 14.—Showery morning; fair afternoon*.

Aug. 15.—Showery. W and NW.

Aug. 16.—Long continued showers. W and NW.

Aug. 17.—Showery. Westerly wind.

Aug. 18.—(At Birmingham.)—Showery. W.

Aug. 19—(In London.)—Fair; *cumuli* and *cirrostratus*; nimbiform sheets of cloud, &c.

Aug. 20.—(At Tunbridge Wells.)—Fair; *cirrus* and *cumulus*.

Aug. 21.—Fair, and occasional showers.

Aug. 22.—Fair; *cirrus* and *cumulus*. Showers at night.

Aug. 23.—Fine day; *cumulus, cirrus, cirrocumulus*. Rain at night.

Aug. 24.—Wind got to N. Rainy day.

Aug. 25.—Wind southerly again; gentle showers; clear intervals. I saw *cirrus* sailing below elevated irregular *cumuli*. Fine moon-light evening.

Tunbridge Wells,
Aug. 25, 1814.

THOMAS FORSTER.

* I ascended *Cader Idris* this afternoon. When at the top, several scud-like *cumuli*, and such as feed the showers, passed below me through the valleys. The *cumuli* in general, at least their bases, appeared very little above me; but *cirrostratus* and *cirrus* as high when seen from the top of the mountain as when viewed from below. The flimsy irregular cirrocumulative *cirrostratus* in patches seemed highest of all.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For August 1814.

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	67	80	69	30·04	78	Fair
28	70	85	79	29·94	91	Fair
29	68	74	60	·95	62	Fair
30	66	78	64	30·08	67	Fair
31	66	79	64	29·99	72	Fair
August 1	67	78	62	·95	64	Fair
2	66	75	63	30·04	66	Fair
3	65	72	59	29·95	60	Fair
4	60	73	60	·88	61	Fair
5	62	74	59	·78	54	Showery
6	60	72	56	·82	70	Showery
7	64	72	56	30·00	66	Fair
8	63	69	56	29·70	57	Showery
9	57	66	56	30·05	49	Fair
10	58	67	54	·05	36	Showery
11	56	69	60	·13	49	Fair
12	58	70	60	·12	56	Fair
13	60	69	60	29·90	60	Fair
14	60	67	54	·89	66	Fair
15	56	66	54	·85	64	Fair
16	57	63	54	·82	0	Rain
17	58	60	56	30·02	54	Showery
18	57	72	54	·07	60	Fair
19	56	66	52	29·98	54	Cloudy
20	52	65	56	30·02	60	Fair
21	56	67	54	29·92	52	Cloudy
22	56	69	57	·82	41	Showery
23	67	74	56	·72	60	Cloudy
24	58	60	55	·52	0	Rain
25	59	67	58	·64	54	Showery
26	60	71	54	·84	63	Fair

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

XXVII. *Explanation of the Cuticle of Leaves. By Mrs.* A. Ibbetson.

To Mr. Tilloch.

Sir,—I have often promised to give a thorough explanation of the cuticle of leaves, one of the most important subjects of phytology. as being the means nature employs not only to help to nourish the plant, but to bestow on it all those juices and gases the various compounds it has to form, must require: the quantities of dissections necessary to enable me to understand such a subject thoroughly, have hitherto prevented my arranging them properly; but having now dedicated many months to the labour, abstruse as it is, I flatter myself I am prepared to give it, with all its variations, in the two following letters.

It is now apparent that much of the nourishment of plants is administered by their leaves, and proceeds from the atmosphere: that it is not water alone that is thus bestowed, but every different mixture that produces that inexhaustible variety of *scents* and *juices* with which the leaves and flowers are endowed for the general benefit of man and animals. It becomes therefore of the greatest consequence to understand the manner of receiving the dews, and to point out the mechanism of each different cuticle, how it is formed and acts in this respect to be capable of retaining each separate juice, nor mix them together till nature by their conjunction is ready to produce the new compound. I have in a former letter shown that no moisture enters the leaves but through the hairs, or some projection protruded in the cuticle; but the art with which the upper skin is formed in points and bags to draw in the moisture, and the various contrivances to secure the gases, are so *wonderful*, that it will scarcely be credited by those not used to contemplate and admire the astonishing works of the Almighty. In tracing the many species of leaves, I find their cuticles vary according to the quantity of nourishment they receive and dispense to the plant; and it is also remarkable, that the pabulum of the leaves lessens in substance in exact proportion to the quantity of liquid absorbed from the atmosphere: the more hard and solid the pabulum, the less is the quantum of nourishment received from the dews.

In describing the formation of leaves, I think I proved that they proceeded entirely from the exterior of the plant, and had little or no connexion with the interior parts. This is so apparent, that a plant entirely dead within, has been known to throw out its leaves the ensuing spring. Having long before formed the leaf-bud, it has still the power *to act:* the line of life not *reach-*

ing to it, the stroke of death has not *yet attained it*. I have shown the amazing variety of juices received by the hairs and cuticles from the atmosphere. Now those who have ever decomposed the bark liquid know that it is formed of many different sorts of matter. It is very probable, then, among the variety of liquids absorbed by the hairs and cuticles, that gluten, gelatine, tannin, &c. all enter the plant in separate juices, and are received by those parts, nor unite to form the compound till they meet and compose the pabulum of the leaf, and afterwards its superabundance settles in the bark, and fills the inner bark vessels. All plants that die partly down in the winter lose their bark juices, nor regain them till after the leaves are formed: the vine, for example, the lavender, and innumerable other plants. But if the bark juices are thus formed, they begin to collect in the leaf-bud the preceding autumn; when within the gemma they retain a quantity of these juices ready prepared, and in an almost coagulated state for the shooting of the leaf-vessels. I cannot but be persuaded that this is the real origin of the blood of the plant, for in the root we most easily trace the sap proceeding from the earth. But no power that I could exert, no dissection that I could contrive, has been able to show me any other origin of the bark juices: they flow not from the earth, nor are they formed in the root; but from the *leaves* I have ever discovered *much probability* of their proceeding. All plants with respect to the different formation of their cuticles may be divided into eight sorts. Trees and shrubs receive but little nourishment from the atmosphere, their roots alone affording them plenty. Evergreens absorb none at all. Herbaceous and annuals gain a vast deal of support from the rains and dews. Bog plants hardly any in this way, and are therefore more hurt by a change of situation than any other except downright water plants; because they can in their leaves receive no indemnification for the loss of food to their roots. Sand plants receive much of their support from the atmosphere; while rock plants are wholly fed by its means, and draw no assistance from any other source: the leaves of firs are incapable of receiving moisture; the cuticles being too much like evergreens, and *too well guarded to admit it;* and as to water plants, I have already shown their general formation in my last letter. There is however much that concerns their outward skin, which will admit of some detail, and which I shall give in my next. No property of nature is more wonderful than that which vegetables possess, of adapting the cuticles of their leaves to the different situations in which they find themselves placed. This is so peculiarly the case, that I could scarcely credit the evidence of my eyes, which certified

certified the changes thus operated and caused in this part by every alteration of soil and situation. It must here be observed, that the extreme variation that has taken place in different plants, from the desire now so *universal* of obtaining in every situation and climate all the different species of flowers that can by labour and art be made to grow, and thus produce in one garden, plants of all soils, of every climate, and every degree of moisture, rock, sand, bog, water, herbaceous plants and trees: this has so mixed the cuticles, that their habits are no longer to be known by their outward appearance or form; indeed they by degrees obtain new ones. I know few plants (except *real* water plants and bog plants) that obstinately retain their customs, and die if placed in an improper soil. The only means I have been able to devise, to *comprehend this* subject, and study it completely, was to collect a few of each different *sort* that still retained their original properties unchanged by custom, and make myself thoroughly acquainted with their *exact* and *natural formation*; and when that was accomplished, to dissect those which habit has altered. This I have done. A dozen plants of each were sufficient to complete the task I had in view; and I flatter myself I shall now be capable of discriminating clearly the variations that take place, and give an exact account of them.

The leaves of trees and shrubs which are little indebted to the atmosphere for support, consist of three upper cuticles and two under ones; I shall begin with the lowest part of the leaf: the under cuticle is a skin perfectly impervious to water, while the next is a net which has on it bladders alternately placed in diamonds or otherwise; these rise high, and therefore keep the cuticle hollow from the pabulum of the leaf: these bladders are very succulent, and cannot therefore bear the heat without being destroyed and absorbed; nor can the cuticle, hollow and dry as it is, support the exposure to the sun without being liable to peal off, tear and break: hence the reason why, when once the leaf is dried, it never again turns its back to the sun. Almost every leaf is formed in the same manner in its under cuticle: next to this skin comes the pabulum, which is thick or thin according as the leaf is so; for it is the increase of this part alone which thickens a leaf. On each side of the pabulum the veins are to be discovered: these contain the spiral wire: then follow the three upper cuticles, the impervious one being in the middle: (see Plate III. fig. 1.) Elm leaf dissected: when there are hairs, they not only pass through the first skin, but pierce to the pabulum, while the various divisions connect themselves with the different vessels of the cuticle, see fig. IX. This is admirably seen by taking pains to adjust properly the compound micro-

 scope,

scope, which if well managed throws the cuticles to their focal distance, showing the cylinders of the hairs between, and thus dividing the cuticles when they are too close for either lancet or finger to do it. It may be observed of all leaves, that the young ones have much more hairs than the old, and I may say that it is difficult to find a plant even of trees when the buds are in their aggregate state, whose leaves are not loaded with hairs, and often covered with a sort of net of that kind, which entirely disappears in the more advanced age of the plant: nor does less alteration take place in the edge of young leaves, which generally change in a very extraordinary manner. This is not to be seen by the naked eye, but is very conspicuous to the dissector, who, in all these changes, finds one general system running throughout nature, and tending to procure to the young *élève* plenty of nourishment, (see fig. III. The sides of young leaves, when compared with old ones of the same plant.) There is also another peculiarity in which the leaves of trees differ much from those of herbaceous plants; the pabulum is not only more solid, but their cuticles lie much closer to each other, and the older the leaves grow, the more perceptible is this circumstance.

In herbaceous and annual plants, and indeed all those which rise each year from the earth, the pabulum is less solid than in the leaf of trees, and more watery; they have infinitely more hairs, and therefore receive much more moisture from the atmosphere; but they have in general but one set of veins, which lie next the under cuticle, and cause it to be much more raised and pulpy in its upper surface than the leaf of trees: it is this that gives the leaves of the *melissa* and *mentha* that peculiar look. It is this also which gives the cabbage the same; and it is merely the drawing in of the spiral wire which produces in the *brassica* that curious effect on a cold morning. I have taken it within doors very often, and seen it almost smooth; when returning it to the frost, it has directly shrunk up into absolute little nests, by means of the contraction of the spiral wire in the several divisions which are drawn in like a high quilting.

I have in vain tried to discover the reason for these leaves having but single veins, as I doubt not some curious cause is attached to it; but it requires much time to develop all these peculiarities and minute differences, and watching alone can discover them.

I gave in my last letter a specimen of the sort of cuticle of hairs that often covers annuals and herbaceous plants, that will continually spread over both the upper and under surfaces of the leaves, and that had a bottle attached to it with a curling vessel twisting like the spiral wire. Many different sorts might be given as curious as that; it is inconceivable how much moisture they

they contain, and how many various juices are thus produced to the plants; for it is by no means to be conceived that *one sort of hair always brings* the same juice. I have proved how very differently this is managed in seeds, by seeing and detecting the same hairs bringing not only various coloured juices, but changes of gases, known to be so by the alteration of tints of the air-bubbles: at one time the vessels would be full, *then changing;* every day produce a new ingredient. I doubt not it is the same in all *other plants* which receive moisture and support from the leaves.

Evergreens receive no moisture whatever from the atmosphere. Besides the impervious skin which forms their outward cuticles both above and below, they are so guarded over the pabulum by a thin layer of bark juices, and bladders of inflammable air, that no moisture can approach them in that direction. They have five upper cuticles and three under ones, all so closely pressed together, as apparently to form but one: those that have scented leaves have in the second cuticle bladders of an oily compound: this evidently enters from the atmosphere, and it is thus all leaves are scented, the perfume being kept perfectly separate from the rest of the juices of the plant. The scent would be strong indeed if it could be taken thus: but this is impossible, and by distillation we again separate it from the general liquid, with which it gets mixed in drawing it from the plant. It is however greatly to be regretted that we cannot contrive at once to draw the several liquids from a plant without this assemblage, as it would give a much clearer view of the subject. It will be asked, As these leaves have *no hairs*, and no *collecting cuticle*, how they can receive the oil and all the various juices they must require to form the compounds and gases necessary to their health and vigour? I know but of three means: the *edges of the leaves;* the large open points common to all evergreens; and the top of the gatherer; for in these leaves it is only the under part that draws up; the other is studded with pores formed in a curious manner *within;* and as this formation is only to be discovered in these leaves, I take it to be some indemnification for the very few hairs they possess. It must however be noticed, that, like almost all other plants, when first shooting they have many hairs. The extreme shining of the upper cuticle is caused by the glutinous matter which fills up like oil the network of the exterior one, and renders it perfectly lucid; and it is this hard and stiff skin, and the bladders of inflammable air, which cause it to explode when placed in the fire; and it is this glutinous cuticle which protects it from the cold of the winter; and in proportion as these glazed skins are many in number, so much more will they endure the frosts of the winter. This is the

 reason

reason why the birch will grow so much further north than any other tree: its rind is composed of an innumerable quantity of those skins: if it was studded with pores, as all leaves and cuticles were formerly supposed to be, nay, if there were only *a few*, it would destroy the whole mechanic formation of the leaf: therefore, as soon as the exterior of an evergreen leaf gets pierced by an insect, it decays directly.

All those grassy leaves of early spring have a peculiar formation: they receive *no moisture*, or *very little, from the atmosphere*. The crocus, snow-drop, hyacinth leaf, &c. are all filled with a glutinous liquor in their pabulum, which in a great measure protects them from the early frosts of spring; they have three cuticles above and three below—the upper skin being ribbed like a grass leaf, and carrying in stripes the spiral wire; and it is the pabulum, which is in pockets, and stands at a great distance, while the cuticles are merely longitudinal vessels, which differ from the gramina only by having a *pabulum:* they have three cuticles on each side, which are scolloped; and of course they have *no veins*, as their place is supplied by the exterior cuticle. Most of the bulbous roots have the thickened juices, *which are formed to encounter the difficulties* of the early spring, and thus carry in their bulbs a great quantity of nourishment ready prepared. There is a peculiarity in these plants which deserves much study: most of them will shoot without the assistance of any earth; and yet it is evidently not from the quantity of nourishment they receive from the atmosphere, (for that is hardly any thing,) but from that which is laid up and accumulated in the bulb; and from some moisture they have the art and mechanism to draw in at the end of the roots, which will always be found wet when growing on a dry floor, or on stones: but this must be a trifling quantity. These bulbous roots, then, form a third means of support to plants, and not the least curious of the three.

I hope I shall be able to give a more complete picture of the effects of these plants growing without earth, from some trials I am now engaged in. The astonishing difference that may be made in the appearance of bulbous flowers by lessening or increasing the nutriment offered, is well known to most gardeners: it requires, however, the *nicest management:* on the essential characters it has *no power;* for although, by the multiplication of the petals or other parts, the more consequential organs are destroyed; yet these changes affect the *genera* more than the *specific distinctions;* and notwithstanding the number of these artificial varieties, such is the tendency of nature to maintain a uniformity in the individuals of the same species, that a neglect of the gardener for a few years will soon bring all these gaudy double

double and unnatural flowers back to their ancient beautiful simplicity. But this is not my business: to understand how far a bulb will maintain itself without any extraneous aid; whether in this situation it is capable of giving life to others, as well as to support itself, and how far this may be carried in bulbs; and to examine whether the seed can draw its source from itself, or whether (like all other plants) it is created in the roots by the joint juices of the sap of the earth and *mother plant*, is what I am most anxious to discover; also how long the nutriment laid upon a bulb will last, and what it loses in weight. I must here observe, that when it is not a bulb, the cuticles of the grassy leaves differ extremely.

Of all the cuticles of plants I have been studying, none have puzzled me like the bog plants. They certainly differ much both from rock and sand-plants; for they appear to receive almost all their nourishment from the root, and to depend on them so much as to die if removed from that soil; and yet they are almost always to be discovered collecting moisture from the atmosphere, especially the *scirpi*, and most particularly the *junci*. All these certainly draw-in moisture from the sides of their leaves; as do all the *azalias*, which have the most curious mechanism for that purpose. Now to require in so extreme a degree both the nourishment of root and leaves, is an uncommon circumstance; but I rather fancy it is not the *moisture* of the bogs *that is required*, but the *peculiar earth* with which it is *formed*. It appears indeed far more easy for *nature* to supply her wants of moisture or dryness by the change of cuticle, than to do without the sort of earth she is accustomed to; and there appear for all bog plants some ingredients in the earth necessary to their well doing, and in which alone they can thrive. To discover this is a matter I have much at heart, not merely to make one set of plants *do well*, (though that would be a sufficient incentive,) but as producing *another fact of essential consequence* in our knowledge of the formation of plants. What this portion of earth is, or what part of the bog matter it may be, I am endeavouring to discover, having analysed the earth, and extracted by turns each separate ingredient. But as it is necessary that the plant should *grow in it*, it requires many years to ascertain the fact, and complete the trial. Bog plants of the *azalia* kind have three cuticles above, and two below, with a thick pabulum not at all watery; and this convinced me, they could not draw much water from the atmosphere: for this appears an invariable rule, that the more moisture the leaves take in, the more loose and juicy is the pabulum. Most of the bog plants have a very raised and glittering upper cuticle of the net

 kind,

kind, which I doubt not has often deceived botanists, and been taken for perspiration; whereas it is so perfectly without moisture, that the longer the leaves are kept within, and the more they are dried up, the more shining and brilliant it is. The yellow *azalia* will live and do well out of bog earth; but I believe the others almost always linger and die away. The cuticle of the leaves of the *yellow azalia* differs much, especially one always growing in common earth: but as I keep the variation to be discovered by a change of situation and soil, for my second letter, I shall not anticipate it here, except by saying that it has more hairs, and not so thick a pabulum, nor are the leaves so generally rigid.

I shall now turn to the rock plant, one of the most curious of all, as drawing hardly any nourishment from the earth. It is the only plant formed without the *impervious skin*, and has therefore millions of mouths open in every leaf ready to suck in the moisture of the atmosphere. The upper cuticle is entirely *composed* of diminutive round swellings; and these are full of *points*, which absorb the moisture by means of the vacuum managed within; and the cuticle, being at some distance from the pabulum, is nothing but plain water, confined in skins with each a pore that opens and closes, *contracted* by the *spiral wire*. Real rock plants have no hairs. Used to live in alpine situations, on the Table mountain of the Cape, or on some high rocks, where the atmosphere is constantly bestowing vapour which feed and bathe them, they scarcely want the little appendage that often belongs to the minor ones, which creep over the rocks, and seek the little cavities of the stones. This is a small kind of pump, something like that which belongs to parasite plants: it seems merely intended to keep the root-stems moist, for the running-up of the seeds. All the leaves are very thick and succulent; two cuticles above and the same below, but most of the leaves flat on one side; they have (like all other plants) hairs when young. The ice plant is not a proper epitome of a rock plant, because it has its pabulum at the *exterior* of the outward cuticle, instead of the middle of it. But most of the *sedums* are so, and many of the *saxifragas* and *sempervivums;* the *draba acyoides, erinus alpinus*, and innumerable others: most of these would soon die, if deprived of the moisture of the atmosphere. Though these plants have few if any hairs, yet they have points of various kinds which produce the same effects; that is, bestowing on the plant those peculiar juices without which it cannot be supported. To receive these oils and liquids is a very different matter from gaining *sustenance* from the leaves: nor can the dissector mistake them, as the colour alone, as well as the

the forms of the hairs which contain the juices, will explain what they are. I doubt not that most of the plants are much altered from their first appearance when growing in their original mountains. Though I have taken all the plants I have dissected from as appropriate situations as it was possible to procure them, yet rock plants in particular must be very inferior in their cuticles to what they would be could I have drawn them from their native and original places; most of the *cactus* and melon thistles are of this kind; they grow on the steep sides of rocks in the hottest parts of America, where they seem to be thrust out of the aperture, having scarcely a morsel of earth (and often none at all) to assist them, though they are such large plants; their roots shooting down into the fissures, and thus running to a considerable depth, so that it is extremely difficult to procure the plants. The roots appear to seek water only, for which their little pumps serve them not alone for the purpose of clinging to the rocks, but to moisten and invigorate the lower part of the plants.

It will be seen that though many of the vegetable tribe receive no nourishment from the atmosphere, yet they all have the means of procuring the bark juices in their various different ingredients. I before showed how many sorts of hairs each plant possessed—long indeed before I had an idea that those separate liquids, when collected and coagulated, formed the pabulum of the leaf, and the bark juices. I may truly say that in this case (as frequently before) nature has developed herself. I have only had to show what appears in the microscope; all the rest arranges itself almost without my assistance, as too evident to be misunderstood. I shall now close the present letter with the account of the dissections that are intended to explain it; and in my next I shall conclude the subject with the sand, fir, and cryptogamia plants; and with the variations each change of soil, of moisture, or dryness, beyond their usual allotment, produces in the cuticle.

To give only a faint idea of how necessary all these matters are to elucidate the knowledge of deeper *philosophers*, I shall only mention, that all those curious trials established (I think) by Sennebier, for want of it are *useless;* since the various experiments he made to make plants grow in different *matters* and *soils* were tried with plants that do not draw their nourishment from the *roots*, but from the atmosphere: therefore most of those that really succeeded may be supposed to have done so because they touched not, nor interfered with, the soil bestowed on them. It is so necessary in all these trials to understand *why* we fail, and *why* we prosper. It is certain his attempt could draw no result to be depended upon, and to be of real

use

use to science: no plants in this case should be tried, but trees, shrubs, and evergreens, receiving little in the first and no nourishment from the atmosphere in the last case. I cannot but flatter myself that this will open a larger field for understanding plants; and so much is it my desire to know the different vegetables that prefer each peculiar soil, and the reason why they do so, that I cannot but hope the want of ability may be compensated by excessive diligence and attention.

I am, sir,

Your obliged servant,

Bellevue, August 6, 1814. AGNES IBBETSON.

PLATE III.

Dissections of the Leaves of Trees.

Elm Leaf dissected.

Fig. 1, first cuticle; fig. 2, third cuticle (the second being impervious); fig. 3, veins of the leaf containing the spiral wire; fig. 4, the pabulum of the leaf; fig. 5, the under cuticle with the bubbles; fig. 6, leaf shown in thickness.

The Leaf of a herbaceous Plant dissected.

Fig. 7, first cuticle; fig. 8, third cuticle (the second being impervious); fig. 9, pabulum; fig. 10, the veins of the leaf containing the spiral wire; fig. 11, thickness of the leaf.

Dissections of the grassy Leaves of early Spring.

Fig. 12, first cuticle; fig. 13, third cuticle (the second being impervious); fig. 14, pabulum; fig. 15, thickness of the leaf.

Dissection of the Leaves of Evergreens.

Fig. 16, shown in the breadth of the leaf.

Dissections of the Leaves of the Rock Plants.

Fig. 17, the upper cuticle; no impervious one, being the only set of plants that are without it.

Fig. 19, the swellings in the upper cuticle; fig. 18, the holes or *apertures* which open into *the swellings*: they have certainly a valve at *a a*, and are most curiously formed: this made me suppose that there must be an impervious skin; but, if there is, it is so thin that I could not discover it.

Fig. IX. one of the hairs, to show how they manage the boxes or reservoirs under them, and between the different skins or cuticles: it may be thought puerile to mention such spaces, but they are by no means useless to nature; since probably it is in these

these the juices are all collected to form the pabulum. The smallest veins of a leaf are larger than the veins and arteries of a mite, and what use does not nature make of all those!

Fig. 24 and 25 will give an idea of the means used to increase the given moisture to young leaves: 24, is the edge of an old and young leaf: it is not only increased to the young one in an amazing quantity of hairs, but an additional piece (*bb*) is formed, and added to the leaf (quite as large as the leaf is broad) to give an increase of moisture. No words I can make use of will paint the extreme difference between the *old* and *young leaves* of most plants; they must be examined to prove this truth. In the birch, for example, they are not in the least alike either in shape, size, texture, or edge.

25 is the increase of the edge of one of the *idenea* in a young leaf, which in the old one ends by being a plain scollop with points, and no hairs.

Fig. 20, 21, 22, 23, and 16 are the dissection of the evergreen leaf.

XXVIII. *Report from the Select Committee on Weights and Measures. Ordered by the House of Commons to be printed, July* 1, 1814.

YOUR committee, in the first place, proceeded to inquire what measures had been taken to establish uniform weights and measures throughout the kingdom. They found that this subject had engaged the attention of parliament at a very early period. The statute book from the time of Henry the Third abounds with acts of parliament enacting and declaring that there should be one uniform weight and measure throughout the realm; and every act complains that the preceding statutes had been ineffectual, and that the laws were disobeyed.

The select committee of the house of commons, which was appointed in the year 1758 to inquire into the original standards of weights and measures in this kingdom, and to consider the laws relating thereto, made a very elaborate Report on this subject, in which is contained all the information that is necessary with regard to the inquiry into what were the original standards of weights and measures: such parts of that Report as have appeared to your committee to be most important, are inserted in the Appendix to this Report.

The committee of 1758 first give an abridged state of the several statutes which have been enacted relating to weights and measures: 1st, so far as they establish any weights or measures, or standards for the same; and 2d, so far as any means, checks,

or

or sanctions are provided to compel the use of the established weights and measures, or to punish disobedience. The committee then point out what appeared to them to be the principal causes which had prevented the attainment of that uniformity, so much and so wisely desired by parliament. These are stated to be the want of skill in the artificers who from time to time made copies of the standards kept in the Exchequer; and as these imperfect measures were again copied from, every error was multiplied, till the variety of standards rendered it difficult to know what was the real standard, or to apply any adequate remedy.

In the second place, the multiplicity of statutes made on this subject, many of which are at variance with one another, and in many of which there are partial exceptions of particular counties, and particular articles, from the operation of the acts, appeared to the committee to be the principal cause of the various errors which were every where found to prevail.

Upon an accurate comparison of the various measures preserved in the Exchequer, and which are directed to be used for sizing and adjusting all other measures, they were found to differ materially from each other, and yet (the committee observe) as the law now stands, all these measures must be understood to contain the like quantities, are equally legal, and may be indiscriminately used.

Of these various measures, the committee recommend the adoption of the ale gallon of 282 cubical inches, and to abolish the use of all the others. They also recommend that the troy pound should be the only standard of weight. Though your committee agree entirely wish the Report of the committee of 1758, that there should be only one gallon for measuring all articles whatsoever, and only one denomination of weight; yet they cannot concur, for reasons which will be hereafter stated, in the selection made by that committee in appointing these standards.

This Report was agreed to by the house; and in the year 1765 two bills were brought in by Lord Carysfort, who was chairman of the committee of 1758, for the purpose of carrying into effect the resolutions of that committee. These bills were severally read a first and second time, and committed; and the bills, as amended by the committee, were ordered to be printed on the 8th day of May. Parliament was however prorogued in that year on the 25th day of May; and these bills, which (as far as can be collected from the Journals) were approved of by the house, were thus unfortunately lost.

Since that period, little has been done to accomplish this important object. A committee was indeed appointed in the year 1790;

1790; but they do not appear to have made any progress, as your committee have been unable to find any minutes of their proceedings.

Two acts were passed in the years 1795 and 1797, the 35 Geo. III. cap. 102, and 37 Geo. III. cap. 143, which empower justices of the peace to search for and destroy false weights, and to punish the persons in whose possession they are found; but no mention is made in these acts of deficient measures.

Your committee now proceed to state what appear to them to be the principal causes which have prevented the establishment of uniform weights and measures; and to state the reasons which have induced them to differ from the committee of 1758 in some of their resolutions.

It appears to your committee, that the great causes of the inaccuracies which have prevailed, are the want of a fixed standard in nature with which the standards of measure might at all times be easily compared, the want of a simple mode of connecting the measures of length with those of capacity and weight, and also the want of proper tables of equalization, by means of which the old measures might have readily been converted into the new standards. Some rude attempts seem to have been made to establish a mode of connecting the measures of capacity with weight. In an act of the 51st of Henry Third, intituled, "Assisa Panis et Cervisiæ," it is declared "that an English penny called the Sterling, round without clipping, should weigh 32 grains of wheat, well dried, and gathered out of the middle of the ear; and 20 pence to make an ounce, 12 ounces a pound, eight pounds a gallon of wine, and eight gallons of wine a bushel of London."

Nothing however can be more uncertain and inaccurate than this method of determining the size of a gallon measure by the weight of a certain number of grains of wheat, which must vary according to the season and the nature of the soil and climate where they are produced.

In order to obtain some information as to what were the best means of comparing the standards of length, with some invariable natural standard, your committee proceeded to examine Dr. W. Hyde Wollaston, Secretary to the Royal Society, and Professor Playfair of Edinburgh.

From the evidence of these gentlemen, it appears that the length of a pendulum making a certain number of vibrations in a given portion of time, will always be the same in the same latitude; and that the standard English yard has been accurately compared with the length of the pendulum which vibrates sixty times in a minute in the latitude of London.

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The length of this pendulum is 39·13047 inches, of which the yard contains 36.

The French government have adopted as the standard of their measures, a portion of an arc of the meridian, which was accurately measured. The standard metre, which is the 10,000,000th part of the quadrant of the meridian, which is engraved on the platina scale preserved in the National Institute, has been compared with the English standard yard, by Professor Pictet of Geneva, and was found to exceed it, at the temperature of 32°, by 3·3702 inches; and at the temperature of 55°, by 3·3828 inches.

The standard yard may therefore be at any time ascertained, by a comparison either with an arc of the meridian, or the length of the pendulum, both of which may be considered as invariable.

The standard of linear measure being thus established and ascertained, the measures of capacity are easily deduced from it, by determining the number of cubical inches which they should contain. The standard of weight must be derived from the measures of capacity, by ascertaining the weight of a given bulk of some substance of which the specific gravity is invariable. Fortunately that substance which is most generally diffused over the world, answers this condition. The specific gravity of pure water has been found to be invariable at the same temperature; and by a very remarkable coincidence, a cubic foot of pure water (or 1,728 cubical inches) at the temperature of 56½° by Fahrenheit's thermometer, has been ascertained to weigh exactly 1,000 ounces avoirdupois, and therefore the weight of 27·648 inches is equal to one pound avoirdupois.

This circumstance forms the groundwork of all the succeeding observations of your committee.

Although in theory the standard of weight is derived from the measures of capacity, yet in practice it will be found more convenient to reverse this order.

The weight of water contained by any vessel affords the best measure of its capacity, and is more easily ascertained than the number of cubical inches by gauging.

Your committee therefore recommend that the measures of capacity should be ascertained by the weight of pure or distilled water contained by them, rather than by the number of cubical inches, as recommended in the 4th resolution of the committee of 1758.

Your committee are also of opinion, that the standard gallon, from which all the other measures of capacity should be derived, should be made of such a size as to contain such a weight of pure water of the temperature of 56½° as should be expressed in in a whole number of pounds avoirdupois, and such also as would admit of the quart and pint containing integer numbers of ounces, without any fractional parts.

If the gallon is made to contain 10 pounds of water, the quart will contain 40 ounces, and the pint 20.

This gallon recommended by your committee, will contain 276·48 cubical inches, being nearly 3 per cent. larger than the gallon or 8th part of the Winchester bushel as fixed by the act of the 13th and 14th of William and Mary, cap. 5, which contains 268·803 cubical inches, and the bushel will contain 2211·84 cubical inches, instead of 2150·42.

Your committee are of opinion, that this departure from the corn measure, which is employed in the collection of the malt tax, and is supposed to be most generally used throughout the kingdom, is justified by the advantages which they anticipate from the change.

General uniformity cannot be expected, unless some simple and accurate method of checking the standard measures is adopted; and as the weight of water appears to be the best and most simple method of checking measures of capacity, it is desirable that all minute fractions of weight should be avoided. There will be much less chance of error in weighing the water contained in any measure, if only one or two weights are to be employed, than if a greater number were necessary, which would be the case if fractional parts were required.

The difficulty of remembering long fractions is also an important consideration, and would very materially impede the attainment of that general uniformity which is so much desired.

If this gallon is adopted, the bushel will contain 80 lbs. of water, or 2211·84 cubical inches; the quart 69·12 cubical inches or 40 ounces of water; the pint 34·56 cubical inches or 20 ounces of water: the half pint will contain 17·28 cubical inches (which is exactly $\frac{1}{100}$th part of a cubical foot) or 10 ounces of water. Any smaller measures might with great propriety be described, according to the number of ounces of water they contained.

Your committee are of opinion, that the simple connection which will in this manner be established between the standard of weight and measures of capacity, will greatly tend to preserve the uniformity of those measures which are found to be most liable to error. They have, besides, been induced to select this size of measure as a standard, both because it very nearly coincides with one of the standard corn measures preserved in the Exchequer, namely the standard pint marked 1602, which contains 34·8 cubical inches; and also because it possesses the

the advantage of bearing very simple relations to the gallon measures which are employed in measuring ale and wine. The calculations therefore which would be necessary for ascertaining the corresponding duties, which must be charged upon exciseable liquors, would be easily made.

The proportions between the proposed gallon and the measures now in use, will be found in the Appendix.

Your committee will now proceed to state their opinion with regard to the standard of weight. Although the troy pound is the only weight established by law, yet the avoirdupois pound is so much more generally known and used, that your committee cannot hesitate to recommend it in preference to the troy pound. Your committee are however inclined to recommend two exceptions: As the goldsmiths regulate their traffic by the troy weight; and considering the connection of this branch of trade with the standard of the coin, it appears advisable that gold and silver should be sold by troy weight.

Your committee would also recommend that the apothecaries weight should remain without any alteration, as any change in the weights employed by them in compounding their medicines might be attended with dangerous consequences.

It appears to your committee that the most accurate mode of ascertaining the standard pound, is to immerse in water a solid cylinder of brass containing 27·648 cubical inches, and to ascertain the difference between its weight in water and its weight in air, by means of the common hydrostatic balance. The difference between its weight in water and its weight in air (or the weight of the volume of water occupying the same space) is the pound avoirdupois.

This method is recommended, as it has been found to be much more easy to ascertain the solid contents of any body, by taking its external dimensions, than to find the exact contents of any measure by gauging.

In this manner, the standard of length is kept invariable, by means of the pendulum; the standard of weight, by the standard of length; and the standard of capacity, by that of weight.

Your committee have not gone into any detail of the inaccuracies of the present system, as they are very fully stated in the Report of the committee of 1758, before alluded to.

In order to preserve uniformity in the weights and measures to be used in future, your committee would recommend that no person be allowed to make or sell any weights or measures without having obtained a license for that purpose, on payment of a certain sum; and that all weights and measures should be marked with the name of the maker, and the initials of the person who examines them. The person who last examines the weights

weights and measures, should not be permitted to alter them; but, if he finds them to be incorrect, should return them to the maker, to be properly sized and adjusted. All new weights and measures will therefore in this manner undergo a double examination. Your committee are of opinion, that the powers given to justices of the peace by the acts of the 35 Geo. III. cap. 102, and the 37 Geo. III. cap. 143, to search for and destroy deficient weights, and to punish the persons in whose possession they are found, should be extended, so as to give them the same powers with regard to false or deficient measures.

Your committee cannot conclude without stating the opinion of Professor Playfair, with regard to the necessity of adhering strictly to one simple and general view in appointing the standards, without departing from it for the sake of accommodating individuals or particular classes of the community. The advantage of the public at large cannot be consulted, unless this rule is rigorously observed. The simplicity and accuracy of the system would be sacrificed by any partial exceptions; and it may be considered as certain, that unless the rules are simple, and the constructions independent of minuteness of division, an opening will be left for fraud, and for all the perplexity in which the standards of the country are at this moment involved.

1. *Resolved*, That it is the opinion of this committee, that it is necessary, in order effectually to ascertain and enforce uniform weights and measures to be used for the future, that all former statutes relating thereto should be repealed.

2. *Resolved*, That it is the opinion of this committee, that the distance between the two points in the gold studs in the brass rod, described in the Report of the select committee of 1758, and preserved in the custody of the clerk of this house, ought to be the length called a yard; and that one-third part thereof should be a foot, and the 12th part of the foot one inch.

3. *Resolved*, That it is the opinion of this committee, that the length of a pendulum vibrating 60 times in a minute of time, in the latitude of London, has been ascertained to be 39·13047 inches, of which the standard yard contains 36.

4. *Resolved*, That it is the opinion of this committee, that all measures of length whatsoever should be taken in parts, multiples, or certain proportions of the said standard yard.

5. *Resolved*, That it is the opinion of this committee, that a cubic foot of pure water at the temperature of $56\frac{1}{2}°$ has been ascertained to weigh exactly 1000 ounces avoirdupois.

6. *Resolved*, That it is the opinion of this committee, that all measures of capacity should be ascertained by the weight of

water therein contained, as well as by the number of cubical inches.

7. *Resolved,* That it is the opinion of this committee, that all measures of the same denomination, whether of liquids or of dry goods, ought to contain the same weight of water, and the same number of cubical inches.

8. *Resolved,* That it is the opinion of this committee, that the gallon ought to contain 10 pounds of pure water, or 276·48 cubical inches; that the quart, or fourth part of the gallon, ought to contain 40 ounces of water, or 69·12 cubical inches; that the pint, or half of the quart, ought to contain 20 ounces of water, or 34·56 cubical inches.

9. *Resolved,* That it is the opinion of this committee, that the bushel ought to contain eight of the said gallons, or 80 lbs. of water, or 2211·84 cubical inches; and that all other measures of capacity ought to be taken in parts, multiples, or proportional parts of the said gallon.

10. *Resolved,* That it is the opinion of this committee, that the standard of weight ought to be the pound avoirdupois, which is equal to the weight of 27·648 cubical inches of pure water of the temperature of $56\frac{1}{2}°$; that the 16th part of the said pound should be an ounce, and the 16th part of such ounce should be a drachm; that the third part of the drachm should be a scruple, and the tenth part of the scruple one grain; and that all other weights should be taken from parts, multiples, or proportional parts of this pound.

11. *Resolved,* That it is the opinion of this committee, that all contracts, bargains, sales and dealings, ought to be taken and adjudged to be according to the standards aforesaid; and that no person should recover the price of goods sold, or the goods themselves, or any damages on account of any contracts, bargains, sales or dealings, but according to the said standards.

12. *Resolved,* That it is the opinion of this committee, that it ought to be penal for any person to have in his possession any measure or weight that is not agreeable to the aforesaid standards.

13. *Resolved,* That it is the opinion of this committee, that it ought to be made highly penal for any person to make or sell any measure or weight that is not agreeable to the aforesaid standards.

14. *Resolved,* That it is the opinion of this committee, that for enforcing an uniformity in the weights and measures to be used for the future, no person should be permitted to make weights and measures, without having first obtained a proper license for that purpose, on payment of a certain sum.

15. *Resolved,* That it is the opinion of this committee, that all

all weights and measures to be hereafter made, ought to be marked with the name of the maker; and, after a proper examination of the weight or measure, the same to be stamped with the initials of the name of the person who has examined it.

16. *Resolved*, That it is the opinion of this committee, that all weights exceeding one pound should be made of brass, copper, bell metal, or cast iron; and that all weights of one pound, or under, should be of gold, silver, brass, copper, or bell metal.

17. *Resolved*, That it is the opinion of this committee, that the standard yard mentioned in the second resolution, and a pound avoirdupois, made according to the directions before mentioned in this Report, together with models or patterns of the measures of capacity before mentioned, ought to be deposited in the Court of the Receipt of the Exchequer, and there safely kept under the seals of the Chancellor of the Exchequer, and of the Chief Baron, and the Seal of Office of the Chamberlains of the Exchequer, and not to be opened but by the order of the Chancellor of the Exchequer and Chief Baron for the time being.

18. *Resolved*, That it is the opinion of this committee, that models or patterns of the said standard yard, gallon, and pound avoirdupois, and of the parts and multiples thereof, before mentioned, should be distributed in each county, city or corporate town being a county within itself, in such manner as to be readily used as evidence, in all cases where measures and weights shall be questioned before the justices of the peace for each county or city, and for adjusting the same in a proper manner.

19. *Resolved*, That it is the opinion of this committee, that the provisions of the acts of the 35 Geo. III. cap. 102, and the 37 Geo. III. cap. 143, should be extended, so as to empower justices of the peace to search for and destroy false measures as well as false weights, and to hear and determine and put in execution the law with regard to weights and measures, and to inflict or mitigate such penalties as shall be thought proper, and to have such other authorities as shall be necessary for compelling the use of weights and measures agreeable to the aforesaid standards.

20. *Resolved*, That it is the opinion of this committee, that the sheriff of each county ought to be directed to summon a jury of twelve sufficient men living within the county, to return a verdict, on the comparison to be made before them of the proportions which the new standards bear to those formerly in use in each county respectively; and according to the verdict then returned, Tables of equalization should be made, and copies of the same should be distributed through each county; and that

all existing contracts or rents payable in corn should be calculated according to these Tables of equalization.

Dr. Wollaston in his examination before the committee made the following remarks:

There is one standard of capacity that would be *particularly* advantageous, because it would bear simple proportions to the measures now in use, so that one of the great inconveniences arising from change of the standard would be obviated, by the facility of making many necessary computations without reference to Tables.

If the gallon measure be defined to be that which contains 10 lb. of water at $56\frac{1}{2}$,

Then since the cubic foot of water weighs 1,000 oz. at $56\frac{1}{2}$,

$\frac{1}{2}$ pint $= 10$ oz. $= \frac{1}{100}$ of cubic foot $= 17\cdot28$ inches.
Pint $= 20$ oz. $= 34\cdot56$.
Bushel $= 80$ lb. $= 2211\cdot84$.

And the simple proportions above alluded to will be found as follows:

	Cubical Inches.	
The gallon of 10 lb. =	$276\cdot48 \times \frac{50}{49} = 282\cdot01$	282 Beer gallon.
Also .... — =	$276\cdot48 \times \frac{10}{12} = 230\cdot40$	231 Wine gallon.
The pint of $1\frac{1}{4}$ lb. =	$34\cdot56 \times 3 = 103\cdot68$	103·40 Stirl^g jug.
Bushel of 80 lb. . . =	$2211\cdot84 \times \frac{35}{36} = 2150\cdot40$	2150·42 Win. bus.
A cylinder of $18\frac{1}{4}$ diam.	$\times\ 8 = 2208\cdot93$	Approximate bus.
.. Ditto . . . $18\frac{1}{4}$ — ..	$\times\ 8\cdot0105$	2211·84 New bus.

The following mode of defining the standards of length, weight and capacity, is submitted to the committee on weights and measures, as the most distinct answer to their inquiries:

	One yard of 36 inches	is such, that a pendulum of 39·13 inches vibrates seconds in London.
Avoird.	One pound of 16 ounces	is such, that 1 cubic foot of water at $56\frac{1}{2}°$ weighs 1,000 ounces.
Troy ..	One pound of 5,760 grains	is such, that 7,000 grains = 1 pound (avoirdupois).
	One gallon of 8 pints	may be such as to contain 10 pounds of distilled water at the temperature of $56\frac{1}{2}°$, with great convenience.

XXIX. *The*

XXIX. *The Opinions of Dr.* Thomson *and the Chevalier* Delambre, *in reference to Don* Rodriguez's *Animadversions on the Trigonometrical Survey, contrasted. By* Olinthus Gregory, *LL.D. of the Royal Military Academy.*

To Mr. Tilloch.

Sir, If Dr. Thomson had not, with a species of moderation and candour, which I suppose he indulges that it may be seen with what propriety *he* assumes the character of a genuine philosopher, positively shut his "Annals" against any reply I might be inclined to make to his animadversions, and even to his *questions**, I should not have troubled you with an additional communication, on a subject now so well understood, and correctly appreciated, as Don Rodriguez's remarks on the "Trigonometrical Survey."

On that subject I took up my pen unwillingly; but it is gratifying to know, at the end of a year and a half, that on the one hand the only person who has put his name to any public censure of my undertaking, is one who is obviously ignorant of the matter respecting which he ventures to obtrude his sentiments upon the world; while, on the other hand, the justice of my strictures has been acknowledged by all competent judges, and has indeed been confirmed by the most distinguished astronomer now living in Europe.

The readers of No. 179 of the Philosophical Magazine, and No. 159 of the Journal, will recollect that I there attempted, and I hope successfully, to refute every argument advanced by Don Rodriguez to prove the incorrectness of Colonel Mudge's observations at Arbury Hill; that I have proved that the Don's opinions as to the regular ellipticity of the terrestrial meridians, and indeed as to the subject generally, run counter to those of the greatest philosophers in Europe; that an error of 4½ seconds *could not* arise from the fixing, the construction, or the use of the instruments employed; and that, so far from there being an error of that magnitude either at Arbury Hill or Shanklin Beacon (the Dunnose station), there cannot *possibly* be one of even *half a second*, unless there be a corresponding or greater error in the series of observations *for years* at the fixed observatories of Blenheim and Portsmouth.

Notwithstanding this, however, Dr. Thomson, as if to convince the world how profoundly *he* is conversant with such topics, affirms that "*well-founded doubts* may remain on the matter," and that "the point *can only* be settled by repeating the observations:"

* See Thomson's Annals, No. 16, page 286.

servations:" and, as if to add weight to his opinion, and show how adequately and temperately he forms his judgement, he observes "that there is *no set of men* more apt *to indulge in inaccurate or absurd views than mathematicians!!*" A position so liberal and demonstrative that he adds, if it "stood in need of confirmation, it would be easy to bring forward some of the most celebrated names both of former and present times in confirmation of it."

But, happily, neither the reputation of mathematicians nor the true issue of this question depends upon the *fiat* of Dr. Thomson. A philosopher of no less celebrity than the Chevalier Delambre has published some "reflections" on Don Rodriguez's paper, in the "*Connaissance des Tems pour l'An* 1816," from which I shall select a few quotations.

Speaking of the northern partial arc examined by Don Rodriguez, and of its indicating a compression approaching to nothing, he says, "I acknowledge that this last result is extremely destitute of probability; but it is *little probable* also, that the same instrument, which had given accurate zenith distances at the two extremities, should furnish *an error of five seconds at the mean station.* Besides, it is not fully demonstrated that the curve of the meridian *is a regular ellipse*, and that the compression which is inferred from two very remote arcs, as those of Peru and Sweden, is the compression which accords with all the intermediate arcs." "The operations which determine the compression do not offer any real satisfaction, and we may yet doubt of the perfect *regularity* of meridians, as well as of their perfect *similitude*." In another place M. Delambre remarks, "that of the eight partial arcs of Colonel Mudge, M. Rodriguez has only examined *three*;" and adds, "it would be curious to see how Greenwich and *Blenheim* accord with the idea that the whole error arises, from the observations made at Arbury."

Perceiving from these remarks how unwilling this eminent mathematician and astronomical observer was to infer, even from Don Rodriguez's paper, that there existed an error of $4\frac{1}{2}$ seconds at Arbury Hill, I took an early opportunity of transmitting him a copy of my strictures inserted in your publication. I have the satisfaction to know that, with the exception of my remarks on the observations at Dunkirk and Chatillon, to which he has replied, he thinks my attempt successful: he agrees with me, that there can be no such error at Arbury Hill as Don Rodriguez imagines, and that my comparison of the observations there and at Dunnose with those at Blenheim and Portsmouth has set the matter at rest.

Here, then, I may with perfect composure take my leave of this

this question. Dr. Thomson, a respectable chemist, I believe, but (as far as his History of the Royal Society, and his ill-judged interference in this business, furnish evidence) a man of very moderate mathematical and philosophical attainments, affirms that "the point *can only* be settled by repeating the observations." The Chevalier Delambre, the most excellent theoretical and practical astronomer now living, *is of a different opinion.* Whether your scientific readers will concur in sentiment, on this purely astronomical inquiry, with the chemist or the astronomer, it is not difficult to predict.

When a writer has rendered himself notorious for assuming an air and tone of superiority and infallibility in all his controversial productions, which ill accords with his palpable mediocrity of talent and knowledge; and when he is indiscreet enough to libel a whole class (and that class comprehending such men as Archimedes and Newton and Laplace) for the supposed mistake or misconduct of an individual; it would neither be ungenerous nor unjust to lower his pretensions, and completely expose his numerous blunders. But it is a task which, however beneficial it might be to the public, and however easy of accomplishment, has no fascinations for me. Dr. Thomson's other observations*, therefore, not bearing upon the main question, I leave to sink by their own weight; and remain,

Sir,

Yours respectfully,

Royal Military Academy, Woolwich, September 6, 1814. OLINTHUS GREGORY.

XXX. *Memoir upon the compound and smooth or simple Eyes of Insects, and on the Manner in which these two Species of Eyes concur in Vision.* By M. MARCEL DE SERRES, *Professor of the Sciences in the Imperial University.*

[Continued from p. 118.]

THE tunic of the cornea is generally black in the *coleopteræ*: it is the same in those which want the choroid and the mucous varnish. The colour of this tunic is on the contrary much more variable among the *orthopteræ*, and sometimes one and the same eye

* Such, for example, as the reasons he assigns, why the first two volumes of "The Trigonometrical Survey," which were actually printed and published in The Philosophical Transactions, *were not admitted* into that work. O exquisite philosopher! to amuse and satisfy himself with accounting for the non-appearance of a phænomenon in a certain place, which did, notwithstanding, appear there *permanently!* Should I ever again be involved in controversy, may I have the good fortune to meet with such another *opponent!*

eye presents it of two different shades, as we see in the *locusta lilifolia*: lastly, in certain species the tunic of the cornea presents stripes variously coloured, and traced with the greatest regularity. These stripes are parallel in the *gryllus lineola*: some are of a blackish brown, and others of a yellowish gray, the clear colour of which dips into that of the rest of the eye. It must be remarked, that frequently these shades are effaced after death, on account of the alterability of the tunic of the cornea. There are even species in which the tunic of the cornea is as it were marbled; then it exhibits various shades in different parts of the eye.

As to the tunic of the smooth or simple eyes, it is seldom black in the *orthopteræ*, and its colours are always uniform in the same eye; although they are much variegated, when we examine a great number of species. The most common colours are white, red, or pale green.

A great number of *hemipteræ* also present the tunic of their cornea of colours pretty much variegated; but its brown or rather black tints are much more common than in the *orthopteræ*. As we find among the latter, several *hemipteræ* present the tunic of the cornea of their smooth eyes, either whitish or of the most agreeable red, as is observed in the common grasshopper. Lastly, it appears to me, that almost all the *hemipteræ* which live in water have the tunic of their cornea completely black; perhaps it is the same with all the *coleopteræ* which have the same way of living.

The *lepidopteræ* furnished with the finest colours have also the tunic of the cornea of shades greatly variegated. Nevertheless, among all those which fly at sunset or during the night only, we observe that the tunic of their cornea is generally black. To conclude: the case is the same with several species of diurnal butterflies: for instance, the *papilio podalirius* has the tunic of the cornea completely black; and notwithstanding the sombre hue of this colour, the varnish of the choroid is very thick in this species. In the *papilio atalanta* the tunic of the cornea is of a clear brown; thus the black point resulting from the aperture through which the optic nerve passes, is seen on the outside of the eye. It must be observed, however, that as this circular aperture is formed in this species by vesicular tracheæ, and these tracheæ being a little separated from each other, the black point which is seen generally at the exterior of the eye, seems surrounded by other black points, which present a peculiar form, all having a smaller extent than the central point. In the *papilio cardui*, the tunic of the cornea is of a brilliant green, but much clearer in the lower part of the eye than in the upper, on account of the luminous rays less easily reaching them.

them. This tunic is thin, whereas the varnish of the choroid and the choroid itself are on the contrary thick. We also observe externally on the eye the central black point, and several others produced by the same cause with those of the eye of the *papilio atalanta.* Only these blackish points are more irregular in their form than those of the *papilio atalanta.*

The larvæ of the *lepidopteræ* have only smooth eyes situated generally on the sides of the head. They are variable in colour and number. Thus we observe eight in the silkworm, and six only in the caterpillar. The former are black, and the latter transparent. As to their internal structure, we shall speak of it elsewhere, inasmuch as it exhibits some differences from that of the other smooth eyes.

In the *neuropteræ,* the tunic of the cornea is also very much varied, and its colours vary much in various species. In the *libellula vulgaris,* the compound eyes exhibit all their upper portion of a reddish brown, and all the lower of a yellowish green. On carefully removing the cornea, it is easily seen that the difference in the two colours is owing to the diversity of colour in the tunic of the cornea. It seems even that this tunic is thicker in the brown part of the eye than in the green. We shall also remark that the upper facets of the cornea are greater than the lower; which is rather singular. If we remove the cornea, we distinctly perceive the optic nerves passing through the choroid and its varnish, membranes which are easily distinguished from their being of a deep black.

The circular trachea surrounds the retina, and its ramifications pierce with the optic nerves all the other membranes, dispersing themselves *ad infinitum* under the cornea. This disposition is so evident, that we may easily conceive how Swammerdam thought the cornea was formed by the meeting of these tracheæ. In other respects the eye of this *libellula* presents nothing particular, with the exception of the enlargement or great extent of the retina.

I have also dissected the eyes of certain larvæ of *agrion* and *aeshma,* but have observed nothing very different. These larvæ seemed to want the circular trachea, the place of which is supplied by an infinity of other tracheæ, which spread in a great number around the optic nerve, which is very broad and thick in these larvæ. I never observed that the eye had any particular conformation in the species which habitually frequent the water; and this remark is important, because the case is very different in the other animals. The form of the cornea also presents differences in the species in the aquatic insects, as well as in those which live habitually in the air.

If

If the tunic of the cornea presents shades much varied in the *lepidopteræ* and *neuropteræ*, these shades are not less varied in the *hymenopteræ*, in which we sometimes see a gray, a green, a yellow, and even all the shades of black. Certain bees, and particularly the *violacea*, present this tunic of the most opaque black. Generally the *hymenopteræ* have their cornea very thick, and it is not difficult to remove it by thin slices. Sometimes this cornea is surrounded by hairs, and rarely do they issue from the cornea itself. In a great number of species, the circular trachea is totally wanting; but its place is always supplied by other and smaller tracheæ which surround the optic nerve, and in general these tracheæ are extremely multiplied.

What has been said with respect to the eyes of the *hymenopteræ* is applicable to those of the *dipteræ*, which have the tunic of the cornea with the most variegated shades. In a certain number of species these shades are very brilliant, and the *tabanus* as well as the *musca* hold a distinguished part in this respect.

Certain species of *syrphus* present, at the exterior of the eye, two semi-circular stripes deeper than the general tint. These stripes are produced as usual by a deeper shade of the tunic of the cornea in this part of the eye: but what renders this disposition remarkable is, that there are numerous hairs precisely in this very part of the compound eye, and which originate from the cornea itself.

As to the eyes of the *apteræ*, they are in general of dark colours, and not very large. These insects seem therefore not to be much favoured in respect to the organ of sight, and probably their way of life does not require acute vision.

It is remarkable that the insects which live in water have in general their eyes dim and opaque externally, nearly like those which exist in dark places. Thus the nymphs of the *libellulæ* in passing to the condition of a perfect insect assume brilliant and transparent eyes, whereas previous to their last metamorphosis these eyes were dull and without lustre. This observation has not escaped the sagacity of Reaumur in his History of Insects.

In the descriptions which we have given of the situation and form of the compound and simple eyes of insects, it will be remarked that the former disposition is less subject to vary than the latter. This consideration might have arisen *à priori*, since no form can exhibit differences in any order of animals, without other variations following in the parts which surround those whose disposition changes. The only very remarkable example which we can mention of the variation in the position of the compound

compound eyes, is that of the *diopsis ichneumonea*. This insect exhibits its eyes situated nearly like those of the *mollusci:* thus they are placed at the extremity of a long pedicle, or a kind of contractile tentacular point, but which can at least follow all the motions of the head. We may say, on observing this singular organization, that this insect has its eyes at the extremity of a telescope; but as we have not seen it alive, nor dissected it, it would be difficult to decide how far this idea is correct.

As to the descriptions which we shall presently give of the relation which exists between the size of the bodies and that of the compound eyes, they prove that the *dipteræ*, the *hymenopteræ*, *lepidopteræ*, and *neuropteræ* have very large eyes: it even seems that there are few animals equally favoured. As a great number of *coleopteræ* and *hemipteræ* have also very large eyes in comparison to their bodies, we may say that this size is very general in all insects. The two solitary examples of the *phasmæ* and *scolopendræ* ought not to prevent us from concluding that, of all the animals, insects have the largest eyes in comparison with the size of their bodies.

§ II. *Of the simple or smooth Eyes.* The number of simple eyes is far from being as constant as that of the compound eyes. Certain species present two; others four, six, and eight; but in general we observe three. This number is even pretty constant in families which have at once compound and smooth eyes. We rarely see exceptions to this arrangement; and the *blatta*, as well as certain *acheta*, are perhaps the only kinds which, having compound eyes, have only two simple eyes situated on the upper part of the head. When there are three simple eyes, they are always arranged in the form of a triangle, so that there are two lateral, and one in the middle: their situation is also constantly on the summit of the head. Sometimes, however, the simple eyes are arranged two on the summit, and one in the middle of the front of the head. This arrangement holds in the *gryllus*, the *truxalis*, the *acrydium*, the *locusta*, and the *gryllotalpa*.

The *empusa*, which have upon the summit of the head a small triangular elongation, cannot on account of this organisation see objects with their eye in the middle.

The general form of the simple eyes is very variable: it seems, however, that in general that of the lateral eyes is elongated and elliptical, whilst that of the middle one is round: there are also many exceptions in this respect, since the *acheta* and the *gryllotalpa* present that in the middle of a very much elongated oval of which the greatest diameter is transversal.

The smooth or simple eye is formed of an external hard transparent membrane, convex externally, and concave internally. This membrane enveloping the eye necessarily determines its form:

form: it is not composed, like that of the compound eyes, of an infinite number of facets, but rather of a membrane of a single piece, and on which no divisions are perceptible. We may consider this first membrane as a cornea, on account of its transparency, a transparency which we observe even at the exterior of the eye, on account of the little colour of the varnish applied on the membranes situated under it.

After having removed the cornea, we find a viscous tunic more or less thick, and of which the colour also undergoes considerable variations. Thus this tunic or varnish presents the most opposite colours: it is almost always black in the *hymenopteræ*, whereas it is whitish in the *orthopteræ*. In the caterpillar, this varnish is frequently black, yellow, or red, and sometimes it is of the most beautiful brilliant green. This viscous tunic has been regarded by Swammerdam as a kind of uvea, although it seems very like the tunic which covers the cornea in compound eyes.

We ought not, however, to decide that the tunic of the cornea of the simple eyes is distinct from the varnish of the choroid. This appears probable notwithstanding, since in certain species the external colour of the simple eyes is not the same with that of the varnish with which the choroid is covered. Thus much is certain, that the cornea of the simple eyes is always coated internally with a kind of varnish, the colour of which appears at the exterior of the eye on account of the transparency of the cornea. This coating surrounds the optic nerve, which proceeds into the concavity of the cornea after having passed through the choroid and its varnish. Swammerdam very properly remarks, that the smooth eyes of insects receive nerves which are furnished to them by the cerebriform ganglion lodged in the head. Lyonnet, whose researches announce so much sagacity, has also observed that the simple eyes of the willow caterpillar are composed of a cornea and a choroid traversed by the extremity of the optic nerve*.

The optic nerves of the simple eyes issue immediately from the brain, when these eyes are removed far enough from each other; but when, on the contrary, they are close together, as observed in the caterpillar and a great number of larvæ, the optic nerves are only divisions of a larger nerve which issues directly from the brain.

Lyonnet has described this arrangement accurately, and the figure which he gives of it, Plate XVIII. No. 6, is very correct. Then there exists a peculiar membrane in the form of a funnel, to which the six branches of the optic nerve are attached, and

* *Traite Anatomique de la Chenille du Saule, par Lyonnet*, p. 570.

this

this membrane ends at the place where the nerve itself divides into these six branches. We do not know positively, if this arrangement exists in the spiders: we only know that it does not hold in the scorpions. Besides, externally the eyes of the spiders and of the scorpions have the same structure and figure with the simple eyes of insects.

Immediately after the optic nerve and the tunic of the cornea, we observe a peculiar membrane which we shall consider as a kind of choroid. It exhibits, however, this difference from the choroid of the compound eyes, that it has no varnish so distinct; and finally, its breadth is always greater than the circumference of the cornea itself. This membrane, coloured frequently in red or black, is also sometimes colourless; and lastly, its whiteness is so dull that it is easy to distinguish it from the tracheæ. The thickness of this membrane is great enough to make it resist even a long maceration.

In the species in which the simple eyes are whitish, we see this membrane clothed with a coating of colours varying with the colours of the tunic which covers the choroid. This arrangement is even very perceptible at the exterior of the eye.

It would seem that this choroid is particularly very thick and white in such species as have their cornea concave and as if sunk. This arrangement must have taken place on account of the divergency undergone by the rays of light in arriving on a concave and transparent surface, their direction even being parallel, a divergency which is such that we may very well conceive that the eye would be too much affected by it, if a black membrane had also absorbed a certain number of luminous rays. A white membrane, sending back on the contrary all the rays which it receives, may augment the excitability of the optic nerve, and thus contribute to render the vision more distinct. Although this species of choroid presents in certain circumstances a very great whiteness, it is nevertheless always easy to distinguish it from the tracheæ, the only vessels with which it can be confounded. For, as little as we are in the habit of seeing the organs of insects, we can easily ascertain the tracheæ from their azure colour; and besides, by dissecting them in water (the most advantageous way of acting in these experiments) we see the tracheæ rise above the surface of the liquid; and from that moment we can no longer have the smallest doubt, for the tracheæ are the only organs which have so little specific gravity.

This choroid, the membranes of which are close and very thick, seems formed by a cellular texture of very close meshes, over which a heap of tracheæ are distributed. It is therefore underneath the cornea that the optic nerve is situated, which issuing from the anterior faces of the brain proceeds to the mid-

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dle of the eye. It does not appear that this optic nerve has any swelling at its base, or that a kind of retina is formed analogous to that of the compound eyes. In some species it appears to me that this nerve becomes broader towards its extremity, *i.e.* at the point where it corresponds to the cornea. I shall not, however, assert this positively, for it is possible that this dilatation may depend on the contraction produced by the section of the nerve. As I have not dissected these organs in the living subject, (which ought always to be done for the sake of precision,) I have not been able to verify, whether this dilatation depended on the sensibility of the nervous organ. However the case may be, the optic nerve before reaching the cornea traverses the muscles of the various parts of the head, sometimes enters a trachea, penetrates afterwards through the choroid and its varnish, and afterwards, when surrounded with the tunic of the cornea, corresponds to the internal surface of this membrane, on which it seems to lose itself.

As the cornea of the simple eyes is not, like that of the compound eyes, divided by numerous facets, it has not been necessary that the small nerves should give it a great number of small threads, but only that they should proceed to the point where the divergency of the rays of light should be the least considerable.

The small optic nerves which proceed to the simple eyes issue from the brain. As their position varies in regard to the other parts, they are sometimes the second, third, fourth, or fifth pair of nerves: this depends on their situation with respect to the various organs of the head, a situation which determines by what pair they are furnished. These nerves are directed always towards the simple eyes, being retained in their position either by pneumatic pouches, or by tracheæ, proceeding more or less obliquely according to their position with respect to the brain. They always terminate under the cornea, forming a kind of retina.

It results from this description, that the simple eyes are formed quite differently from the compound eyes. Thus the cornea of the former is all of one piece, whereas in the latter it is formed by the union of a great number of hexagonal facets. As to the tunic of the cornea, it does not exhibit any difference in the two kinds of eyes, except perhaps in point of thickness; but with respect to the varnish of the choroid, it is always less distinct in the simple than in the compound eyes. Finally, the choroid is not always black in the simple eyes, whereas it is uniformly in the compound. Sometimes this membrane, instead of being black and opaque, is of a peculiar whiteness and lustre. To conclude: this membrane and its tunic are placed in the same way with respect to the other parts in both kinds of eyes, and their

uses

uses are also similar. The large circular trachea, which we observe in the compound eyes when they present a choroid, does not seem to exist in the simple eyes. In fact, the tracheæ or pneumatic pouches which belong to them are used in supporting the optic nerve, and perhaps also in forming a part of the choroid in that sort of eye. At least the tracheæ of insects seem to perform the functions of the blood-vessels which are observed on the choroid of the red-blooded animals.

[To be continued.]

XXXI. *Some Particulars respecting Hail Storms in the West Indies in the Month of April* 1814; *and Experiments to ascertain the medicinal Powers of the* Hura crepitans.

To Mr. Tilloch.

Nevis, June 22, 1814.

Sir,—Though I have already communicated to Sir Joseph Banks some particulars respecting the late hail storms in this island and Saint Christopher's, a slight notice of their uncommon phænomena may not be deemed altogether undeserving of a place in your Magazine.

The fall of hail intermingled with masses of ice took place in the island of Saint Christopher's at eleven o'clock in the forenoon of Wednesday the 13th of last April, at which time I was absent from that island on a visit to a friend here. The weather had been for some time previous excessively dry, and the heat in general very great; and even upon the morning of the day on which this phænomenon appeared, the atmosphere was at an early hour close, sultry, and oppressive. As the day advanced, black and dense clouds gathered from all sides, and thunder was heard at a distance; a small quantity of rain fell in the southern and western parts of this island (Nevis), but the thickest part of the storm skirting our northern shores proceeded along the Salt Pond Hills towards the valley of Basseterre (St. Kitts), where an abundance of rain fell. A short time before the hail fell, an unusual and, as some described it, even painful sensation of cold was observed, a loud clap of thunder was heard in the south-east, and the rattling of the hail upon the roofs of the houses attracted the attention of the inhabitants little acquainted with the nature of such appearances.

The fall of hail was by no means general throughout the island, confining itself to certain narrow limits little removed from the line of mountains which, after the manner of the Appenines, traverses the centre of the island from east to west. No hail fell at Basseterre; but at the estate called O'Gee, belonging

longing to Sir James Earle, its descent was first observed, and lasted but a few minutes. This estate is situated about two miles nearly due north of Basseterre. Hence the cloud proceeded skirting the northern side of the mountains in the direction of Dieppe Bay, where large masses of ice, mostly of a radiated form, were observed to fall during a space of not less than fifteen minutes, Mr. Charles Woodley, who resides in this neighbourhood, and is an enlightened and intelligent planter, made drawings of three of these masses, which he kindly permitted me to copy for Sir Joseph Banks's inspection; to whom I accordingly forwarded them. Some masses were said to have fallen which measured seven inches in length: but I suspect this to be an exaggerated statement. The consternation produced among the negroes by so unusual (and to them inexplicable) an event can be more readily conceived than described. But the white inhabitants it might be supposed would have regarded it with somewhat more philosophic composure: yet I have been credibly informed that a creole overseer who happened to be in the field, upon being struck with one of the hail-stones, fell in a fainting fit (the result of his fears) from his horse. A similar occurrence is said to have been observed some years ago by the officers of the garrison at Brimstone Hill; but I cannot procure any certain account of the period or circumstances of the event.

Subsequent to this fall of ice in St. Kitts, two hail showers have been observed in the windward district of this island, the last of which took place so late as Trinity Sunday. The hail-stones, I am informed, were of the size of the common gray nickers (the seeds of the *Guilandina bonduc*). I cannot learn that any radiated specimens were observed among them, as was the case at Saint Kitts.

I have lately made a few experiments with a view to ascertain the safety and efficacy of the seeds of the Sand Box (*Hura crepitans*) as a purgative remedy. Their operation is commonly represented as so violent, both upwards and downwards, as to endanger the life of the experimenter. Aware however of the strong disposition to exaggeration, and acquainted with an instance in which upwards of a dozen of these seeds had been eaten as an agreeable fruit by a newly-arrived European, without producing any fatal result, I conceived that they might, by a cautious use, prove an advantageous as well as highly palatable addition to our Materia Medica. I accordingly took at first an entire nut previously decorticated, upon a full stomach, but felt no nausea or other inconvenience follow; neither were my bowels, as I had wished, in the least affected: in consequence of which, upon the following morning I took half a nut, which weighed six grains, from which I omitted to remove the germ and

and interior lining. In consequence I felt for some time after an unpleasant burning sensation in my throat, but neither purgative nor emetic effect followed. I afterwards repeated the six-grains dose, having previously removed the germ and paper-like lining, and with much difficulty obtained one stool little differing from the natural.

I have not at present prosecuted these researches further: but, should circumstances admit of my following up these preliminary experiments, I shall lose no time in communicating their results to you for the general information of the medical world. The nut of the Sand Box is extremely palatable, tasting much like a good filbert.

An American captain who was in the habit of visiting Saint Kitts, before the war, was in the constant practice of using one or more of the seeds of the common Physic Nut (*Jatropha Curcas*) as a purgative when his bowels were irregular, using only the precaution of separating the germ, in which the violently drastic effects seemed solely to reside.

I have the honour to remain,
With high esteem, sir,
Your most obedient servant,
WILLIAM HAMILTON, M. B.

XXXII. *Chemical Inquiries into the Nature of several fatty Substances, and particularly on their Combinations with the Alkalis. By M.* CHEVREUL*.

Of a new Substance, called Margarine, *obtained from the Soap made from the Fat of Pork and Potash.*

1. THE combination of a fat substance with an alkali, presenting several products which are of great utility in the arts and domestic œconomy, was examined with respect to their usual properties: but in point of theory they were not made the subject of any special inquiry, so that we are reduced to hypotheses in order to explain one of the most common operations in our manufactories. The chemists of Stahl's school, who thought that the acid obtained from distilled oils was one of their constituent principles, generally regarded it as the cause of saponification. This idea was renounced when Lavoisier demonstrated that most of the bodies produced from organic matter, when distilled, resulted from a decomposition produced by caloric. Previous to Lavoisier M. Berthollet had regarded saponification in a more simple and accurate manner, by making it depend on the

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

the affinity of the oil itself for the alkalis; so that soaps must be considered as compounds in which salifiable bases were neutralized by fat substances, the action of which was analogous to that of the acids: it was in this respect that he compared the oils to the latter substances, and we shall presently see how far this comparison was just. This way of thinking was adopted: but afterwards they went further, and pretended that the oil was not saponified but by absorbing oxygen: they did not found their opinion upon any positive fact, the slightest indications appearing sufficient.

2. In the present state of chemistry, however, we must not content ourselves with bare assertions: to establish a theory, we must be acquainted with the bodies which are in contact, analyse the compounds which they have formed, and see if the principles separated from it are the same with those which have been put in reaction. In commencing the account of my experiments on this substance, I find the double advantage of fixing the attention upon one of the most common products of the saponification of animal fat, and at the same time facilitating the study of fatty substances, by making known a species which possesses their generic properties in the highest possible degree.

3. When we immerse soap made of pork grease and potash in a large quantity of water, one part is dissolved, while another is precipitated in the form of several brilliant pellets, which I shall call mother of pearl substance.

Of the Purification of the Mother of Pearl Substance, and of its Decomposition by the Muriatic Acid.

4. Eighty grammes ($3\frac{1}{4}$ oz. or 1551 grains English) dried in the air were diluted in 12 litres (3 gallons $1\frac{16}{100}$ pints English) of water, and exposed to a temperature ranging from 50° to 60°: they absorbed this liquid, and increased greatly in size. At the end of ten days, the matter was put upon a filter; when it was drained, twenty litres of cold water was thrown over it, to deprive it of any soluble soap which it might retain. When thus washed it was dried, then treated thrice successively with two litres of boiling alcohol of the gravity of 0·820 divided into three equal portions: the first congealed almost wholly upon cooling; the second was slightly disturbed, and the third almost not at all. The residue insoluble in alcohol weighed rather more than two grammes (about $38\frac{1}{2}$ grains); it resembled the mother of pearl substance: it was different, however, from the part which was dissolved. The experiment taught me that the mother of pearl substance was formed of a fatty substance, absolutely new, combined with potash, lime, and oxide of iron; and that when we treated

treated it with boiling alcohol * we dissolved the combination of potash, and a little of those of lime and oxide of iron, while the latter, insoluble or much less soluble than the former, formed the residue.

I convinced myself of these facts by treating in the following way the depositions which were separated from the alcoholic washings by cooling and concentration † comparatively with the residue. I put some very dilute muriatic acid into two porcelain capsules: to the one I added some mother of pearl substance which was soluble in alcohol, and to the other the residue. On heating them the acid was combined with the salifiable bases, and the melted fatty substance was separated from the liquid. After keeping this substance in fusion in distilled water, and leaving it to become solid by cooling, I added the washings to the acid liquids, and evaporated to dryness. I found that 100 parts of soluble matter had given to the muriatic acid 0·06 of lime and oxide of iron, and 8·07 of potash, while the residue had given to this acid lime, oxide of iron, and a mere atom of potash.

5. The fatty substance, separated from the salifiable bases, was dissolved in boiling alcohol: on cooling, it was obtained crystallized and very pure, and in this state it was examined. As it has not been hitherto described, it ought to be distinguished from other substances by a peculiar name: consequently I purpose to call it *margarine*, from the Greek word signifying pearl, because one of its characters is to have the appearance of mother of pearl, which it communicates to several of the combinations which it forms with the salifiable bases.

Of Margarine.

6. It is of a pearly white, and tasteless. Its smell is feeble, and a little similar to that of white wax. It is lighter than water. At 45·2·5 Reaumur it melts into a very limpid colourless liquid, which crystallizes upon cooling into brilliant needles of the finest white.

7. When we distilled it, it melted, and exhaled a white vapour which was deposited in a farinaceous-like matter in the neck of the retort. It boiled, and soon afterwards emitted an invisible vapour,

* A process somewhat similar has been adopted in this country to make what is called "transparent soap."—TRANSLATOR.

† Every washing was separated from its deposition, and the filtered liquor was concentrated to two-thirds its primitive volume, afterwards cooled and filtered. The depositations in the filter were washed with cold alcohol, then pressed in joseph paper, and by this means I obtained the mother of pearl substance in a state of purity.

 which

which was condensed into a liquid, and then into a white concrete mass. At the same time there was produced a little acid water, which was indebted for this property to vinegar, and perhaps some sebacic acid, and a strong smell, which I am inclined to attribute to a combination of volatile empyreumatic oil and acetic acid. When the matter contained in the retort began to turn black, and the product extricated from it was coloured yellow, I changed the recipient, and continued the fire until there was no longer any thing volatilized; very little gas or liquid was formed, and almost the whole of the product was solid. Four grammes ($61\frac{3}{4}$ grains) of margarine yielded 0·045 gr. (about ·3432 grain) of charcoal, containing an atom of lime, iron and potash, a first product, which was very white, weighing 2·19 gr. (33 grains), a second product, coloured yellow, weighing 1·45 gr. (22 grains).

8. The first product boiled with half its weight of potash dissolved in water combined with the alkali. The combination diluted in water deposited a good deal of pearly matter. Hence it follows that this product was formed in a great measure of non-decomposed margarine.

9. The second product treated in the same way gave out an atom of ammonia, which I thought accidental: instead of being combined with potash, it melted into a yellow oily liquid, which was again boiled with double its weight of alkali, and which refused to unite with it. After these two operations it weighed one gramme, and there was of course a loss: it had given little or nothing to the potash. When it was heated in alcohol it liquefied: a great quantity of this liquid is necessary to dissolve it: upon cooling, small crystals were precipitated of a pearly citrine white, which became of a slight yellow by fusion: the alcohol from which the latter were precipitated, when evaporated, left an oil of a deep yellow, which was liquid at 18° centigrade. If the second product contained margarine, it must be concluded that it was there in a particular state of combination, since the concrete crystallized substance and the liquid substance differ from it in every respect.

10. The alkaline liquors which had been boiled with the two foregoing products were united, concentrated, and filtered several times. They contained very little fatty matter in solution: when distilled with sulphuric acid, they gave an atom of vinegar, and a residue containing some small crystals which had the appearance of sebacic acid.

11. Margarine is insoluble in water.

12. It is extremely soluble in alcohol; for 100 parts of the latter of the gravity of 0·816 dissolved at 75° 180·79 parts. This

This solution was only disturbed at 41° of the centigrade. Upon cooling it congealed into a solid mass: this mass had a greenish shade, which became particularly evident in those parts of the centre where a hole was made with a glass spatula. This phænomenon seemed to be owing to the alcohol, for the colour disappeared on the evaporation of this liquid. When the margarine is deposited by a gradual cooling, from a solution which is not saturated with it, it crystallizes in small needles which unite in stars.

Action of Potash on Margarine.

13. I now come to speak of one of the most remarkable combinations of margarine, being that which it forms with potash. It truly characterizes this substance, and leads me to examine some points of the chemical doctrines respecting acidity.

14. When explaining as above the process for purifying the mother of pearl substance, I have observed that the latter gave me 8·07 gr. per cent. of potash; but having found alkali in the charcoal of distilled margarine, and being thereby convinced that the muriatic acid had not completely decomposed the mother of pearl substance, I wished to make a new analysis of it. I took two grammes of this substance perfectly pure and dried. I decomposed them by the muriatic acid, and kept the margarine several times in fusion in acidulated water, in order to take from it all its alkali. I obtained 0·255 gr. of muriate of dry potash, which represented 0·1632 of potash, admitting 64 of base in the muriate of potash. The dried margarine weighed 1·865 gr. after having been fused. The 0·0282 exceeding the weight of the matter analysed, ought to be attributed to water remaining in the margarine, if the calculation of the muriate of potash which we have admitted be exact. In order to assure myself that the muriatic acid had dissolved all the potash united to the margarine, I charred two grammes of mother of pearl matter in a small platina crucible; I lixiviated the charcoal, and afterwards incinerated it: the atom of ashes which it left was mixed with the lixivium of charcoal. I combined the whole with the muriatic acid, and obtained 0·253 of muriate of potash, which confirms the first result. I think from this that we may state the following as the composition of the mother of pearl substance:

Margarine	91·84	100
Potash	8·16	8·88

15. The pearly matter is soft to the touch, and has no perceptible taste.

16. When heated in the sand bath, it does not melt; it begins

gins to turn soft, however, and the lumps of matter get joined together.

Action of Water.

17. It does not undergo any action from cold water, for it preserves all its physical properties after the maceration of a month in this liquid: nevertheless, if we examine the latter, we find in it a small particle of alkali, and some almost imperceptible traces of pearly substance. Hot water has a more decided action on it: thus, when we put into 100 grammes of boiling water one gramme of pearly substance, the latter becomes semi-transparent, it resembles flakes of alumine recently precipitated from a very dilute solution. The water filtered when boiling does not pass over clear; if before filtering it we allow it to cool, it becomes turbid, and deposits mother of pearl substance: if we then filter it, we find very little potash in it. 1·5 litre of water (3·1699 pints English) in which I had boiled 20 grammes of pearly matter, filtered after having been cooled and evaporated, left a residue of subcarbonate and of pearly matter which scarcely weighed 0·1 gr. (1½ grain). The matter deposited from the boiling water retains a little water, which gives it the property of melting at a temperature of 100° centigrade, and a semi-transparency analogous to that of wax. I am strongly inclined to think that the pearly matter is not dissolved in hot water, and that it is only divisible in it by combining with water. I formed my opinion upon 1000 parts of water being boiled for two hours over one part of matter, without making it disappear.

18. The mother of pearl substance is less soluble in alcohol than margarine. 100 grammes of this liquid of a gravity of 0·834 only dissolved 31·37 at a temperature of 67° centigrade. This solution becomes turbid upon cooling; and this ought to be the case, since one part of mother of pearl matter requires 318 of alcohol to be dissolved at the temperature of 20° centigrade: it does not change the colour of hematine; which proves that the potash is more strongly attracted by the margarine than by the colouring principle.

19. When we mix this solution with water, an abundant precipitate is formed: the sediment contains less potash than pearly matter: this is demonstrated by the following experiment. I dissolved in boiling alcohol 4·40 gr. of pearly matter containing 4·04 gr. of margarine and 0·36 potash. I poured the liquor while still warm into 1·5 litre of water, and shook the mixture several times. In twelve hours I filtered: the liquid evaporated deposited an inappreciable quantity of pearly matter: it

it contained 0·052 gr. of potash: consequently, the marg rine a the alkali of the matter precipitated must have been there in t proportion of 100 to 7·62. The analysis which I made with t muriatic acid gave me the proportion of 100 to 7·95, which d fers from the former only by 0·33. If we take the medium, shall have 7·78, and we shall then find that one-eighth of t alkali contained in the pearly matter was separated by the wat

20. The matter which was precipitated from the alcohol the water did not yield a perceptible quantity of alkali to t latter; but if we add a little hematine, a part is then separate This is proved by the two following experiments. I boiled t matter in water, divided the liquid into two equal portions, f tered one of them, and afterwards put hematine into the filter liquor, and also into that which was not filtered. The form makes almost no change on the colouring principle, where the latter renders it purple by giving alkali to it. This decon position takes place only in consequence of the united action the water and hematine; for, if we mix the dissolved matters i alcohol, the hematine undergoes no change. (18.)

21. The matter which was precipitated from the alcohol b water, redissolved twice in the alcohol, was deposited upon cooling in the state of true mother of pearl matter, which contains 100 of margarine and 8·88 of potash.

22. It results from what I have said on the precipitation means of water from the alcoholic solution of mother of pea substance: 1st, that in this precipitation there is a quantity potash separated from the margarine, which seems to be one eighth of that which is combined with it; that this separation is owing to the affinity of water for alcohol and the potash, and to the insolubility of the margarine in water. If boiling water takes less alkali from the mother of pearl substance than cold water poured into the alcoholic solution of the latter, this seems to arise, because in the second case this liquid exercises its action on a dissolved body; whereas in the other it exercises it on a solid, the cohesion of which is an obstacle to its solvent power. 2d, That the matter precipitated from alcohol yields little if any alkali to boiling water, but yields it to an aqueous solution of hematine. 3dly, That by redissolving it in boiling alcohol, it is precipitated in mother of pearl matter upon cooling. This proves, that in this substance the two elements exist in a proportion in which they enjoy a cohesion sufficient to determine the separation of the excess of margarine from which the wate had taken the alkali.

23. This last consideration led me to inquire if marga when presented to a hot solution of potash holding a q of alkali much more considerable than that which was n

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to convert it into pearly matter, should be changed into this matter, or rather into a more alkaline combination.

24. I put into 160 grammes of water holding 24 grammes of alcoholized potash, 40 grammes of margarine. The latter became soft by heat, gelatinous, and semi-transparent, by uniting with the potash. The substances put in digestion for six hours, at the temperature of 80° or 90° centigrade, were afterwards allowed to stand by themselves. In fifteen hours a white and opaque mass was separated from an almost colourless mother water. This liquid supersaturated with sulphuric acid deposited a small particle only of margarine, and yielded upon distillation neither acetic acid nor volatile oil.

25. The white mass separated from the mother water was pressed between pieces of joseph paper, until no more liquid was yielded: it was then white and opaque. This method appearing to me to be insufficient for taking up all the alkali which was not in combination with the margarine, I found, after several fruitless experiments, that boiling alcohol dissolved the substance very well, and that upon cooling it was deposited in the form of small needles, which it was very easy to obtain in a state of perfect purity by putting them on a filter, washing them with cold alcohol, then squeezing them in joseph paper, and exposing them for several hours to the rays of the sun.

26. Two grammes of these needles, thus treated, decomposed by the muriatic acid, gave 1·72 gr. of margarine, and 0·475 gr. of muriate of potash, representing 0·3072 of base. If we admit that the excess of weight is owing to water retained by the margarine, we shall have the following proportion:

Margarine	100
Potash	18·14

This result shows that when we unite directly the margarine with potash, there is a combination formed which contains twice as much alkali as pearly matter; for we have found this last formed of 100 parts margarine, and 8·88 of potash. Now this quantity multiplied by two, gives 17·76 which differs only in 0·38 from the foregoing determination. Margarine therefore in these combinations with potash obeys the same laws with the inorganic bodies.

27. The combination of margarine saturated with potash presents the following properties:

28. It is white, and not so soft to the touch as the pearly matter. It has a very slight alkaline taste.

29. When we put it into pure water, it is decomposed into a pearly matter and into potash. We may be convinced of this by putting it into abundance of cold water, and shaking the mixture from time to time. The matter thrown upon a filter and washed yielded

yielded margarine 100, and potash 8·55. The washings when filtered and evaporated contained potash and a trace of margarine. If instead of putting the combination of margarine saturated with alkali in plenty of water, we put it in a small quantity of this liquid, it swells, becomes semi-transparent by absorbing it, and forms a thick mucilage which exhibits pearly matter when it is shaken. In this case the mass of water not being sufficient to overcome the whole of the affinity of the pearly matter for an excess of potash, it happens that the decomposition of the saturated combination is only partial, and it is the part which is not altered which absorbs the water, and forms with it a mucilaginous liquid, by retaining it among its molecules. If we throw the whole upon a filter, after having mixed enough of water with it to facilitate the filtration, analysis demonstrates less potash than in the saturated combination, and more than in the pearly matter; which proves that the decomposition has been but partial.

30. When we put three grammes of saturated combination in 100 grammes of boiling water, we obtain a solution which is perfectly limpid while the liquor is warm, and which even may be filtered. This solution upon cooling deposits abundance of mother of pearl matter, and afterwards becomes thick. When it is completely cooled, it is precisely like cold water in which we have put a great quantity of pearly combination: only the mucilage is much more homogeneous. The filtered liquor contains potash and some atoms of margarine; for it is not rendered turbid by the acids, except when it has been concentrated.

31. It follows from what has been said: 1. That the saturated combination of margarine is decomposed into mother of pearl substance, and into potash, by a great mass of cold water; 2dly, that a little of this liquid only decomposes a part of it; that what is not decomposed absorbs water, and forms a thick semi-transparent mucilage; 3dly, that when the action of the water is assisted by that of caloric, the saturated combination may be completely dissolved, and that by cooling there is formed pearly matter, and a thick mucilage of saturated combination, if the water be not always in excess.

32. The saturated combination is dissolved in boiling alcohol, and partly precipitated upon cooling, without undergoing decomposition. 100 parts of boiling alcohol of a gravity of 0·834 appeared to me to dissolve only 8·93. When we dilute this solution in water, we obtain pearly matter in the form of very brilliant small crystals. If the alcohol does not decompose the saturated combination as water does, this is owing to its dissolving equally well potash and margarine; whereas water dissolving only

only one of the elements of the combination, attracts a portion of the latter with more force than the insoluble element does.

33. Margarine decomposes the subcarbonate of potash. We may render the decomposition evident, by passing into a tube full of mercury a mixture of eight parts of water, one of margarine, and half a part of subcarbonate, and by heating it afterwards with a red-hot iron to ebullition. After cooling we find a gaseous residue which is pure carbonic acid. By repeating the experiment in a small phial furnished with a curved tube, I observed that the margarine was dissolved before there was any extrication of carbonic gas, and that this extrication only took place when the liquor boiled. This circumstance induced me to think that, at the temperature at which margarine may be united with potash, the carbonic acid which is separated from it may act upon a portion of subcarbonate, and convert it into saturated carbonate, and that it is afterwards the latter which sends out carbonic acid at the temperature of boiling water. The combination of margarine formed in this operation gave me, after having been washed, margarine 100, potash 8·88; it was therefore pearly matter. The liquor from which it was separated, filtered several times, presented only some particles of margarine, although it contained a great excess of alkaline carbonate.

Action of Margarine upon Turnsole.

34. The strong affinity of margarine for potash having made me think that this substance could redden turnsole, I put three grammes into the aqueous extract of turnsole: when cold there was no action; but when heated the margarine became soft, without melting however, and the blue colour became red. I decanted the cooled liquor, and boiled several times the solid matter which was separated from it with new extract of turnsole. I filtered, and there remained upon the paper red clots and a blue semi-gelatinous matter, which became partly red when dried: each of these substances was dissolved by boiling alcohol: the two solutions were red, and deposited upon cooling small crystals: those coming from the former yielded margarine 100, potash 7·5; those of the latter, margarine 100, and potash 8·45. As I made these determinations on very small quantities only, I cannot vouch for their perfect accuracy; they are sufficient at least to establish that margarine takes up the potash from the colouring principle of turnsole, and that it acts like the acids.

35. The affinity of margarine for potash is not only sufficiently large to determine the formation of the pearly matter at the expense of the alkali of the turnsole dissolved in water, but it is also

also sufficient for the pearly matter itself dissolved in alcohol seizing upon its alkali, and being converted into the combination of margarine saturated with potash. If we only obtain pearly matter instead of this last combination, by boiling margarine in the aqueous extract of turnsole, this ought not to be surprising, if we recollect that the saturated combination is decomposed by water, and consequently it cannot be formed in the midst of a great mass of this liquid. This besides may be easily demonstrated by pouring water into turnsole which has been reddened by the alcoholic solution of pearly matter: at the moment of the mixture, the colour passes to blue, because the water determines the pearly matter to give up to the colouring principle of the turnsole the alkali which it had at first taken from it.

36. We have shown that margarine possesses a part of the characters of the acids; for it neutralizes the alkalinity, and attracts the potash with more force than the colouring principles do when employed as reagents. But are these properties sufficient to entitle us to place it among the acids? If we had fixed the properties which are essential to those bodies, it would be easy to decide this question; but as this has not been done, it is indispensable, before seeking to resolve it, to examine the most general characters which have been ascribed to the acids.

37. These characters are six in number; viz. 1. A sour taste. 2. Being capable of being attracted positively by electrified surfaces. 3. To neutralize more or less the salifiable bases. 4. To redden turnsole. 5. To redden the colour of violets. 6. To turn yellow or redden hematine.

1. The sour taste was the first property which served to distinguish the acids: and this character is good; for it belongs to the greater number of those bodies; and, so far as I know, none of the substances which are regarded by chemists as non-acids possess it.

2. If the second character seems common to all the acids, it is not exclusive to them; oxygen possesses it *par excellence;* and M. Berzelius asserts that sulphur, carbon, and arsenic act equally towards the surfaces positively electrified.

3. While the sour taste was the first property assigned to the acids, at a period when chemistry did not yet exist, so that of neutralizing more or less the alkalis was one of the first which was regarded as the chief property, when chemical phænomena began to be understood. It was even supposed that there must be an acid principle in several bodies which united with the alkalis, and which in other respects differed extremely from the acids.

4. The property of reddening turnsole has been strongly insisted

sisted upon; but this character does not differ essentially from the foregoing; for, the turnsole being a combination of red colouring matter and potash, it happens that it is reddened by all the bodies whose affinity for alkali is superior to that of the colouring matter. Consequently this reagent announces that the body which reddens it is susceptible of being united to potash with a greater force than its colouring matter; it does not therefore indicate acidity by itself, it simply establishes a relation of attraction between bodies which have affinity for the alkalis. The case is not the same with the indications by the violet colour of the hematine: those reagents announce the acidity by a change of colour resulting from the combination of the acid with the colouring principle itself.

5. Hitherto, the acids only have been supposed to redden the colour of violets, but there are some which act differently: thus the boric acid and prussic acids do not redden it. In all probability, it is the same with sulphuretted hydrogen.

6. The action of the acids on hematine is much more general than that which they exercise on the colour of violets: thus all of them make it pass to the yellow or the red, and there is scarcely one but sulphuretted hydrogen, which seems to make an exception; it forms with it a combination of a yellow so slight that it seems to be colourless when it is in a thin layer; and it ought to be added, that almost all the oxides which neutralize the acids act with it like the alkalis, with the exception, however, of the oxide of tin at the maximum, which acts in the manner of an acid.

38. If we now compare the whole of these characters, we find:

1. That the sour taste does not belong to all the acids: consequently it has not been regarded as being essential to those bodies.

2. That the property of being attracted by surfaces positively electrified is too general to characterize acidity.

3. That the case is the same with the neutralization of the alkalinity; for those chemists who have been most forcibly struck with this character have never formally said that it is sufficient for assigning acidity to a body which possesses it. If some of those chemists have regarded sulphur as an acid, no person has ever asserted that the oxides of lead and of zinc were in the same predicament: nevertheless, in ranking sulphur among the acids, there was no reason for separating from it the latter, which belong very evidently to the class of salifiable bases, since they form salts with the acids, and they react on hematine in the manner of potash, barytes, &c.: hence it follows that the faculty of neutralizing an alkali does not convey the idea of acidity.

4. That

4. That although the indication of turnsole has not been appreciated at its true value, it is nevertheless this character which has been most generally employed, and that which chemists seem unanimously to have adopted by tacit consent; for none of the bodies reputed to be acid are deprived of it; and this character, which differs from the foregoing only in so far as it values the strength at a given point, has been sufficient to place several bodies among the acids, and to exclude others from the latter, because they did not possess it as at first supposed. I know but two objections which can be made to this. The first is, that there may exist bodies which do not redden turnsole on account of their cohesion, and which may nevertheless approach closely to the acids: the second is, that if we have demonstrated that the property of neutralizing the alkalinity does not belong solely to the acids, we may suppose one of those bodies which, not having any other property of acidity, will redden turnsole, because it will attract its alkali more strongly than it does the colouring principle.

5. That the property of reddening the tincture of violets is not sufficiently common to entitle us to draw any conclusion relative to the bodies which do not possess it.

6. That although the indication of the hematine is much more general than the foregoing, it is nevertheless difficult to admit it exclusively, because there are acids almost insoluble, whose affinity for it is so weak that it is difficult to approach its action.

39. To resume our conclusions: We see *in the first place*, that turnsole appears to be adopted by all chemists to detect acidity. That if we do not pay some attention to the objections alluded to above as to its value, and if we persist in following the principles which have hitherto been taken as guides in the classification of the acids, we shall be compelled to rank margarine among the latter; since it reddens turnsole, takes up potash from the carbonic acid, and since its combinations with this base have the greatest analogy with the salts. If it be objected that its composition removes it too far from the series of the acids, we may cite a single example, that of sulphuretted hydrogen, which evidently possesses the characters of acidity, as M. Berthollet has proved. All the chemists, in regarding this body as an acid, have, I believe, established that in the chemical system the analogy of properties has been consulted in preference to that of composition. *Secondly*, that if we refuse to rank margarine among the acids, we are led to these conclusions: 1. That the taste and action of the colouring principles which undergo changes of colour by their immediate combination with the

acids,

acids, are the only characters admissible for ascertaining acidity: that, in this case, we shall be forced to exclude one or other of these characters, since the oxide of tin at the *maximum*, without having an acid taste, acts on the hematine in the manner of an acid. Finally, that by determining for the colouring principles, we shall be reduced to the employment of hematine only, since the tincture of violets has an action too limited. 2. That the indication of turnsole being absolutely insignificant, we must submit to a new examination all the bodies which have been ranked among the acids, from the sole consideration that they were combined with the bases with sufficient force to redden turnsole, that it is not doubtful that several of these bodies will be separated from them. 3. That margarine ought to be ranked among the fatty substances, without having any regard to its properties.

40. After having appreciated the indication of each of the characters of acidity in particular; after having demonstrated that some were too general, while others were too limited; I have detailed the consequences to which we are led, according as we admit such or such of those characters. I have not decided, because I have regarded my individual opinion as of little importance. Whatever may be the case, however, it is very remarkable to see a fatty substance in which hydrogen and carbon prevail, possessing in as marked a degree as certain acids, one of the most characteristic properties of the latter. But let it not be supposed that margarine alone is in this predicament: I have already met with several analogous substances, which I shall describe in future papers: and the observation of Messrs. Bouillon Lagrange and Vogel, that the resins redden turnsole, proves that it exists completely formed in vegetables. These facts, therefore, permit me to think that we shall be able to make a group of fatty substances, the affinities of which for the alkaline bases will be analogous to those of the oxygenated acids, and whose combinations will present species of saline compounds to which we may give the generic name of soaps, which several of them have so long gone under. This analogy of properties which we observe between oxygenated and inflammable bodies is well adapted for supporting a principle which had already guided me in my experiments on astringent substances; namely, that an analogy of properties is not always a consequence of an analogy of composition.

XXXIII. *On*

XXXIII. *On the Question, Whether Alcohol be a Product of Fermentation, or of Distillation? By* M. Donovan*.— (*Read to the Kirwanian Society of Dublin, May* 29, 1813.)

The intoxicating property of fermented liquors has been known to mankind from the remotest ages of antiquity. For a series of centuries nothing of their nature was regarded further than their agreeable taste and exhilarating effects: and of the person who first separated alcohol from them, nothing at this distance of time can be certainly known. When, however, alcohol had been obtained, it was discovered that it possessed an intoxicating power in a greater degree than the original liquor; and that the residuum of the operation no longer produced even exhilaration. Hence, by a simple and natural process of reasoning, it was inferred that alcohol is the true intoxicating portion, and that fermented liquors owe their influence entirely to its presence. Accordingly, we find this opinion to have been received without alteration until the days of Rouelle the elder. This philosopher, observing that, in the distillation of wine, no spirit appears until the temperature is raised to boiling, suggested that alcohol does not exist in wine completely formed, and that it does not assume a distinct existence until a sufficiently high temperature be applied. In support of this opinion he adduced no other evidence; and the suggestion, although admitted by some, perhaps without much examination, seemed to have obtained no particular attention until in 1785 it was revived and somewhat new-modelled by Fabroni, a Florentine philosopher. In a memoir which obtained the prize from the Academy of Florence, Fabroni asserted that chemists had been much deceived in supposing that alcohol is a product of fermentation. He stated that when he added a small quantity of alcohol to new wine, he could by saturation with potash separate precisely the quantity added, and no more; while, when the experiment was made with new wine, to which no alcohol had been previously added, not a trace of the latter could be perceived, although by distillation 25 per cent. might be obtained. Hence he thought he had a right to conclude, that alcohol is not contained in wine; that it is formed from the elements of wine by the operation of fire: in a word, that it is a product and not an educt of distillation. In further support of this opinion he stated, that when wine is distilled, and when the distilled liquor is mixed with the residue, wine is not reproduced. He allowed that a little alcohol may be found in old wines; but he accounted for this by supposing that lapse of time and the natural

* Communicated by the Author.

tural heat of climate had formed it in the liquor: for, according to his experiments, a heat not exceeding 14° Reaumur (64) is sufficient totally to volatilize the spirit *. These opinions seem to have gained but little confidence amongst modern chemists: a kind of mean seems to have been adopted. Chaptal considers the alcohol as formed, not insulated, but in combination with colouring and extractive matter †. Berthollet states his opinion in a manner less precise; he considers wine as an uniform compound, in which the properties of hydrogen are predominant; the alcohol consequently not existing in the insulated state ‡. Fourcroy allowed that without doubt alcohol is not contained exactly and purely in wine with all its properties, and under the same form as it is obtained by art: but he looks upon Fabroni's experiments as quite insufficient to prove their object; and observes that it is difficult to conceive how alcohol, which, according to Lavoisier, is reduced to vapour at 64° Reaumur (176), should in Fabroni's trials be volatilized at 14 §.

No experimental objections of any importance were offered to Fabroni's opinions until lately, when Mr. Brande published a paper on the subject ||. Fabroni, when he failed in separating alcohol from wine by means of subcarbonate of potash, affirmed that none was present. Mr. Brande shows clearly, that this is no proof; for when he distilled so much as three ounces of strong spirit from a pint of wine, and mixed the distilled liquor with the residue, he was not able to separate a particle of spirit by subcarbonate of potash; the latter combining with some of the ingredients of the wine, and the whole assuming the form of a gelatinous compound. Fabroni asserted that by subcarbonate of potash he could separate a hundredth part of alcohol purposely added to wine. It must, as Mr. Murray remarks, be considered rather singular, that this portion should be separated without loss from a fluid in which it is dissolved. Mr. Brande has shown that such a separation does not take place; and so far is Fabroni's result from coinciding with Mr. Brande's, that not until two ounces of alcohol (825) were mixed with six of wine, could he obtain a stratum of the former swimming on the surface of the alkaline solution. So far Mr. Brande's refutation of Fabroni's opinions was complete; it was proved that the grounds for objection to the common opinion were insufficient: but it remained to be proved that the opposise opinion was untrue.

The latter question Mr. Brande made some experiments to decide. His method was, to distil quantities of wine at different

* *Annales de Chimie*, tome xxxi. p. 304. † Phil. Mag. vol. ix.
† Chem. Stat. vol. ii. p. 427. § *Annales de Chimie*, tome xxxi. p. 322.
|| Phil. Transact. 1811, p. 337.

temperatures,

temperatures, from the lowest adequate degree to the highest that the operation would permit. Were the alcohol a product, and not an educt, he conceived that in these trials the quantity obtained would vary with the temperature: but since the quantities were equal, he conceived the converse proposition to be true.

It appears to me, if we suppose the decomposition of the wine to take place at the lowest degree which Mr. Brande tried, that the quantity of spirit produced should be the same at any higher temperature*.

With regard to this question, there seems to have been a strange confusion in the opinions of Fabroni. He tells us that 14° (64) is sufficient to distil off, and consequently to form the spirit; yet he considers that spirit does not exist ready formed in fermented liquors. In the brewing of common ale, the wort is mixed with yeast at 55°: the temperature rises to 70, often higher. Wash receives the yeast at 60; the heat rises to 90, and frequently to 100°. In the wine countries, *must* is set to ferment at 68°, and a much higher temperature succeeds. In all these cases the heat is much above that at which Fabroni considers alcohol to be produced; so that it is not clear how he infers alcohol to be only a product of distillation. The following passage extracted from his own writings seems to throw no light on the subject: for my own part, I do not fully comprehend its meaning: "la chaleur nécessaire à cet effet ne doit pas être considérable; car il s'en forme par la chaleur de la fermentation, à une chaleur de distillation de 14 degrés."

It has been supposed by many, that during the fermentation of liquors the evolved carbonic acid carries off with it some vapour of alcohol. If this were the case, I thought it sufficient proof that alcohol must be formed during fermentation. To ascertain it, I set a vessel of wash to ferment, and contrived the apparatus so that the carbonic acid was forced to pass through some ounces of water under tolerable pressure. The process continued for six days, and the gas which passed through the water was in considerable quantity. This water when distilled, and the product also several times distilled, manifested no signs of containing alcohol: so that, if any were produced, it was so minute as to escape detection.

I was the more surprised at this result, as Chaptal had satisfied himself that alcohol is carried off from fermented liquors. He exposed water to the gas arising from a fermenting tun: he then set it by for a month; and at the end of that time found it con-

* Since the above was written, Mr. Brande has himself stated this very objection, and has instituted new experiments on the subject, which shall be adverted to in proper place.

tained vinegar,—a proof he conceives that alcohol and extractive had been carried off from the tun.

May not the vinegar have been formed from a portion of vegetable matter dissolved in the carbonic acid, which the water had imbibed, and which by dilution and other causes had passed at once into the acetous fermentation? The supposition receives countenance from a fact which I observed, namely, that the water exposed to the gas has always the disagreeable smell of the wash.

It is well known to distillers, that a dense solution of sugar will after a successful fermentation become much lighter than water. Without allowing that the diminution of specific gravity is owing to the formation of alcohol, it will not be easy to account for this fact. I have obtained this result even by operating on a few pounds of sugar.

Wash after a complete fermentation will be found capable of becoming coloured by digestion on raspings of the *Pterocarpus santalinus*, a wood which (as is well known) imparts no colour to water, but readily to spirit.

The attraction is so strong, and the mixture so uniform and intimate, between the alcohol and the other ingredients of fermented liquors, that it is not easy to detach the former at once: yet the attraction will be considerably weakened by subcarbonate of potash. I mixed a few ounces of wash fermented at 56° with more of this salt than it could dissolve: the mixture was made in a glass cylinder of which two-thirds remained empty. When the vessel was strongly agitated, the stopper taken out, and a burning taper introduced, a blue flame descended to the surface of the liquor, precisely similar to the burning vapour of alcohol.

There were now strong presumptions that alcohol was really present in the fermented liquor. If it could be obtained in the insulated form without employing heat, it appeared to me that the question was at an end.

The opinion of Fabroni was, that at and much below the boiling point the wine is decomposed, producing alcohol by a new combination of its elements: so low a temperature as 64° he supposed to be sufficient. To make an experiment as unexceptionable as possible, I dissolved 36 troy ounces in 144 of water, and to this while at 60° I added six ounces of active yeast. I observed narrowly the commencement of fermentation; and when this took place, the vessel was immersed in cold water, which caused the temperature to sink to 56°. The process continued for ten days, during which the heat never rose beyond 57°. The fermentation being over, a small quantity of the wash was mixed with as much subcarbonate of potash as it could dissolve.

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The difficulty was to distil the alcohol off from this alkaline solution at a lower temperature than that at which it was supposed to be formed. In a rare medium the alcohol would assume the gaseous state: but the cause which first volatilized it would continue it in the state of vapour. The necessity of intense cold on one part of the apparatus became therefore evident. The following arrangement I found to answer the purpose perfectly.

A retort was selected with a long beak, on the middle of which was blown a globe; so that a fluid distilling from this retort would be detained in the globe without passing out of the beak. The beak, by a proper arrangement of caps and stopcocks, was connected with a capacious glass sphere. The wash, which had been saturated with alkali, was passed through a long funnel into the belly of the retort. The large sphere was then exhausted by a good air pump, and screwed to the retort. The cocks were opened, then shut, the sphere was again exhausted, and again screwed to the retort. Round the small globe on the beak a freezing mixture was confined by means of a tin vessel closely cemented to the glass. The cocks being opened, the liquor began to boil, and striæ of alcohol began to appear on the naked part of the neck which was kept cool by ether. After some time the sphere was again exhausted, and by peculiar management a quantity of alcohol (small indeed) was produced, which proved to be exceedingly strong and highly inflammable. In the whole of the process the alkaline wash never rose beyond 56.

These experiments I considered sufficient, and I thought I was entitled to draw up the following conclusion: "that alcohol is a product of fermentation, that it exists ready formed and perfect in fermented liquors, that it exists in them in a state of very loose combination with water and vegetable matter."

Addendum.

The foregoing was read to the Kirwanian Society, May 19, 1813: the paper was in a somewhat less condensed form, and contained other experiments and arguments, which being of minor importance are here suppressed. Since that time Mr. Brande has published a paper, in which he states, that if the extractive colouring matter be precipitated from wine by subacetate of lead, the remaining colourless liquor will easily afford a stratum of alcohol, when saturated with subcarbonate of potash. Gay-Lussac ascertained the same fact by means of litharge; and found that by distilling wine in a vacuum at the temperature of 66° an alcoholic fluid was obtained. Those who still maintain the opinion of Fabroni might perhaps consider these experiments as not free from objection, on the following accounts:

The Florentine philosopher ascertained that by an exceedingly tedious distillation of wine, the alcohol could be drawn off, and consequently formed, by so low a temperature as 63. He even allowed that alcohol absolutely existed in old wines, formed as he supposes by lapse of time. In the experiments of Mr. Brande and of M. Gay-Lussac wine was employed which must have suffered a much higher temperature than 63: and in all probability is was very old. M. Gay-Lussac, as appears by the very short notice given in the *Annales de Chimie*, distilled the wine in vacuo at 66, which is even higher than the degree which Fabroni states as sufficient to form the alcohol, and at which he distilled it off. In my experiments the heat was never suffered to rise beyond 57: by which means I conceived that I had avoided the foregoing grounds of objection. On this account I thought fit to publish the paper; believing that, as it afforded some additional evidence, it would not be looked on as superfluous.

The coincidence between my experiments and those of M. Gay-Lussac is striking: both selected the same subject of inquiry, both were employed in it at the same time, both happened nearly upon the same manner of conducting the investigation, and both drew the same conclusions*. M. Gay-Lussac had, however, read his memoir to the Institute two months before I read mine to the Kirwanian Society: but the first notice of his was given in the *Annales de Chimie*, which was not published until some time after I had given an abstract in the Philosophical Magazine; so that I could have had no knowledge of what had been done before me.

XXXIV. *Summary Ideas on the Probabilities of the Origin of Aërolites. By* Armand Seguin†.

Very few, if any, persons have seen stones fall from the atmosphere, and yet the fact is incontestable. The exact analysis of these stones made by eminent chemists; the nature and identity sufficiently constant and regular of their principles, which we do not usually meet with in the places where they are found, would be sufficient of themselves to give to the existence of this phænomenon a degree of probability very near the truth: but

* In the translation of Gay-Lussac's memoir, Phil. Mag. vol. xliii. 193, line 15, the word "not," which materially influences the sense, is an error of the press: it ought to be cancelled.

† *Annales de Chimie*, tome lxxxviii. p. 262.

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the origin of these stones, and the cause of their fall, are not yet known; hitherto conjectures only have been formed on the subject. Some distinguished men of science have demonstrated that it might be possible that they came to us from the moon, or some other celestial body. But as we are far from having exhausted all the orders of possibilities, and all of them equally admissible, it will only be when we shall have accumulated hypotheses that we may, in the absence of irrefragable demonstration, found an opinion upon the order of possibility which shall be the least repugnant to our ideas and to our knowledge. It is with this view that I shall collect under one point of view, the observations upon which we may found another probability, however unreasonable it may be considered by some, as to the formation of aërolites in our atmosphere.

The nature of the atmospheric air is well known: it can dissolve more or less water; and the more it dissolves, the lighter it becomes specifically; which establishes a continual circulation in the atmosphere. It also serves as a vehicle for many substances.

It is thus that smells, the nature of which is still a problem, are disseminated through the air. Are they merely mixed with the air? or, are they dissolved by air, by water, by the essential oils, or, finally, by other fluids? Of all this we are ignorant; but it is clear that on no occasion is the transparency of the air disturbed.

The same questions and observations apply to the oil and essence of turpentine, the effects of which are so pronounced, that by simple respiration they give the urine a very characteristic smell, and terminate even by producing asphyxia.

Every person knows that by sleeping in newly painted rooms colics are experienced, from the action of the lead on the interior of our system. Is this lead in the state of metal, oxide or salt? Is it dissolved in the air, or is it merely mixed with it? In the first supposition, is it dissolved, either by the immediate principles of the air, by the water which it holds in solution, or by other fluids, and particularly essential oil? On this point there is no kind of data, but it is always certain that this lead is carried through the air even to great distances.

Mercury, a metal still heavier, presents a similar phænomenon. When a person remains some time in a sick room, salivation comes on, and any gold which he may have upon his person is whitened, even without his having any contact with the patient.

If we place on a table a glass half full of a solution of muriate of soda, we find in a short time, not only the exterior of the glass, but also a great part of the surface of the table, covered with salt. The salts may therefore be carried by the atmospheric air to certain distances.

In the atmosphere there are frequently miasmata which do not disturb its transparency, and on which our best eudiometers have no effect. The experiments which I made with M. Lavoisier in the *Salpêtrière* prove this fact beyond all doubt. We took air in a low room, where two hundred women were in bed, the windows of which had been shut for six hours: we compared it, by means of the most accurate eudiometers, with air collected externally, and we found only the same principles; and yet the smell of the apartment was so infected, that in a very few seconds M. Lavoisier fainted, and the women threatened to break the doors and windows if they were longer subjected to the disagreeable sensations which they experienced.

May not the small-pox and a great many contagious diseases, such as the marsh fever, and the fevers resulting from the pits in which flax has been steeped, be propagated by simple respiration? Here we have another kind of substances, which may be either dissolved, or mixed with the air and carried by it to distances which can only be limited by the decomposition or alteration which the substances thus transported undergo.

If a person remains habitually in a close and damp place filled with newly printed paper, a mortal disease will be the consequence.

We might also cite those showers of insects which probably are brought by the winds, which have either taken up the insects themselves or their eggs; the showers of corn which we are assured have fallen in Spain; whirlwinds and waterspouts, which frequently carry with them heavy bodies to great heights.

Bats, foxes, civet-cats, bugs, and various other animals, have a peculiar smell, so strong that it is too easy to perceive when they are near at hand.

Dogs not only discover animals by the scent, but even their masters.

In short, it is evident that the combustible bodies, the metals, the metallic, alkaline or earthy salts, essential oils, gases, with animal and vegetable matter in general, may be either dissolved in or mixed with the atmospheric air.

We may therefore, by analogy, suppose that the substances generally found in aërolites may be in the same way either dissolved or suspended in the atmospheric air, in their natural state, the state of oxidation, or the state of salt, and carried to a certain height, either on account of their specific gravity in this state of solution or suspension, or on account of a first impulse, such as the eruption from a volcano, and there

there remain suspended from being dissolved in water, air or other fluids, and consequently from the obstacle thus presented to their fall by the lower part of the atmosphere, in the same way as the clouds, which, although specifically heavier than the air, nevertheless keep suspended in it without being dissolved.

But supposing for an instant the dissemination in the upper parts of the atmosphere of the principles of aërolites, how, it may be asked, can these principles so disseminated, and probably in minute division, be united, and form masses so considerable as those which are said to have fallen?

This question is doubtless of most difficult solution: we may, however, hazard a probable answer.

In fact, it is agreed on all hands, that the fall of aërolites takes place in storms, and particularly after explosions of thunder.

On the other hand, M. Monge, in his excellent memoir on Meteorology, has supposed with great probability that the noise of thunder was owing to the vacuum produced by a cause not yet ascertained, and immediately filled up by the strata of surrounding air.

It is not impossible, therefore, that the constituent principles of aërolites, being transported by chemical or mechanical means into the upper regions of the atmosphere, where the vacuum is produced which occasions the noise of thunder, there remain suspended by causes to which I have already alluded, until this vacuum is produced; and then these principles, although disseminated, being pressed by the external strata which fill the vacuum, unite, conglomerate, and form a mass, the more considerable in proportion to the quantity which it meets with in this place.

XXXV. *Of the Anatomy and Physiology of the Brain and Nervous System, as taught by Drs.* Gall *and* Spurzheim*.

All improvements in natural history, and particularly in the natural history of man, are interesting, and deserve to be recorded and published, in order to enable society in general to avail themselves of the labours of those ingenious and persevering individuals to whom the investigation of nature is a passion, and the discovery of truth a sufficient reward for their undertakings. Viewing in this light the important discoveries relative to the anatomy and physiology of the brain, recently made by Gall and Spurzheim, it seems expedient to communicate a short ac-

* From a Correspondent.

count of it, for the benefit of a numerous class of readers in England, who only know what is going on in the capital through the medium of the periodical journals; and to enable those who have time, and who are themselves organized for such investigations, to labour in the same wide and fertile field of inquiry, and by their cooperation to contribute to the advancement of this useful science.

Few anatomists there are, I believe, who will not be ready to acknowledge the imperfect state of our knowledge of the minute anatomy of the brain, before the time of Gall and Spurzheim's dissections. The manner too in which anatomists dissected (or rather sliced) that organ, was ill calculated to lead to any useful and satisfactory results as to its component organs. Drs. Gall and Spurzheim, by a method of dissection entirely new, have traced the various nerves to their origins, have shown the anatomy of the brain and its several organs, and pointed out the best method for future discoveries. As we hope to give a more particular account of the anatomy in our next, we must content ourselves with giving a short detail of the proceedings of Dr. Spurzheim with respect to the physiology of the brain, and the organs of the mind's manifestations, &c. Since the last account we gave in our number for June, the Doctor has finished his lectures on the science. They were attended by numerous medical persons of the first respectability, and gave great satisfaction. He has also examined many schools, and pointed out the peculiar dispositions of the different boys, merely by the examination of the shapes of their heads, in a manner which has quite astonished the schoolmasters and others who knew their faculties and dispositions. One principal part of Dr. Spurzheim's last lecture was to state the *mimic*, or expression which the action of the different organs gave to the features and muscles. In this part of the course he pointed out wherein Lavater was correct, and in what respect deficient. Dr. S. resumes his lectures on the 18th of October, at seven in the evening. He has collected many thousand skulls of persons of different characters of mind, and moulded many busts to illustrate by example his peculiar doctrines. In the conclusion of this short sketch, which we hope in our next will be extended into a more systematic account, we must observe, that some of the warmest and most able opposers of this theory have now become its advocates.

XXXVI. *Ob-*

XXXVI. *Observations on the Fall of Stones from the Clouds, or Aërolites. By M.* Marcel de Serres*.

The phænomenon of the falling of stones is in itself so singular, that we ought not to be astonished that, although observed by a great number of intelligent men†, it was long doubtful whether such an event ever took place. The ancients, much more credulous than the moderns, have almost all admitted the fall of aërolites; but when various terrestrial phænomena began to be accounted for, their existence was entirely denied, because it was impossible to explain their formation.

The first among the moderns who discussed the origin of aërolites and proved their existence, was Albert Groot, or Albert the Great, whose numerous writings fill nearly twenty-two volumes in folio ‡. But from that period to the time of Chladni, *i. e.* from the thirteenth century to the present time, naturalists have paid very little attention to this phænomenon. The reality, however, of the occurrence could not be denied. and since it has been admitted, numerous writings have demonstrated that the ancients had left us many testimonies on the subject. If the writers who have turned their attention to such inquiries have exhausted the subject so far as erudition is concerned, it would seem at least that they have not been acquainted with the various opinions hazarded lately to account for the formation of aërolites. Several German writers have in fact adopted an hypothesis on the subject of these meteors, of which the French authors have taken no notice, either because the German language is but little understood among us, or because the writings in which this hypothesis is mentioned have not come to their knowledge. The present observations are therefore intended to supply this omission; and while upon the subject of the hypothesis admitted by certain German writers, we ought not to refrain from saying a few words as to the work recently published by M. Bigot de Morogues on the same subject.

We may refer to three principal hypotheses all those which have been hazarded on the subject of aërolites. Some persons assign an extra-atmospheric origin; while others consider them, on the contrary, as formed in our atmosphere. Finally, there are who

* *Annales de Chimie*, tome lxxxv. p. 262.

† Pliny speaks of stones fallen from the clouds, as if he had seen them himself. *Ego ipse vidi in Vocontionum agro paulo ante delatum.* Lib. ii. cap. 60.

‡ Albert Groot, born at Lawingen in Suabia in 1205, was Bishop of Ratisbon, and cultivated the sciences with great success. His History of Animals is remarkable, for the times in which he lived, although the basis of this work was borrowed from Aristotle and his commentators, particularly Avicenna.

think

think that aërolites had a terrestrial origin. But all these explanations must still be subdivided, according as we refer the formation of the aërolites to this or that cause. Thus we find among those who have assigned an extra-atmospheric origin to these stones:

1. That some, with Pliny, make them come from the sun, since, according to them, they have a black colour, or look as if burnt, *colore adusto*.

2. That others, with Chladni, regard them as small isolated planets, or rather, with M. Lagrange, as fragments of small planets.

3. Lastly, that the rest, with the illustrious author of the *Mécanique Céleste*, consider them as bodies darted from the moon,—an opinion adopted by most English writers.

Those, on the contrary, who have assigned an atmospheric origin to them, have thought,

1. That they were produced in our atmosphere by the combustion of inflammable gases, which hold suspended or dissolved metallic and earthy particles.

2. Or that they have been produced in the same way as metals or earths are formed in plants, as the experiments of Schrœder and Crell have proved. These naturalists observed, that causing plants to vegetate in sulphur and charcoal, the metals or the earths which are generally found in these substances were formed by the act of vegetation.

As to those who have ascribed a terrestrial origin to aërolites, some have admitted that these substances preexisted in the places where they were found, and had been merely affected by the lightning: others, that they come from volcanos, and that they were a species of lava.

The opinion which tends to regard aërolites as formed by new combinations operated in the atmosphere by the contact of all the bodies incessantly carried up by evaporation, is as yet so little known, that it is the only one with which we shall now concern ourselves.

The authors of this hypothesis have in the first place observed, that the fall of aërolites does not appear to have taken place equally at all seasons; for in sixty-five or seventy of these falls, the epoch of which is well known, nearly two-thirds have happened in the months of June, July, and August. Lastly, they also prove that in all the winter months the falls of stones have been less frequent than in a single month of summer.

The same observation, which demonstrates the influence of the seasons on aërolites, applies equally to the different times of the day: thus, from a catalogue drawn up with great care, of all the falls of meteoric stones hitherto known, seven only fell

between

between midnight and noon, and still these stones were only precipitated at the more advanced hours of the morning, *i. e.* between eight and eleven o'clock. On one occasion only, this phænomenon was observed between eleven o'clock in the evening and six in the morning, whereas we have evidence of thirty-six falls between noon and midnight, and still the greater part took place between three o'clock in the afternoon and sunset.

The geographical situation of the places where stones have hitherto fallen, is by no means a subject of indifference to these observers. It seems, in fact, that the number of these meteors decreases with the distance from any place to the equator: thus, none have been as yet seen in Sweden or Denmark, and it has only been in the south part of Russia that four have been found; and lastly, six only have been witnessed in England. The number of aërolites has on the other hand been very great in Italy, France, and Germany.

The weather seems to have a certain influence over the fall of stones; for we have heard of none which have taken place in cloudy weather, during a great rain, or abundant snow, or, lastly, during a high wind, particularly when it was northerly, north-east or easterly. Of forty-three falls of stones, the weather during which was noticed, twenty-nine happened in warm and serene weather, and the thirtieth and thirty-first were manifested while the sky presented some scattered and insulated clouds. The remaining twelve were accompanied by very violent storms of rain and hail, such as the falls of aërolites in 1103, 1249, and 1552. The pressure of the atmosphere seems also to diminish before or after the fall. This was observed in 1806 at Alais, at Stannern in Moravia in 1808, and at Maurkirchen in Bavaria in 1811, when the sky was cloudy a little before and after the meteor.

After having shown the circumstances which accompany the fall of aërolites, the authors of the theory in question proceed to discuss their origin. Out of twenty-nine falls of stones which took place in serene weather, twenty seemed to issue from a very extensive but round cloud, black or variable in colour according to the colour of the stones themselves: thus, the cloud was white in the fall which took place at Burgos, and the stones were also white. At all times the cloud seems essential to those meteors, for from it proceeds the noise which accompanies or which precedes the fall of aërolites; and from it, the stones proceed. The extent of these meteors is not less in general than from half to a whole league in diameter, a size very different from that of the stones themselves, the mass of which is frequently of very small dimensions. We cannot account for this difference, by admitting that the vapours of the atmosphere give

give such an extent to the meteor; for then the meteor must be composed of the metallic mass, and of the vapours which it drags with it; whereas the form of this metallic mass is always more or less round and circumscribed. It must, therefore, be supposed that the greater part of these globes is not composed of metallic parts solely, while they pass through the air, but of inflammable parts which are consumed during the rapid course of those globes.

This seems also proved by the luminous phænomena which accompany these meteors, for they are not the same with those produced by incandescent metallic bodies. The colour of the flame is, in fact, white, like that of camphor or phosphorus in a state of ignition. In the aërolites of Connecticut or Weston in America, which fell in 1807, the light precisely followed the path of the eruptions; it was extinguished with every eruption, to re-appear with the one succeeding. If, as some observers have asserted, the light was the consequence of the incandescent state of the aërolite occasioned by the rapidity of its fall, then this state of incandescence must increase with the time occupied by the fall: but the case is almost always different, and several stones observed in the air are extinguished before reaching the ground. The form of these meteors, besides, agrees with this opinion; for it is not always the same: thus, the stone which fell in England on the 18th of August 1783 sometimes affected a round form, and sometimes an elongated one. The remarkable effervescence which has even been observed in several of these aërolites, seems also to prove that there is no question in the case either of a metallic consistence, or of simple vapours which surround the nucleus, because the edges are always very distinct, and are not insensibly lost. In short, if, in spite of all appearances, the greater part of this description of meteors is to be ascribed to vapours, we should nevertheless be puzzled to account for their origin, since aërolites are almost uniformly composed of earthy and metallic parts, which can scarcely be vaporized at the temperature of our atmosphere.

A parabolic orbit is generally assigned to aërolites, but the angle formed by the parabola with the horizon is not always the same. In fact, there fell in France in 1783, a stone which formed an almost horizontal hole, and that of Stannern made a cavity of two feet diameter and two inches in depth only. Other stones, like that which fell in 1810 at Orleans, or that which was observed in Calabria in the year 1755, constantly preserved in their fall a direction almost perpendicular. These facts seem to indicate, that besides gravity there must also be another force, which opposed to the direction given by the weight may modify their orbit. We have a new proof of this in the aërolite of Connecticut,

Connecticut, which before being extinguished, and after having thrice made an explosion, rebounded as many times from the ground, and consequently took a direction completely opposite to that given by the law of gravity.

As to the velocity of aërolites, it seems in general to be very great: frequently it equals or surpasses even that of the earth; but in all cases it is much more accelerated than a simple fall. Thus, after a great many observations, it is completely uniform, without increasing with the time taken up by the progress of these stones through the air.

The duration of this phænomenon seems also to present great differences; thus, it varies from a quarter of a second to a few minutes: it is also very singular, that the loud noise similar to discharges of cannon, which generally accompanies the fall of aërolites, lasted in the explosion which took place in Russia in 1787 four whole hours, *i. e.* from one o'clock until five, before the stones fell. It was also observed in 1200, before the fall of stones which happened near Abdona in Italy, that the cloud from which the stones issued as if completely inflamed, remained visible about two hours.

We may also consider it as a new proof, that besides the gravity, there is a force which influences the direction of aërolites, when we reflect on the little depth to which they sink in the ground. For a long time it has been observed, that, left to their proper weight, these stones would naturally enter very deep into the ground, if the moon was their point of departure, and if their velocity was in the ratio of their volume or mass. Nevertheless this was far from being the case in the fall of stones which took place in 1768 in the Maine, and in Gascony in 1790: several fell with little celerity, others very slowly, others more quickly, and others with such rapidity that they hissed violently in passing through the air, and without their difference of velocity having the least reference to their weight. Latterly, one of the stones which fell at Thoulouse in 1812, of the same density as the others, had touched the ground so lightly that it scarcely left a mark. Other stones which fell at Agen did not break the roofs of the houses on which they fell; which was also observed of those that fell in 1753 near Tabor in Bohemia.

It is not less remarkable to find that the great and small stones do not fall together, but that at the end of the orbit the smallest are precipitated, and the latter become greater in proportion as they approach the other extremity of the orbit. This was observed in the meteor of Stannern, which, by being directed from east to west, precipitated stones which became larger as it advanced. The meteor of Aigle, as described by Fourcroy, and of several other places, presented the same phænomenon also.

When

When we examine the cohesion of atmospheric stones, we find that it is not the same before and after their fall. A great number of these stones are in such a soft state that they are often flattened on touching the ground, and those which fell in 1768, 1753, and 1808, &c. furnish a proof of this. The case has been the same with others which have been observed in a state of fusion and fluidity; like those which fell in 1731 at Lessay near Coutances, and finally those collected in Poland in the year 1796. To conclude: all these stones became solid, and even compact, some time after their fall. This state of softness which aërolites often present agrees very well with their form, which is almost always that of an obtuse triangle, or an oval flattened on the lower side, a form which every body must present which falls from a considerable height, if it does not possess great solidity.

As to the temperature of the aërolites, it is rarely like that of the air: in general it is that of boiling water, since where they fall in a certain state of softness they adhere to sheaves of straw, or other combustible substances, without setting fire to them.

It has been asserted, since a certain number of aërolites has been analysed, that their elements were always nearly the same; but have all their constituent parts been minutely examined? For instance, has any account been given of the brown gluey matter, similar to a varnish, which covered the stones of Benares, as well as those which fell in 1775, and, finally, which was discovered on the aërolites which fell at Valence in 1806? This gluey substance was also abundant on the stones of Stannern: according to a description given of it, it seems to have been similar to cart-wheel grease. This viscous matter is probably a residue of that which in the preceding combustion was not entirely consumed, and to which must be ascribed the smoke which these stones emit frequently after their fall.

There have been even aërolites differing from those hitherto analysed: we ought to rank in this class the small white stones which, covered with ice, fell in Russia; the white flints which in 1552 made so great ravages near Schleusinger in Bavaria, and at the gates of Munich, and of which several specimens were long preserved. Lastly, the stone which fell in Ireland in 1771, and which resembled a grayish siliceous flint, like those precipitated at Burgos in 1438, and which were so light that the largest did not weigh half a pound, and some were only the size of small ear drops. This last fact, however singular it may appear, may be considered as correct, according to the account given of it by M. Proust.

Aërolites seem therefore, from all that has been observed, to have

have so intimate a connection with balls of fire, that we may be almost certain that in seasons during which abundance of ignited meteors has been seen, there have been also one or more falls of stones. In the same way as ignited meteors precede or accompany earthquakes, aërolites also meet with these great phænomena. In fact, seasons remarkable for violent storms, earthquakes, or other phænomena, have also been productive of falls of stones. We may cite as a proof the years 1618, 1650, 1654, 1668, 1674, 1723, 1743, 1753, 1755, 1768, 1812, &c. Frequently also the period of an earthquake has coincided perfectly with a fall of aërolites, as in 1764, when in the island of Funen in the north of Germany, there were in the same week violent shocks of an earthquake, and a shower of meteorolites was precipitated. The same effects have been witnessed in Germany and Switzerland: thus on the 7th of November 1742, the day on which the city of Basle suffered so much damage from an earthquake, an aërolite fell at Ensisheim, a short distance off.

We have already remarked, that the principal hypotheses proposed to explain all these phænomena are reducible to two fundamental propositions; the one may be called *cosmic*, and the other *telluric*. The former has been most generally adopted, particularly that branch of it which considers aërolites as bodies darted from the moon, and which have exceeded the point at which the attractions of the earth and of the moon are in equilibrium. We may say on this subject, that by adopting this hypothesis, it does not appear that the least attention has been given to the difference of the weather, or to the state of the barometer and thermometer, to the season, or the time of the day when these aërolites have most usually fallen. Nevertheless there exist on this subject very striking differences, and which can hardly be accounted for by adopting the theory which makes aërolites come from the moon. Nor does this theory show the relation which exists between the fall of these stones and the cloud which always accompanies their fall. This cloud even precedes in certain circumstances the fall of the meteorolites, which proves that it is not formed by the vapours exhaled by the stones, as some have asserted. This explanation would besides be inadmissible on account of the quantity of vapours, which must be in the ratio of the size of the stones. Finally, these bodies, if they came from the moon, would certainly be consumed to the last atom, on account of the length of way which they have to come, and yet they never explode until very near the ground. Those who adopt this theory regard atmospheric stones as lava, which agrees badly with the always slight and superficial oxidation of those bodies. Nor does this theory account for the explosions which always accompany the fall of aërolites; whereas in in the eruptions of volcanoes they are very rarely perceived, and in a way purely accidental. The frequently considerable inequality of their orbit, the obliquity of their direction, and their course, frequently also almost parallel to the earth, and still more the multiplied reboundings of one of those stones, (which prove a direction quite the reverse of that of gravity,) are so many proofs which will not admit of our regarding the lunar theory as the most probable.

We may add, that by adopting this theory we can by no means explain the slowness of the fall of aërolites. Bodies falling from the moon would not be precipitated on the roofs of houses, without sinking into them or damaging them considerably: but this has never been observed. The duration of the phænomenon ought to be nearly the same, making allowance for their volume or weight; but sometimes their duration is prolonged for some minutes, and on some rare occasions for whole hours.

Besides these difficulties, which are weighty, there are other phænomena which scarcely can be accounted for upon the same theory; and although these phænomena are not absolutely of the same kind with meteorolites, they are so closely allied to them that we can scarcely separate them.

We ought probably to rank with aërolites the ignited bodies, which are only distinguished from them by their substance not being metallic. Besides, they fall, like meteoric stones, in the warmest months and in calm weather; they burn in the same manner, and traverse their orbit with the same velocity; while the direction in which they approach the earth is similar to that of the aërolites. Then their explosions are nearly similar; and even there has been observed, as well as with meteorolites, particularly that of 1772, a rotation around their centre.

What merits most attention is, that these ignited globes have, like the aërolites, a roundish form and a gelatinous consistence. In fact, a globe of fire which fell in the East Indies in 1218 left, after a dreadful explosion, a large round heap of gelatine of tolerable consistence. A similar mass, but gray and spongy, was found at Coblentz after the explosion of a ball of fire *. These observations are not unique: latterly similar masses have been found of the size of a man's head †. Silverschlag even relates having seen the residue of an ignited globe which presented a gelatinous aspect with a whitish colour ‡.

* *Comment. de Rebus*, tome xxvi. p. 179.
† *Journal de Physique de Gilbert*, tome vi.
‡ *Theorie der 1762, beobachleten Fever Kugel.* Leipsic, 1764.

[To be continued.]

XXXVII. *De-*

XXXVII. *Notices respecting New Books.*

Description des Tombeaux, &c.—Description of the Tombs discovered at Pompeia in 1812. *By* A. L. Millin, *Member of the French Institute, &c. &c.* Naples: printed at the Royal Press. Published at Paris in 1814. 1 vol. 8vo.

We are indebted to the Travels of M. Millin in Italy, which lasted two whole years, for the present work, and which has been published separately, and indeed as a kind of precursor to his more ample details.

The first tomb described is square, surrounded by a wall and covered with a roof composed of flat stones resting on each other, and gradually becoming narrower so as to form on each side a kind of staircase of three steps: the last of these steps bears a square base a little elevated, on which there was probably a statue. This gives the tomb itself a very elegant form, resembling that of Mausolus king of Caria, to which, however, it cannot be compared in point of the luxury and beauty of the ornaments. It would appear that this form was much in vogue at Pompeia; for two other tombs quite similar have been found.

Without entering fully into the accurate details given by M. Millin respecting the construction and internal distribution of this tomb, we shall proceed to notice the bas-reliefs with which it is decorated. These bas-reliefs are three in number. Two are in front of the tomb, and are arranged on two plans: the first represents combats of gladiators; the second, one of those combats called *venationes*, because they took place between men and animals. The third bas-relief adorns the upper part of the entrance into the tomb: we there see five men, of whom four are armed. We shall describe this presently. All these bas-reliefs are interesting, because they convey some curious information as to the combats of gladiators and others. On the first bas-relief there are twelve gladiators who combat two and two, forming six pairs (*paria*). The latter word, M. Millin observes, has been always used to denote two adversaries. Above each pair there are inscriptions painted. The first pair are on horseback: here M. Millin takes occasion to refute Justus Lipsius and Ferrarius, who assert that these mounted gladiators were called *andabatæ*. He admits that there were gladiators known by this name; but he remarks that they wore casques or helmets which covered their heads completely, and left no aperture for the face; that they fought without seeing one another; and that no author says they fought on horseback. The gladiators on horseback were simply called *equites*, which M. Millin proves by a passage of Isidorus, and by the fine inscription of Venusium published by Fabretti, *Inscript. domestic.* c. l. n. 202.

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Those whom we see there fight with lances: they bear a small round buckler (*parma*), which is particularly suitable for horsemen, because it is lighter than the *scutum*, and are clothed only in a short tunic and a small chlamys: the vizor of their helmet is not down, and the countenance is completely exposed.

M. Millin enters into some curious details on the subject of the inscriptions which are over the heads of these combatants. We read above the first BEBRYX IUL. XV. V; and above the second NOBIL. FOR. IV. XII. M. Millin interprets the one *Bebryx Juliensis XV. vicit*, "Bebryx the Frioulian has conquered fifteen times;" and the other "*Nobilis Forojuliensis XII.* (*vicit*), "A Noble Frioulian has conquered twelve times." *Bebryx* and *Nobilis* are their names: there is nothing surprising in this last, as the proper name of a gladiator, since we find one in Winckelman *Monum. ined.* No. 197, who is called *Habilis*: as to *Bebryx*, which properly speaking is an adjective designating an inhabitant of Bebrycia, a celebrated country of Asia, where the inhabitants were renowned for bodily strength and their address in gymnastic exercises, it is proved that it had become a proper name, since we have an example in Muratori MDCCLXXXIV. 40: thus it may have been the name of a gladiator, although he was not born in Bebrycia. IUL. and FOR. IUL. designate the country of our two gladiators. There can be no doubt that the letters FOR. IUL. are the initials of the word *Forojuliensis*, which means an inhabitant of Frejus or Frioul (*Forum Julium* or *Julii*). As to the letters IUL. which are repeated above the other gladiators of the same bas-relief (with the exception of one only), M. Millin thinks with great probability that they are also the initials of the word *Juliensis*, which is all that remains for *Forojuliensis*, and that all these gladiators were natives of the same place. It remains to be decided if they were Frejulians or Frioulians: M. Millin prefers regarding them as Frejulians, because we know that Gaul furnished abundance of gladiators. The letters XV. V. and XII. designate, according to M. Millin, the number of victories gained by Bebryx and his adversary Nobilis, which seems to him the only rational explanation which can be given of it, since the same formula is repeated in all the other inscriptions: it must only be supposed that the last letter V is wanting in the second. This conjecture supplies M. Millin with an opportunity of removing a considerable degree of obscurity in the elegant Venusian inscription mentioned above: "This inscription," he says in a note, "is separated into four divisions by the words *Equites, Traces, Myrmillones, Velites, Oplomachi, Samnites, Retiarii, Scisores, Galli*, which designate the different classes of gladiators of this troop. The name of the gladiator is frequently accompanied by the initials of another word, of which Fabretti and Lupuli have

have given no explanation : perhaps it indicates the country of the gladiator. The number which follows is that of his victories : the word *vicit* is given by the letter ⊳ placed thus. Finally, in the last column we see a T, or a number : the letter T must signify *Tiro*, or apprentice ; and the number, that of the years which the gladiator has passed in the troop. According to my conjecture, we may explain the following passages in this way : SECVNDVS POMP II⊳ II. Secundus of Pompeia conquered twice, and has since served two years. DORYS PIS VI⊳ III. Doris of Pesaro has conquered six times, and served four years. HILARIO ARR VII⊳ VIII. Hilario of Ariano conquered seven times, and served eight years, &c."

All the other gladiators represented on this bas-relief are on foot : those of the second pair have legs covered with plates of metal, and their body is begirt with similar plates : they rest on a buckler formed like the Roman *scutum*, and are ready for fighting. The inscriptions above are not so well preserved as the foregoing, and the names of the two gladiators are lost. We read above the first IUL. XV. which indicates that he was a Frejulian, and that he had been fifteen times victorious : above the second is written IB . XXX . V : M. Millin thinks that the two letters IB. might have been the initials of the word *Iberus*, and that the gladiator who was thus thirty times victor was a Spaniard : this is the only one who is not designated as a Frejulian.

One of the gladiators of the third pair is grievously wounded, and his blood flows on the arena : the other has bent forward on one knee, and raises his left hand. Does the latter supplicate his life, or on the contrary does he patiently await the blow which his adversary is about to give him ? In the course of this inquiry M. Millin elucidates several practices in the combats of gladiators, and cites various expressions of ancient authors on this subject. We need not follow him through these interesting details, but shall adhere to the inscriptions relative to this pair of combatants. These inscriptions are placed on two lines, both above the gladiator on his knee, so that we cannot ascertain at first sight to whom they refer. M. Millin succeeds, however, in settling what belongs to each. The upper inscription is thus conceived : SVS IVL. XV M . Θ . The letters SVS are the last of the gladiator's name, and it is impossible to complete it, because a great number of names have the same termination. The consecutive letters IUL. XV announce that the gladiator was of Frejus, and that he was fifteen times victor. The letters M. Θ. remain to be accounted for. M. Millin proves by the joint testimony of inscriptions and authors, that the last of these letters, which has the form of the Greek *theta*, is a funereal letter, announcing that a man is dead, and that it is employed even in Latin inscriptions. He thence concludes that the inscription thus terminated by that letter should belong to the kneeling and conquered gladiator: we learn also that he died in this contest, after having been victor in fifteen other combats. Another inference which M. Millin draws from hence is, that the letter M, which precedes the Θ, cannot be supplied by reading *Missus* (sent back), and that it must be the initial of the word *Myrmillo*, which we thus find in the list of gladiators. The upper inscription belonging as we have seen to the gladiator conquered, the lower ought to refer to the conqueror: his name is thus expressed in full, HIPPOLYTVS, and the letters which follow in bad preservation, belong to the words FO IUL V. which inform us that this Hippolytus was five times victor. M. Millin here takes occasion to give an explanation of the singular Mosaics of the Albani Villa, which Winckelman has figured, but without any description.

The fourth pair presents no difficulty. The two combatants have thrown aside their bucklers, and the conqueror, who has lost his helmet, puts his hand upon his adversary as if to hold him so as to receive the sword with which he is about to strike him. There is only one inscription here, and it is uncertain to which of the two it belongs. We read: QVS IUL VI, Quintus the Frejulian has conquered six times.

The fifth pair is remarkable from the two myrmillos being armed with a trident, which is contrary to what has been transmitted to us respecting the gladiators; viz. that the myrmillo was armed with a fork or trident (*fuscina*), and that he attacked the *retiarius* who held a net, or the *tracis*, who carried a round buckler and a crooked sword. This proves that customs varied among the Romans according to times and places; and M. Millin thence takes occasion to remark, that it is a general error with all those who give treatises on the manners and customs of the Romans, not to distinguish the æra at which the authors wrote, whom they quote, and the country to which they belonged, erroneously regarding as general, usages which have been modified by a thousand circumstances. Here also there is only one inscription, NITIMO F IVL V, which it is difficult to ascribe to either gladiator. The first letters are the termination of a proper name which cannot be filled up: the others show that the gladiator was a Frejulian, and that he had conquered five times. The termination of the proper name in O, which is Gallic, confirms M. Millin in his conjecture that the gladiators in this bas-relief were Frejulians rather than Frioulians.

The last groupe also has no more than one inscription. We there read the end of the name of one of the combatants which terminates in A, and the letters IVL XV M, which inform us that this Frejulian had conquered fifteen times, and that he was a myrmillo.

Independent

Independent of these inscriptions peculiar to each groupe, there was another which occupied perhaps the whole length of the bas-relief, and which probably contained an account of the personage to whom this tomb belonged, and of the games which were celebrated at his funeral: there now remain a few words only, and part of these even is effaced. M. Millin reads thus: MVNERE QVINTI AMPLIATI PUBLII FILII SVMMO, and he interprets them into "the last offices performed to Quintus Ampliatus, the son of Publius." This explanation he thinks is simple and natural, and he enters into a long discussion in support of it: this discussion was provoked by another inscription found a short distance from the tomb, and which has been placed on its principal front because it was thought to belong to it, and because it fits the place extremely well where it has been fixed. If this inscription really belonged to this tomb, it was not Quintus Ampliatus, but one called Riccius Scaurus, a Duumvir, whose ashes it contained; and Ampliatus can only be regarded as the magistrate who superintended the games celebrated in honour of this Scaurus; so that the fragment of the inscription in question* ought to be interpreted in this manner: "In the great games which were given under the direction of *Quintus Ampliatus*." M. Millin thought at first this last interpretation was forced, and not conformable to the rules of good latinity. He inclined to think that the inscription which seems to ascribe the monument to Riccius Scaurus, really could not have belonged to this monument. Subsequently, however, the manner in which the stone fitted the tomb, and the discovery of another inscription found in the Basilicon, compelled him to change his opinion. This inscription speaks of one Ampliatus, the manager of a troop of gladiators. It is thus conceived:

N .. FESTI . AMPLIATI .
FAMILIA GLADIATORIA . PVGNA . . . . ITERVM
PVGNA XVI K . IVN VENAT VELA

"The troop of gladiators of N. Festus Ampliatus will combat for the second time. Combat the XVI. of the Kalends of June. Hunting and Hawking."

* It is thus conceived:

RICIO AF MEN
SCAVRO
II VIR ID
. ECVRIONES LOCVM MONVM
(X) (X) IN FVNERE ET STATVAM EQVESTR.
FORO. PONENDAM CENSVERVNT
SCAVRVS PATER FILIO.

To Riccius Scaurus, son of A. of the Menenian tribe, Duumvir for the administration of justice; the Decuriones have given the ground for the monument, and 2000 sesterces for the funeral, and decreed the erection of an equestrian statue in the Forum. Scaurus the father to his son.

 The

The second bas-relief which adorns the first tomb represents, as we have observed, a chase, another description of spectacles with whch the funeral ceremonies of the dead were accompanied. We there see a rabbit, a hare, and a stag, pursued by dogs, and two *bestiarii* combating with a bull and two boars.

The third bas-relief, and that which adorns the upper part of the gate of entrance to the tomb, shows us four gladiators whose bodies are covered with plates of metal, and who bear helmets, bucklers of different forms, and cuirasses more or less ornamented. One of these gladiators is wounded; the three others are standing. One of the latter, who seems to have acted the chief part in the games, is led by a man habited in the simple chlamys. He is perhaps the *lanista*, or chief of the troop. M. Millin thinks with reason that this bas-relief refers like the former to the games celebrated at the funeral of Riccius. The gladiators who are therein represented are probably the conquerors, four of whom only have been figured because the two others are supposed to be killed, or their victory disputed, or perhaps merely because the space has only admitted of the four principal gladiators being represented.

M. Millin here introduces some interesting matter as to the passion of the inhabitants of Pompeia for scenic games, remarking the bad style of the figures upon the bas-reliefs in question, and commenting on the technical part of these bas-reliefs and the general form of the tomb.

The second tomb abuts upon the first, and is only separated from it by a wall. It is a round tower raised on a square base, and surrounded by a wall adorned with pilasters. The angles of this wall bear cubes terminated by small pyramids, and decorated on one side with stucco bas-reliefs descriptive of funeral ceremonies and the state of the soul after death: these are allegories like those usually seen on painted vases. M. Millin has figured two of these small bas-reliefs: one represents a woman, perhaps the wife of the deceased, who sacrifices to his manes by offering upon an altar loaded with fruit a patera and a fillet, symbols of the piety and purity of the initiated: on another we see another woman who decorates with a fillet the skeleton of the defunct, to indicate that, purified by the sacred mysteries, he will enter into the fortunate islands destined to receive the souls of virtuous men. This explanation which M. Millin gives of these bas-reliefs is confirmed by the allegorical figures which are painted *al fresco* on the walls in the inside of the round tower, where are to be seen dolphins and other sea animals, other symbols of the felicity which virtuous men purified by initiation are to enjoy in the fortunate islands, where their souls are conveyed by nymphs seated on these animals:

mals: this tomb has no inscription; the entablature only which contained it is to be seen in the middle of the wall. The name of the person to whom it belonged is therefore unknown.

M. Millin has devoted three plates to the two tombs just mentioned. The first plate contains the plans and sections, the second gives the perspective, and in the third are figured more at length the bas-reliefs with which it is adorned.

The third tomb is figured in the fourth plate, and the fifth gives the minor details. The general form of this tomb is very elegant: it is nearly the same with the tomb of Riccius, but it is of very fine white marble and has different ornaments. It is surrounded by a wall absolutely similar to that of the second tomb. We read the following inscription on the principal front:

C. CALVENTIO QVIETO
AVGVSTALI
HVIC OB MVNIFICENT DEÇVRIONVM
DECRETO ET POPVLI CONSESV BISELLII
HONOR DATVS EST.

"To Caius Calventius Quietus, Augustalis; the honour of the Bisellium was decreed to him by a decree of the Decurions and the consent of the people, on account of his munificence."

M. Millin enters into some interesting inquiries respecting the Bisellium mentioned in this inscription. Notwithstanding the learned and voluminous treatise of Chimentelli, entitled *Marmor Pisanum de honore Bisellii*, and the labours of Noris and Mazocchi on the same subject, the former in his *Cenotaphia Pisana*, and the latter in his *Tabulæ Heracleenses*, p. 155, neither the use nor form of the Bisellium was known, and it was equally uncertain what was the meaning of the "honour of the Bisellium." The tomb at Pompeia now under examination is the first monument which affords us an authentic representation of this kind of seat; it is given under the inscription which we have cited, and M. Millin has exhibited it separately in his fifth plate. It is a kind of bench capable of holding two persons, although it was used by one only, and it was more or less ornamented, as well as the cushion which was placed upon it: this is evident, on comparing this *bisellium* with that which will be found in the next tomb described in the seventh plate. As to the kind of honour of which the *bisellium* was the distinctive mark, M. Millin, in comparing the above inscription with that in the fourth tomb, and with another which has been published by Fabretti, *Inscript.* III. 324, deduces from the comparison the following facts; viz. that the use of the *bisellium* was granted to persons of high consideration; that they had a right to sit higher than the rest at the theatre, the forum, and at public

 games

games and festivals; that this honour was decreed in the name of the people by a decree of the Decuriones; that it was obtained by public services or by acts of munificence; finally, that he who received the honour was afterwards entitled to the appellation of *Bisellarius*. M. Millin observes that all the persons mentioned in the inscriptions as having obtained the honour of the bisellium were *Augustales*, or Priests of Augustus, and he is inclined to think with Fabretti that this honour was peculiar to them. This kind of distinction, he adds, was absolutely municipal; it gave no rank, no prerogative, and even no distinction out of the city where it was decreed; this is the reason why no author has mentioned it, although it is contained on a great number of inscriptions.

The principal front of the tomb is agreeably decorated with mouldings which surround the inscription, and leaves of the palm tree enwreathed form festoons around, the extremities being adorned with rams' heads. The two lateral fronts are ornamented with crowns of oak leaves tied together with fillets. Lastly, on the small pyramids raised on the outer wall there are stucco figures, the most interesting of which M. Millin has engraved in his fifth plate. Among them is Victory on a globe, holding a garland or fillet: Œdipus who unravels the riddle of the sphinx, and probably the same hero who rests after having explained it. Behind him there is a column on which his sword is suspended by a belt, and this column is surmounted by a sphere. These figures are emblems of the employment of life, the uncertainty of the future, death, and finally the mystical doctrine of the ancients.

The fourth tomb, as figured in the sixth plate, has an inclosure similar to the foregoing; but the pyramids which terminate the cubical stones with which this wall is decorated are without sculptures. The form of the edifice itself is nearly the same with that of the tombs of Ampliatus and Calventius. In the middle of the principal front we read this inscription;

NAEVOLEIA LIB TYCHE SIBI ET
C MVNATIO FAVSTO AVG ET PAGANO
CVI DECVRIONES CONSENSV POPVLI
BISELLIVM OB MERITA EIVS DECREVERVNT
HOC MONVMENTVM NAEVOLEIA TYCHE
LIBERTIS SVIS
LIBERTABVS Q ET C MVNAT. P FAVST F VIVA
FECIT

"Naevolia Tyche, a freed woman, to herself and to *C. Munatius Faustus, Augustalis* and *Paganus*, to whom the Decurions, with the consent of the people, have decreed the bisellium, on account

account of their merits. Naevoleia Tyche has caused this monument to be erected while still alive, for her freed men, her freed women, and for C. Munatius son of Faustus."

Under this inscription is a bas-relief representing the solemn sacrifice which probably took place at the funeral of Munatius. This bas-relief and the inscription have a very rich frame-work, in the centre of which, and above, is the bust of Naevoleia Tyche who constructed the monument. On one of the sides we see the *bisellium* of Munatius: it is less ornamented than that of Calventius, and is placed within a frame-work of flowers and leaves of the *acanthus*. The other side has, in the midst of a frame precisely similar, a beautiful marble bas-relief representing a bark freighted with four funeral genii, who perform the office of boatmen. One of these is in the act of ascending the principal rope, and lowering the sail which two others are occupied in furling on the yard: the fourth, who is standing upright, seems to direct this manœuvre, and to superintend its execution. M. Millin regards this manœuvre as an ingenious allegory of the painful voyage which the soul is compelled to take in this life: after many storms, he observes, death offers a secure harbour, and it is into this harbour that Naevoleia, who is stationed on the poop covered like the shades of the departed with a long veil, is about to enter. All these details are exhibited in the seventh and last plate.

In the interior of this tomb, which is square, several vases of a red earth were found, with figures on them in relief; these vases appear to be Gallic, if we were to judge by the nature of the earth, the workmanship of the relievo, and the style of the design: they completely resemble those which are found in such abundance in France, Belgium, and Great Britain. M. Millin does not think it more astonishing that an inhabitant of Pompeia should possess Gallic vases, than to see nowadays at Naples porcelain from Saxony, Sevres, China, and Japan. Glass phials have also been found filled with a reddish water which had probably contained an animal substance. These were so well closed up, that the liquor was not evaporated, and it had a nauseous taste.

Close to the entrance of the tomb is one of these marble pilasters surmounted by a ball, which are very common at Pompeia. These marbles are generally sawed throughout their whole length, and they bear an inscription; M. Millin has engraved one of them in the first plate. These stones were simple commemorations: M. Millin thought at first that their form was allegorical, and a symbol of Fatality, expressed by the sphere, which Lachesis uses to draw the horoscope of men. Subsequently, however, it was found that there was the representation of human hair behind,

behind, which proves that these were heads sawn half through, the back part having still the mark of the hair, and the flattened face bearing an inscription. These heads are thus figured in the work of M. de Clarac, entitled "Pompeia," and in that of M. Mazoi. M. Millin gives in a note the inscriptions which he remarked upon these stones.

Close by the monument of Naevoleia there is also an inclosure, within which instead of a tomb is a *triclinium*, which was probably one of its appendages, although there is no inscription to make it known, nor any communication between the two monuments. This *triclinium* is built of brick, and covered with stucco: it is of a sloping form towards the walls, and rises towards the square table which is in the middle. It was destined to represent the funereal repose, of which mention is so often made in ancient inscriptions. It is also figured in the first plate.

We now come to the last tomb described by M. Millin, and which is also engraved in the first plate. This tomb is in the form of an altar, and is built of quadrangular stones carefully hewn. On three sides of a square stone is this inscription in beautiful characters:

M. ALLEIO LVCCIO LIBELLÆ PATRI
AEDILI II VIR PRAEFECTO QVINQ ET
M ALLEIO LIBELLAE F DECVRIONI VIXIT
ANNIS XVII LOCVS MONVMENTI PVBLICE
DATVS EST. ALLEIA M F
DECIMILLA SACERDOS PVBLICA
CERERIS FACIVNDVM CVRAVIT VIRO
ET FILIO

"To Marcus Alleius Luccius Libella, the father, Ædile II *Vir*, prefect for five years, and to Marcus Alleius Libella, his son, Decurio, who lived 17 years. The ground for this monument was given by the people. Alleia Decimilla, daughter of M. public priestess of Ceres, took care to see it executed for her husband and her son."

M. Millin here terminates his present labours; and with the generous feelings peculiar to a man of true genius and learning, he informs us that he would have been still more ample in his details, but for an intended publication by M. Mazoi, to be entitled "The Ruins of Pompeia," and which is now in the press.

M. Millin, with all the fire of a classical antiquarian, thus expresses his indignation against those who have defaced many of the beautiful monuments of Pompeia, by chipping off fragments or carving their names upon the most conspicuous places. With this honest effusion we shall close our account of his valuable work.

"The

"The spoliations which are daily committed at Pompeia are truly afflicting. It would seem as if the mania of destruction was added to the effects of an insane admiration, so as to leave nothing behind. Wicked and foolish persons are in the habit of breaking altars and columns, and defacing ornaments. Pretended amateurs, ridiculously zealous, profit by the absence or inattention of the guardians, and even sometimes display their folly and cupidity by trying to remove bronze letters, or to detach portions of Mosaic work. As it is, these Mosaics are spoiled by having the dust scraped off with which they are covered in winter, to show them to the curious. The finest paintings are defaced by incautiously washing them with a view to make their colours appear richer. But what shall I say of the odious mania with which some travellers are seized, of writing their names with a pointed instrument, not omitting the place of their birth, and the day of their mischievous visit to Pompeia! It is even fortunate that they do not always add the names of their sweethearts! In regarding this sad mixture of names of all nations, we may suppose we are reading the registry of the grand assembly in Pandæmonium, or of the general meeting which is to take place in the valley of Jehosaphat. It is painful to reflect that the names which disfigure the beautiful coloured stuccoes, the finest paintings, and the most elegant arabesques, do not belong to the lower classes of society, whose want of education might excuse them. We find, on the contrary, the names of a great number of persons very well known in the world, well educated, and of distinguished rank. I have collected a long list, and if I should publish it I should be accused of a want of decorum: I should be doing nothing more, however, than contributing to introduce these *aspirants* a little sooner to the temple of glory, and giving them a little more of that celebrity which they desire. Would it not be possible, for instance, to prohibit all entrance into the houses and other buildings of Pompeia, to those who are not attended by a guardian, leaving the streets only free of access? Why not inflict severe penalties upon those who touch these monuments with profane hands? A large fine ought to be imposed on those who carve their names on the stucco or stone; nay, I could wish that the exaction of the fine and the name of the transgressor should be publicly announced in the Neapolitan *Moniteur*. I could wish also that they would affix to the article the following verse of Martial:

"Nomina stultorum semper in mœnia leguntur."

"The names of fools are always to be found on the walls."

Mr. T. F. Forster has in the press a work entitled *Flora Tonbrigiensis*; or, A Catalogue of Plants grown in the Neighbourhood of Tunbridge Wells.

XXXVIII. *Intelligence and Miscellaneous Articles.*

ATMOSPHERIC PHÆNOMENON.

On Sunday night the 11th of September, a luminous arch extending from the western to the eastern horizon was visible to the inhabitants of the greater part of Britain and Ireland. This phænomenon, interesting from the rarity of its occurence, was, in the present instance, rendered still more so from being in opposition to the received notions of the causes which produce it, and the season of its appearance. It has been supposed that they appear only at the full of the moon, and are occasioned by the refraction of her rays in drops of rain. Now the bow in question appeared four hours before the rising of the moon, which, instead of being at its full, was 26 days old, and there was no rain at the time, the evening being uncommonly serene, the sky unclouded, and the air rather dry and frosty than humid; the thermometer at the time standing at 50, and the barometer rather above 30. The stars during the continuance of the bow were particularly brilliant, but some time after its extinction they became dull. Our informant first observed this phænomenon at 20 minutes past eight. It was a strongly marked and rather luminous band, less in its arc than the *Iris solaris*, and without any variety of colour; it was at that time some degrees above Orion's dagger, which appeared to be nearly the centre of the arc; at 40 minutes past 8 the north-west limb was fainter than the other, and the whole appeared to decline; at 8. 50. it became broader, and covered the first star in the dagger; at 9 it extended to the middle star, contracting and fading at its extremities; at 5 minutes past 9 it was barely discernible; and at 10 minutes past 9 was totally dissipated; large light patches were, however, seen till nearly 12 o'clock, about, but more particularly above, the space which the bow had occupied. A smart frost took place during the night; the thermometer at 12 had fallen to 47.—Although our informant, at the time of viewing this bow, could not distinguish any clouds or mist, yet, on his approaching the city from the southward, between seven and eight o'clock in the evening, he observed a dun-coloured and rather dense vapour, suspended over the town, rising in the south to a considerable height, and, as he supposed, formed by exhalations from the city. Whether this vapour had dissipated at the time the bow appeared, or, if not, whether the vesiculæ of which it was composed, and which were so small as not to obscure or even dim the brilliancy of the stars, were sufficient to refract the rays of light passing into them, he professes himself incompetent

incompetent to decide. The weather for a fortnight previous had been uncommonly fine, and the day following, on which our informant wrote, was the same.

DETONATING BALLS.

These balls, which have for some time been exhibited at Lectures as a chemical experiment, are now sold in some shops in London. We are sorry that they should thus be put into the hands of children and foolish people. They are formed by inclosing in little glass bulbs about a quarter of a grain of fulminating silver, and then covering the bulb with thin paper pasted all over it to secure the orifice. Children by putting them into their mouths may be much injured by their explosion—received into the stomach they would cause death.

SCHOOL OF PHYSIC IN IRELAND.

The Education of Medical Students is committed to six Professors. Three of these, viz. the Professors of Anatomy, Chemistry, and Botany, are on the foundation of Trinity College, and are called the University Professors. The other three, viz. the Professors of the Institutes of Medicine, of the Practice of Medicine, and of Materia Medica, are on Sir Patrick Dun's foundation, and are called the King's Professors.

The Lectures on the above subjects commence on the first Monday in November, and terminate on the last day of April in the succeeding year. Dissections and Anatomical Demonstrations begin with December, and end early in the following April. The Lectures on Botany commence on the first Monday in May, and continue to the end of July.

The Terms of attendance on each Course of Lectures are four guineas.—The Terms for Dissections and Anatomical Demonstrations are six guineas; for which also, half of a subject for the Muscles, and half of another with the Bloodvessels injected, are furnished. The Student is likewise at liberty to dissect any number of subjects he may procure at his own expense. For the use of the Dissecting-room during the Season, without Subjects being furnished, and for the Demonstrations, five guineas are paid; or for the Demonstrations alone, four guineas.

The Students in Botany have the use of one extensive Botanic Garden, and access to another in the immediate vicinity of Dublin. The opportunities of acquiring a knowledge of other branches of Natural History are also very considerable, especially of Mineralogy, by means of the valuable collection of Leske which is open to the public.

The Clinical Hospital, belonging to the School of Physic, is supported by a large endowment of the late Sir Patrick Dun. It

It is calculated to contain above One Hundred patients; from which number, Thirty are selected for the purpose of Clinical Instruction and Lectures. Each of the six Professors acts in succession for three months as Clinical Lecturer and Physician. A large Library of Medical Books bequeathed by Sir Patrick Dun is appropriated to the use of the Students attending the Clinical Hospital. Fee for one year to Sir Patrick Dun's Hospital is at present three guineas, which includes the attendance on the Practice of the Physician in ordinary. The Terms for each Course of Clinical Lectures are three guineas.

Degrees in Trinity College.—There are Two Classes of Medical Students in Trinity College. Those of one Class are matriculated in the University. Three years afterwards, on producing Certificates of their Matriculation, and of their having attended Lectures on Anatomy with Dissections, Lectures on Chemistry, Botany, Materia Medica, the Institutes and Practice of Medicine, the Clinical Lectures and Practice of Sir Patrick Dun's Hospital, they are examined by the Six Professors, and obtain a Diploma from Trinity College to practise Medicine. This Diploma confers all the advantages of the Medical Degrees taken in the Colleges of Edinburgh and Glasgow.

The other Class of Medical Students in Trinity College go through the regular Academical Course of Study. Three years after taking the degree of Bachelor of Arts, having likewise completed their Medical Education, they are admitted to an examination before the Regius Professor of Physic, and the Professors of Anatomy, Chemistry, and Botany, in the University; and after performing the usual Academical Exercises, they take the Degree of Bachelor in Medicine. Upon sufficient Standing, writing a Thesis, and undergoing a second Examination before the Medical Professors of the University, the Degree of Doctor in Medicine is conferred. These Degrees rank with those in the Universities of Oxford and Cambridge.

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

Certificates of attendance on the Professors in the School of Physic in Ireland are received in the Universities of Edinburgh and of Glasgow, as giving standing in those places.

LECTURES.

Mr. T. J. Pettigrew, F.L.S. will commence a Course of Lectures on Human Anatomy and Physiology, adapted to the Amateur, on Friday the 28th of October. The Course, to be comprised in 18 Lectures, will be delivered on Wednesday and Friday Evenings, at Half past Eight o'clock precisely. Admission to the Course, Two Guineas. The Introductory Lecture will consist of a general View of the Animal Structure.

Particulars respecting the Lectures may be obtained by application to the Lecturer, No. 3, Bolt Court, Fleet Street.

Meteorological Observations.

Since the 24th ult. I have not been able to keep a regular journal of the weather. The greatest part of the time I spent at Hastings, on the Sussex coast. The weather has been variable, with no remarkable circumstances. There were several observations on the clouds which I made by the sea-side, which I shall take this occasion to mention, as they may be interesting to the nepheological reader.

I noticed that the *cumuli* when over the sea were lower down than those over the land at the same time, as if the diurnal vapour plane extended higher above the land than above the surface of the ocean. When the same clouds passed over the land, they seemed to rise somewhat higher. Among the colours which the clouds refracted on different occasions, but particularly with east wind, I noticed the frequency of a sea-green tinge in the *cirrus* and other light clouds. I have seen this before on land, though less frequently than in maritime situations. The first impression on my mind was, that the evaporation of sea-water in which some salts might ascend, might possibly be the cause of the glaucous colour of the cloud. I think, however, it must have been owing to some peculiar refractive power originating in the structure of the cloud, and not to any colouring matter; as it was only in certain situations in respect to the setting sun that the green colour was seen. Some small meteors or falling stars of the common kind have happened in clear nights with east wind. After some days of very clear dry weather, and wind easterly; on the 19th of September the sky became full of *cirrus*, *cirrocumulus*, and *cirrostratus*, with rain at night. The 20th, 21st, and 22d, warm, and wind SW, and gentle showers. This evening (the 23d) we have had a thunder shower about nine.

Clapton,
Sept. 23, 1814.

THOMAS FORSTER.

P. S.—I shall be obliged to any of your correspondents who can favour me, through the medium of the Philosophical Magazine, with a scientific account of the luminous atmospheric phænomenon mentioned in the Chester newspaper of last week. It must have been a kind of *aurora borealis*.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For September 1814.

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	52	63	52	29·95	46	Fair
28	52	60	52	30·11	51	Fair
29	57	69	57	·13	56	Fair
30	57	72	57	·23	56	Fair
31	56	69	55	·27	45	Fair
Sept. 1	54	67	56	·34	56	Fair
2	56	69	57	·27	62	Fair
3	52	60	51	·29	41	Cloudy
4	51	66	53	·31	56	Fair
5	52	67	55	·20	60	Fair
6	55	66	54	29·79	54	Fair
7	55	55	54	·75	0	Rain
8	55	60	52	30·11	37	Cloudy
9	52	61	50	·12	46	Fair
10	50	61	52	·12	44	Fair
11	50	60	50	·16	48	Fair
12	49	61	50	·25	56	Fair
13	45	59	49	·20	40	Fair
14	45	61	50	·17	42	Fair
15	50	66	52	·15	51	Fair
16	50	69	53	·17	49	Fair
17	54	70	54	·17	56	Fair
18	55	72	54	·18	54	Fair
19	53	74	58	·08	56	Fair
20	60	75	61	29·91	48	Fair
21	60	65	55	·80	36	Cloudy
22	55	63	53	·81	42	Fair
23	54	62	56	·81	33	Showery
24	56	63	60	·66	34	Showery, thunder in the evening
25	60	67	55	·63	36	Fair
26	57	63	56	·78	30	Showery

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

XXXIX. *A Geological Description of the Neighbourhood of Bristol. By* W. H. Gilby, *Esq.*

To Mr. Tilloch.

Sir,—The paper which I have ventured to transmit to you, contains a geological description of the neighbourhood of Bristol. Some time after I had commenced my inquiries, I saw for the first time Mr. Townsend's very valuable book "On the Character of Moses established for Veracity as an Historian," wherein I found interspersed throughout the body of the work much important information respecting the structure of this quarter. Admirable as his descriptions are, it appeared to me that some interesting particulars, having perhaps no immediate relation to his conclusions, remained yet to be detailed. In the paper that I have addressed to you, I have endeavoured to supply those particulars, and to give a more connected account of the formations in this quarter; and should it appear to you to be worthy of a place in your Journal, you will oblige me by inserting it.

I am,

Very respectfully yours,

W. H. Gilby.

York Crescent, Clifton, Bristol,
September 24, 1814.

The rocks in this quarter may be divided into those that are inclined, and those that lie unconformably in a horizontal position upon the tops of the inclined strata. In the following description I shall first speak of the inclined strata, and begin with the limestone and its accompanying formations, which range round the country somewhat in an elliptical form. I shall then speak of the coal-measures which this ellipsis incloses. The horizontal strata will then be treated of; and I shall, lastly, enumerate those minerals which occur partially, or in veins, in the preceding formations.

The rock which forms the most prominent feature in this quarter, and gives rise to all the bold and picturesque scenery which many parts of this country are remarkable for, is the *gray, compact, mountain limestone.* Mr. Townsend has very accurately described the different localities of this rock. It ranges round Bristol in almost every direction, and forms, in fact, an irregular kind of basin, of which the most northern points are the hills about Thornbury. On the east it is well observed in a continuous range at Wickwar and Sodbury; and in a very bold manner at Week, about half way on the upper road to Bath from

from Bristol. It is wanting on the south-east, in the distance between Bath and Elm near Mells, where it reappears and constitutes the whole of the Mendip Hills, which run east and west to the south of Bristol. On the west it runs in a wide ridge which commences about three miles to the north-east of Langford, and continues to within about two miles of Bristol. On the north-west it is seen at Clifton and Westbury. Independent of this limestone ellipsis there is a subordinate range which begins from Leigh and Ashton Downs, and runs towards the Channel above Belmont, Wraxal, and Tickengham. The inclination of the limestone wherever seen is always at a considerable angle, being never less than 35°. At Week the strata are elevated to an angle of at least 65°. At the Mendips near Langford, and on the road near Westbury, the dip is seventy; and in some parts of Blais Castle grounds the strata may be said to be completely vertical. It is very remarkable that the direction of the dip varies as we observe it at different parts of the basin. At the north the inclination is towards the south. On the east, at Sodbury, due west. On the south, at the Mendips, they incline towards the north. This, however, is only to be said of that part of the range which has a northern aspect; for that portion of the south part at Cheddar Cliffs, and the hills above Wells and Axbridge, are observed to incline towards the south. The Langford road passes along the westward ridge, and wherever the dip is seen, it is towards the east; and at Barrow, where there is a large quarry, it is due east. There are some places, however, where we find that the inclination does not conform to this arrangement. If we diverge from the Langford road, and come any where upon the brow of the ridge above Brockley, Backwell, or Bourton, we find the dip completely altered; instead of its being as it is on the Langford road eastwards, we find it almost due north. This part of the range forms, in fact, the southern boundary of the Nailsea coal-field, which lies between it and the opposite range, passing above Wraxal, Belmont and Tickengham, where the dip is always tending towards the south. We therefore find the Nailsea coal-field shut in by two barriers, each dipping towards the other. From Clifton to Westbury the limestone sinks towards the south-east, but about half a mile beyond Westbury the inclination is to the north. If we go from this point westward towards Kingweston Hill and Pinfield Point, we find the same dip to prevail. It is probable that this variation in the dip is connected with some coal formation in the plain below. I am confirmed in this idea by having observed at Patishead along the point the micaceous sandstone or Pennant stone, which is always in this country an unerring index of coal.

The

The magnitude of the limestone strata is various; they seldom are less than three feet in thickness, and frequently they are observed to be six or seven feet thick. We sometimes see the beds of a black colour, from a deep impregnation of bituminous matter, in which case they exhale a fœtid smell when rubbed similar to that of sulphuretted hydrogen. It should be remarked, that the beds are sometimes very much traversed by contemporaneous veins of calc spar. It is not uncommon to find these veins of great thickness, they then approach to the character of true veins.

In some places we find a highly indurated sandstone of a grayish and oftentimes of a reddish colour and splintery fracture resting upon and dipping in a conformable direction with the limestone. About Clifton it is rather extensively distributed, forming several hills. Paragon Buildings are built upon it, and it is behind this row that the junction of the sandstone and limestone can very well be traced. York Place also stands upon it, from which spot it continues and forms the whole of Brandon Hill, and much of the space between Brandon Hill and Paragon Buildings is composed of this rock. We likewise trace it through Mr. Tyndal's Park, and on Kingsdown.

In all the above-mentioned places it reposes upon the mountain limestone, as is the case also at Week. It is likewise seen at the foot of Almonsbury Hill, in the cross road leading from the Gloucester to the Old Passage road, and in several places about Olverton. Besides the indurated sandstone, which perhaps according to the Wernerian nomenclature should be called a quartzy sandstone, and which, as far as I know, is only met with above the limestone, there are two other rocks which occur but partially, and lie below the limestone in a conformably inclined position—a siliceous pudding-stone and a red sandstone.

The relation which these rocks bear to the indurated sandstone and limestone is beautifully displayed by the section which some grand convulsion of nature has made of the rocks below Bristol. If we follow the path on the left side of the river Avon, where the stratification is best observed, we observe these formations occurring in regular succession. The first rock that comes in view after passing the ferry at Rownham, is the indurated sandstone, which at no great distance ceases, and is observed resting upon the limestone. The limestone then commences and continues uninterruptedly for about two miles. Opposite Cock's Folly it terminates, and then succeeds the pudding-stone, the matrix of which is a grayish-white sandstone cementing round pebbles of quartz. In some of the beds the gravel is so abundant that the matrix is hardly to be perceived, and in other strata it is so thinly distributed that the rock better deserves the name of a sandstone than pudding-stone. From the river side the pudding-stone can be traced crossing the road a little below Leigh. It is then for some distance lost on account of the cultivated state of the country, but in the hills near Pill and St. George's it is very distinctly seen, and can be followed beyond Portbury. In this route the pudding-stone is always seen dipping under the limestone agreeably to its inclination.

We now return to the stratification at the river's side. Beyond the pudding-stone which continues for about 200 yards, we arrive at the red sandstone of a fine friable texture, and containing spangles of mica. We can follow this rock north-westward from the river, and always see it sinking under the pudding-stone. It is exposed in many places in the space between Leigh and Portbury, and I have remarked this rock underneath the limestone at Mendip Hills in the path leading from Langford to Shipham.

I have been thus particular in describing these formations, as they absolutely exhibit a counter part of those that exist in the south of Herefordshire, along the skirts of Dean Forest. I had lately an opportunity of investigating the geology of the hilly country in the neighbourhood of Ross, and could not but be very much struck with the almost perfect identity of the series I have just now described with that near Ross. The lowest strata of which those hills were composed was the red sandstone I have just been dwelling upon. Then succeeded a siliceous pudding consisting of rounded pebbles of quartz, united by a sandstone cement, and above the pudding-stone lay the mountain limestone.

It is probable that the same stratification obtains in the country about Chepstow, for the mountain limestone is seen to prevail there, and I observed that Tintern Abbey is built of the red sandstone, which I was informed was quarried from the neighbourhood.

Having now fully described the limestone and the relation which the other inclined strata bear to it, I have next to speak of the country which the limestone ellipsis incloses. It is almost entirely an extensive coal formation, in which the coal either occurs in regular basins, or in veins following each other in quick succession, without being arranged in any determinate form. Mr. Townsend has described a very beautiful instance of the first kind of formation on the north-east side of Bristol, the outer range of which will be described by a line beginning at Iron Acton, and drawn through Yate, Wapley, Pucklechurch, Mangotsfield, Hambrock, and Frampton. This basin is about five miles in length and four in breadth, and in every part of its circumference I found the coal and its accompanying strata tending to a common centre. The Nailsea coal-field which is inclosed

inclosed by the two ridges of limestone, as described above, is, according to Mr. Townsend, another instance of the basin-shaped formation of coal. In other parts of the country the coal does not occur in any regular form.

Kingwood, from which Bristol has been so long supplied with coal, is a tract to the east of Bristol, about four miles broad. In this space the coal beds are very numerous, and follow each other in very quick succession. I have been informed by several old colliers, that in this district there are upwards of thirty veins of coal. A gentleman of my acquaintance told me, that his estate at Kingwood, in breadth about 1100 yards, contains twelve veins of coal, four of which are six feet in thickness. The beds in general are not of any considerable thickness, and are always inclined seldom less than 26° or 30°, and the prevailing dip is to the south, as is also the case with the veins worked at Bedminster. It sometimes happens that the veins are much impaired in quality, and sometimes completely intercepted by a loose kind of rubble, but faults of this kind are seldom of any considerable extent. The most remarkable instance in this district of a fault, that I am aware of, is in some collieries belonging to the Duke of Beaufort, about two miles on the Pucklechurch road, where the coal is thrown into a saddle-shaped form. On one side of the saddle a shaft has been sunk which meets with the coal at the depth of 147 fathoms. The strata that accompany the coal consist of micaceous sandstone (Pennant stone), indurated clay, here called duns, and bituminous shale. The micaceous sandstone is met with most extensively, and occurs in beds of great thickness. I know of pits that are sunk upwards of 20 fathoms through it. It is very well exposed in many quarries about Bristol, and is seen to great advantage along the bank of the river at Crew's Hole and Hanham, where it forms high and beautiful cliffs. I shall omit mentioning any particulars respecting the collieries at Stowey and Farrington near the Mendips, as they have been fully described by Mr. Townsend, and by a Mr. Williams, in the xxxth volume of the Philosophical Transactions for 1719.

Having now described the inclined strata, I have next to take notice of a class of rocks which is always seen lying horizontally upon the tops of the elevated strata.

In Farey's Derbyshire and Mr. Townsend's work above alluded to, this series, which is distributed over great part of the south of England, is fully detailed. I shall mention generally the places where in this neighbourhood the different members of this class occur, and the relation they bear to each other. The lowest beds of this arrangement and those which rest immediately upon the inclined rocks are described by Townsend and

 others

others under the name of *red ground*, because they are always covered by red soil, and many of the beds are deeply impregnated with that colour. The rocks of the red ground are represented by Farey as being numerous; in this quarter, however, they may very well be reduced to two—a limestone breccia and a calcareous sandstone. The breccia consists of angular fragments of limestone, hornstone, &c. cemented by a calcareous basis. Wherever it occurs, it is always seen resting immediately upon the inclined formations. It is sometimes wanting, and instead of it we find the calcareous sandstone, which is very well seen at Stapleton, where it rests immediately upon the Pennant stone; it has also been cut through in making the new course of the river. We sometimes meet with the breccia and calcareous sandstone together, as on Clifton Down near the turnpike, and among the Mendip Hills in the path leading from Langford to Shipham. Of the two formations the breccia is always the lowest; the best examples of this bed are about Pill and Portbury, where it is superimposed upon the red sandstone.

Above the beds of the red ground, we have what is provincially termed *lyas*, which is a limestone sometimes of a grayish, but more commonly of a blueish hue and dull compact fracture. It is a peculiarity in this rock to occur in strata of about a foot in thickness, which have generally interposing seams of clay or marl. The lyas has extensive localities in this quarter. It is quarried in many places in the tract between Bristol and Dundry, on the road to Bath, and in many parts of Kingwood; we see it abundantly on the Gloucester road. About Wells it rests upon the red ground, and at Aust Cliff it is situated upon claystone, probably one of the upper beds of the red ground. I could readily multiply these instances, but I believe these will be sufficient to show its general disposition in this quarter.

The next formation is the inferior *oölite*, a variety of the Bath freestone. I have no where viewed this rock *in situ*, except at Dundry Hill, where we have a favourable example of it. In walking from the village of Bedminster to the top of Dundry Hill we can trace most beautifully the relation of the three formations just now described. About Bedminster and at the sides of the new river we have the calcareous sandstone; between Bedminster and the Hill the lyas every where comes into view, and even forms the lower part of the hill. On the top near the church and among the caves the inferior oölite is well displayed. The next member of the series in this district is the Bath freestone, or great oölite, which surrounds Bath in almost every direction. There are also large quarries of it about three miles beyond Sodbury, near the Cross Keys. Of the series which lie next in succession, I have no where seen any examples in the

country

country which has been the subject of my examination. I am informed, however, that the chalk which lies above the great oölite begins to appear a short distance beyond Bath.

I have now to treat of those minerals which are found partially or in veins in the several formations I have described above.

Hornstone or *Petrosilex*. This mineral I have found in distinct beds on the top of a hill above Ashton, where it rests upon the indurated sandstone, and is of a red colour from a deep impregnation of iron, and possesses an uneven fracture. At Sodbury this rock is seen upon entering the town, where it lies in distinct strata upon limestone.

Quartz crystals of great beauty and rich variety of colour occur in veins of clay ironstone traversing the limestone. The crystal is commonly a six-sided pyramid, which is very frequently double.

Celestine or *sulphat of strontites* is contained in veins in the claystone at Aust Passage, and it is from hence that the finest and best crystallized specimens are obtained. It has been found in other situations massive and radiated.

Hæmatite. Good specimens are obtained from veins in limestone and Pennant stone.

Gypsum, both massive and fibrous and in the state of selenite, occurs at Aust Cliff, in thin seams in the red claystone lying below the lyas.

Sulphate of barytes. I found crystallized specimens of this fossil in masses of limestone that had been quarried near Harptree.

Calamine and Galena. These minerals have been found in thin seams in the limestone of Durdham Down, and at Clevedon. The brass works at Bristol are chiefly supplied with calamine from Shipham, a village among the Mendip Hills, where it occurs in beds beneath the limestone breccia.

Manganese has been mentioned by Townsend, as being procured from the Mendips.

Compact felspar. Upon visiting some time ago the Druidical stones at Staunton Drew, I was very much surprised to find them composed of rocks which I was perfectly unaware had any existence in this vicinity—a siliceous breccia and compact felspar. In returning a short time since from an excursion among the Mendip Hills, I passed through the village of Harptree, and I was very much struck by observing about half a mile on this side Harptree, masses of compact felspar exactly resembling the blocks at Staunton Drew, which had been brought together for the purpose of repairing the highway. All the information I could learn respecting them, was, that they were taken from the adjoining fields, where I saw large slabs of them lying upon the surface, having been turned up by the plough. The cultivated state of the country prevented further examination: otherwise I have little doubt that the compact felspar would be found *in situ*, for the masses here were in nothing different as to composition from the blocks at Staunton Drew.

Organic Remains. After the very elaborate manner in which Mr. Townsend has treated this subject, I should only be repeating his observations were I to attempt to add any thing. There is, however, one curious fact which I believe, he has failed to mention. It is, that although we find the fossilized remains of the *encrinus* so extensively distributed in the mountain limestone, and a prodigious abundance of shells chiefly *anomiæ* and *pectens*; yet in the indurated sandstone lying upon, and the other formations below the limestone, this sort of petrifactions, as far as I can observe, does not appear. I had once or twice seen in cabinets at Bristol, specimens of a fine-grained friable red sandstone abounding in delicate impressions of a spinous anomia, and I was quite at a loss to know where this substance occurred, till I found it under York Place, apparently filling fissures in the indurated sandstone. A friend of mine the other day sent me a specimen of indurated sandstone containing the impression of the bark of a pine, which is the only instance of that kind of stone (as far as I know) containing in its substance any thing like an organic remain.

XL. *On the variable Action of the Electric Column.* *By Mr.* J. A. Deluc.

To Mr. Tilloch.

Sir,—Your number for June last contains a paper of Mr. Ronalds's, which could not but interest me, as it relates to *the variable action of the electric column*. This variableness depends on many causes, with respect to one of which we do not agree, and it will be the object of this paper.

I have found in my experiments, that an increase of *moisture* did increase the *action* of the *column*. Mr. Ronalds is of a different opinion; but as he describes the experiments from which he concludes *that moisture has a very little effect on that action*, it will be easy for me to show that this disagreement between us results from a mistake on his part, and that his experiments, though very ingenious, according to his idea of the subject, are quite dissimilar to those whence I had derived my conclusion.

One

One single circumstance will show that Mr. Ronalds did not understand my meaning. I could not refer but to the *column* which I had described, of which an essential circumstance is, that it is in communication with the *outside air*, and thus influenced by its *degree of moisture;* whereas Mr. Ronalds's experiments have been made with one of Mr. Singer's *columns* of 1000 groupes *inclosed in a glass tube:* that *tube* therefore precluding the action of the *external air*, the inclosed *column* cannot be affected by its changing *degree of moisture*. I do not doubt that, when Mr. Ronalds shall consider that difference between our experiments, he will judge that the results which he relates in his paper, though interesting in themselves, cannot be opposed to mine: but I must come to particulars, because they will contribute to a further explanation of this new physical apparatus.

The action of Mr. Ronalds's *column* was indicated by the number, in a given time, of the *strikings* of the gold-leaves in an electrometer: in his second experiment made under a glass receiver, the *strikings* having been five in the beginning, while the *hygrometer* was 41°, and the *thermometer* $53\frac{1}{2}$, the *strikings* were reduced to three, when the *hygrometer* ascended to $41\frac{1}{2}$, and the *thermometer* descended to 53°: which experiment appears to favour Mr. Ronalds's idea, that the increase of *moisture* rather lessens than increases the action of the *column*. But in this experiment the *column* being inclosed in the glass tube, the increase of *moisture* took place on the surface of the latter, which produced an increase of *conductive* faculty for the *electric fluid* on that surface, which lessening the difference of electric state of its extremities, diminished consequently its effect on the electrometer.

The third experiment, in which the room was gradually *heated*, shows a case in which I had partly made a mistake, pointed out to me by Mr. Singer. In one of my experiments, when the *sun-rays* fell on my *column*, the number of *strikings* sensibly increased, and returned gradually to their former number when the *sun-rays* ceased to fall on it: this effect I attributed entirely to the action of the *sun-rays* themselves; but Mr. Singer conceived that it was only the effect of *heat* thus increased in the *column*. I considered this explanation as deserving to be submitted to direct experiments; I have since long observed the *column* under this point of view, and found that the increase of *heat* had a great influence to increase the *number* of *strikings*. But this is not an effect so simple as I thought it at first; for, with the same increase of *heat*, I have found great differences in the increase of the correspondent *number* of strikings; which circum-

circumstance I continue to think must be owing to the difference in the *electrical state* of the *ambient air;* and thus the *column* is, as I had found it in my first observations, an *aërial electroscope,* which property Mr. Ronalds had also surmised. But too many causes interfere with that state of the *ambient air,* to distinguish that effect with certainty; and it will require a longer study of the variations of the effects by different circumstances, in order to assign the true influence of each of them.

Mr. Ronalds's experiment 7 is a confirmation of those which I had described in a paper published in the Philosophical Transactions of the Royal Society for 1791. These experiments were made in order to show the cause of a *dissentiment* between M. de Saussure and myself on the point of *extreme moisture,* which I had prescribed to be taken in *water* itself; the reason of which I stated in my first paper on the *Hygrometer,* published in the Philosophical Transactions for 1774. The construction of M. de Saussure's hygrometer did not permit to dip it in *water,* because the index was in the lower part of the frame, and the *hair* of which it is formed, could not be dipped in *water* without the whole instrument, its index and scale, being immersed into water: he therefore fixed its point of *extreme moisture* under a receiver inverted on a dish full of water.

Unluckily this circumstance did not permit M. de Saussure to discover the defect of the *hair* and of all the *threads* of *fibrous* substances, which is to relent successively their *lengthening* when moisture increases, and even at last to grow *shorter,* more or less according to their nature, when *moisture* approaches to its *extreme;* which circumstance excludes all the *threads* from a true hygrometer. But my hygrometer consisting of a *slip* of *whalebone* cut *across the fibres,* is not subject to that defect; it lengthens up to the point of *extreme moisture* taken *in water,* and with it I made the same experiment under the receiver inverted on water; and thus I found the important law of hygrology, that the utmost quantity of evaporated water under such a receiver does not produce *extreme moisture* in the inclosed space, when the degree of *heat* is sensibly above 32; and that it recedes from it in proportion as the *heat* increases. Mr. Ronalds has found the same effect in his experiment 7; for, as long as the temperature was 55, which lasted a long while, the hygrometer never attained a point higher than 93.

In the same experiment under the receiver inverted over water, M. Ronalds found the *number* of *strikings* gradually to decrease, from 4·5 in a minute even to *no striking.* I am not surprised at this effect, as it proceeds from the cause which I have already indicated; for the *glass tube* in which the *column* was inclosed being

being more and more covered with *moisture*, its surface was at last become such a *conductor* of the *electric fluid* from the *positive* to the *negative* end, that it became *neutral*.

The last experiments of Mr. Ronalds would have surely reconciled our opinions on this subject, had they been made on purpose for its investigation; being made with a *column* similar to mine, composed of 800 groupes, supported between three glass pillars covered with sealing-wax; but these experiments were made for a different purpose. He placed an hygrometer, a thermometer, and an electrometer under a receiver inverted over *mercury*, in order to introduce successively an acid and an alkali, and to observe the degree of dryness they would produce: with the acid, the hygrometer descended from 46½ to 29½ by the temperature 59½; having then removed the acid and substituted potash, in the course of a day, it brought the hygrometer to 24.

These experiments, and the following, in which Mr. Ronalds made *moisture* increase under the receiver by introducing in it a moistened card, were made with the view of trying the effect of more or less *moisture* for increasing the action of the column, shown by the number of strikings of the gold-leaf in the electrometer in a given time, compared with the effects of the changes of *heat*: they certainly prove that the increase of *heat* accelerates the *strikings*, as Mr. Singer had supposed; but they show at the same time, what small quantity of evaporated water in a given space, produces in it *extreme moisture*, and that the increase of *heat* with the same quantity of *water*, tends to diminish *moisture*.

This I know by my own experiments, which are the object of my paper in volume xxxiii. of Mr. Nicholson's Philosophical Journal, giving an account of two series of experiments, agreeing with each other, by which I determine the number of *grains of water* which can remain in the state of *vapour* in the space of one cubic foot, by each degree of my hygrometer and of the thermometer. It may be seen in the table of the results, that *seven grains* of water evaporated in *one cubic foot*, by the temperature of 60, brought the hygrometer to 96·6 degrees, and that then the smallest diminution of *heat* caused a deposit of water on the sides of the vessel. This shows what small quantities of *evaporated water* act on the hygrometer in all its degrees; an important circumstance to be attended to in *meteorological systems*; a subject to which I shall return.

The above experiments made with the greatest care in a glass vessel, which was air-tight, show that such experiments made *under a receiver* are more difficult than is commonly imagined, and

and that they cannot be opposed to my experiments made in open air, which demonstrate that the *action* of the *column* is increased by the increase of *moisture*, and diminished by *dryness*. But a more direct proof of it is related in the same Philosophical Journal of Mr. Nicholson, for August 1810, by some experiments which I have made with my late very ingenious friend Dr. Lind, for ascertaining immediately the influence of *moisture* on the *action* of the *column*.

In this experiment all the parts of a *column* were first separately laid on the hearth of a chimney before a great fire, so that the pieces of *paper* were almost singed. In that state we mounted the *column*, and it did not affect the gold-leaf electrometer. We then dismounted again that column, and laid also the separate pieces on a table in my room, in which the hygrometer was above 40°. When they had thus remained one hour, we remounted the column, and it acted on the gold-leaves as it did before the *papers* had been so thoroughly *dried* by a great heat.

This I think to be a direct fact proving that a certain degree of *moisture* in the *column* is indispensable for its *conductive faculty*, the source of all its effects. I have found in all my observations, that within certain limits an increase of *moisture* increases the action of the column; but, whereas the effect of the increase of *heat* is immediately perceived, as it easily penetrates the column, that of the increase of *moisture* is very slow, because, beginning at the edge of the *papers* on the outside, it very slowly propagates in the internal parts of the column; a circumstance of which Mr. Ronalds has not been aware, especially by his *column* being inclosed in a *glass tube*.

But the *column* being a very new apparatus, it requires some time and a greater number of observers to follow all the views it opens in the terrestrial phænomena, especially in those of the atmosphere which constitute meteorology. This was my general conclusion in a paper published in volume xxxiii. of Mr. Nicholson's Philosophical Journal, under this title: "On Hygrology and Hygrometry, and their Connexion with the Phænomena observed in the Atmosphere." There I demonstrated that important fact, not only in meteorology, but in natural philosophy, that *rain* does not proceed from a quantity of *aqueous vapour* or *moisture* existing at any time in any portion of the atmosphere; that it must proceed from the *decomposition* of the *atmospheric air* itself, from which *decomposition* more or less complete result all the meteors, lightnings, thunder, hail, and other atmospherie appearances.

I have been thus particular in the examination of Mr. Ronalds's opinions, as he has shown much ingenuity in his experiments, and

and because, having had the opportunity of being personally acquainted with him, I agree with the judgement of Mr. Singer, in the opening of the same paper, that Mr. Ronalds "is an electrician of great promise, and whose scrupulous attention to the essentials of accurate experimental inquiry has afforded me pleasure to observe."

I am, sir,

Your most obedient servant,

Windsor, September 1814. J. A. Deluc.

XXXVI. *Observations on the Fall of Stones from the Clouds, or Aërolites.* By M. Marcel de Serres.

[Concluded from p. 224.]

The ignited meteors improperly called *falling stars*, do not appear to differ from the globes just mentioned. Thus, these meteors leave behind them gelatinous masses falsely attributed to birds of prey, since they contain nothing which announces an animal origin. To conclude: if igneous globes like falling stars do not always leave similar residues, this is owing to their being composed of entirely combustible bodies, and their being consequently entirely consumed before they reach the ground. We may refer to this kind of phænomenon the globe of fire which according to Geoffroy burst in the Place du Quesnoy, on the 4th of January 1717, that which was observed in America in 1800, and in the county of Suffolk in 1802.

To these globes of fire must certainly be referred the showers of fire which can only be distinguished from them by their greater division, whereas in the fire balls the same substance is concentrated in one and the same body. A similar shower made great ravages in Germany in the year 823, and burnt up whole villages. Another shower of the same kind fell in 1571, in the Grand Duchy of Hesse: after a dreadful explosion, it flowed through the streets, without however causing the destruction of the houses. A third shower of fire took place in 1678 at Sachsen-Hausen, and the inflamed matter burnt half an hour in the streets before it was extinguished. Finally, that which fell over the city of Brunswick in 1721 was so violent that they attempted in vain to extinguish it by means of water.

It would appear therefore that the difference remarked between showers of fire and those of an oily substance which have been seen a great many times, is founded on the circumstance of the substance of the former being in a state of phosphorescence, which is not the case in the latter. After these singular showers come those whose nature is mucilaginous, and which

according

according to Muschenbroeck, fell in Ireland in 1695. As chemistry shows that mucilage closely approaches to the nature of sugar and honey, we ought to refer to these same phænomena the *honey dews*, as they are called, which it is very difficult to acknowledge as an excretion of plants, as some have asserted. Silberschlag collected on paper the matter which was left by one of these falls of dew, and he found it to be a thick and viscous liquid. One of these showers of dew took place at Ulm so recently as 1802, and in so much abundance that every thing exposed to it, as well the surface of stagnant waters and fountains, was covered with a coating of it.

It may perhaps be presumed that the matter which produces globes of fire falls in the form of rain on some occasions, and in the same way that the aërolites themselves are precipitated in very minute divisions, in showers of sulphur, sand, and those falsely called showers of blood *.

As to showers of fire and brimstone †, a vegetable origin has been assigned to them, although facts do not warrant this explanation. Thus the shower of brimstone which fell at Copenhagen in 1646, fell at the same time with a strong rain, while the air was infected with a smell of sulphur, and the sulphur collected by Wormius and some other men of science had absolutely the same properties with that which is generally extracted from minerals. A shower of the same kind also took place at Copenhagen in 1665, and that after a very violent storm. The matter which it brought with it, when thrown into the fire, emitted a strong smell of sulphur, and with the spirit of turpentine it formed a kind of balsam of sulphur. Lastly, in 1801, the rain which fell at Rastadt was so sulphurous that it was used to prepare matches with. In general the substance mixed with these showers resembles much more the balsam of sulphur than sulphur itself: this was observed at Chatillon sur Seine, where the rain left a very fetid, thick, and viscous residue ‡, and finally in Ireland in 1695, where the matter deposited presented a deep yellow colour, with a disagreeable smell and a gluey consistence §. This matter had moreover the property of being deliquescent in the air, and of drying by the action of caloric. Similar showers have also fallen in the duchy of Mansfield in 1658, and at Brunswick in 1721.

It is equally absurd to consider the mineral showers as of animal

* See book ii. chap. 56, of Pliny's History of the World. Memoirs of the Academy of Inscriptions for 1717. Lemain's Antiquities of Orleans.

† Moses, Spangenberg, Olaus Wormius, Siegesbeck, and after them Muschenbroeck, have spoken of these showers of sulphur. See tome ii. of Muschenbroeck's Elements of Physics.

‡ *Hist. Nat. de l'Air*, by Richard, tome v.

§ Muschenbroeck and Izarn.

animal origin, because some of them have presented something like the excretions of butterflies. Thus we can scarcely doubt of the red mineral showers which fell in Westphalia in 1543 *, at Lowen in 1560, and at Embden in 1571. The latter was so extensive, that all the bodies exposed to the air were dyed red to the circumference of ten or twelve leagues. It is also stated that in the year 1653, in Zealand, a similar rain stained every thing red, and finally at Brussells in 1646 a violent rain suddenly fell, when all the waters were died red †. This rain had at first a purple colour, which gradually became yellow; its taste was sowerish, nearly like that of Spa water, which seems to indicate the presence of carbonate of iron, a substance which doubtless forms the essential part of it. This rain, as well as that which fell at Ulm in 1755, was chemically examined ‡.

About the same time, viz. the end of 1755, similar showers fell in Russia, Suabia, near the Lake of Constance and at Lucarno in Upper Italy. The sky was darkened during this rain at Lucarno; the atmosphere became quite red before it came on, and the residue left by it was reddish with an earthy appearance. This rain was almost as thick and heavy as snow, as well as that which fell in the mountains of Placentia on the 17th of January 1810. This last, as observed by a great number of persons, seemed white at first, then became red after some claps of thunder, and finally became white again. In certain places it was a flesh colour, whereas in others it was of a very deep red; but it always preserved its colour after having been melted: this fact seems to prove that it is upon slender grounds that the colour of these showers has been ascribed to a cat's eye substance like mica, as some have asserted.

There are too many testimonies in favour of showers of sand having fallen to entitle us to deny them. One was observed at Bagdad in 930 §, and long before it fell the sky was darkened by

* *Uber Wunder-regen.* Ulm 1755. This work contains a detailed history of all the similar showers at that time known. We there also find analyses of these showers.

† This water when collected had a sour taste nearly like that of Spa water. A little vinegar poured into it produced a thick red precipitate. When kept some time in well stopped bottles, the water became turbid of itself, and there was precipitated a viscous matter of a purple colour with some whitish streaks. On distilling this water we obtained a liquor of a sour and bitter taste. The taste and smell of the residue were like those of turf, which seemed to indicate the existence of organic matter. This rain lasted eight hours: the red colour was very deep when it began to fall, but afterwards it became clearer.

‡ This rain exhibited results analogous to those of the rain which fell at Brussells in 1646. The taste of the water was always sourish. The dark or black residue was partly attractable by the magnet, which proves the existence of iron. Vide *Uber-Wunder-regen*, Ulm 1755.

§ Quatremere, *Mémoires sur l'Egype.*

a red cloud, from which an immense quantity of reddish sand was precipitated, completely different from the sands which exist in that country. Some authors have regarded this sand as a ferruginous oxide. Whatever may be the case, the reality of this phænomenon is equally constant with that of the ferruginous rain observed in the Atlantic Sea in 1719* at 45° lat. and 32° long. at a distance of five or six leagues from the main land. This shower, which was preceded by a very strong light, lasted upwards of nine hours, without the air being disturbed†.

The different kinds of residues or of precipitates mentioned, are new proofs of the atmospheric origin of aërolites. In fact, it is impossible, and perhaps even absurd, to ascribe all those showers and globes of fire to the volcanic eruptions of the moon, or to portions of planets. If therefore we are obliged to assign to them an atmospheric origin, we can scarcely refrain from doing the same with respect to aërolites; since these meteors, whatever may be their name, pass so insensibly from the one to the other, and resemble each other so closely that the origin ascribed to the former of these phænomena cannot be withheld from the latter.

In short, the difficulties of which we have given a succinct account had been long felt; and if an atmospheric origin has not been universally assigned to aërolites as the most probable, it is because some very specious objections occurred to this opinion. We must confess that the formation and fall of aërolites is a phænomenon so singular, and so different from all those whose origin we can trace, that we are always more likely to attain the truth by attacking a theory which endeavours to account for it, than by defending that which seems to have most probability.

The strongest objection which has been brought against the atmospheric origin of aërolites, that which rests on their compactness and gravity, shows how difficult it is to conceive the formation in the atmosphere of bodies so heavy and large. How is it possible, it may be asked, that particles much heavier than the air can rise into the higher regions where meteorolites appear, and that metallic particles thus vaporized should remain suspended in the atmosphere until they have assumed the form of balls, or a mass of a certain volume? We may however remark on this head, that the particles which compose the balls of fire, and which no person has ever said came from the moon,

* *Hist. Nat. de l'Air*, by Richard, tome v. *Lithologie Atmospherique* by Izarn.

† Pere Feuillée exhibited specimens of this sand to the Academy of Sciences. As it was similar to that of an adjoining river, it was supposed to have been carried up by a water spout.

must

must have been also vaporized to be afterwards concentrated into masses of gelatine, and sometimes of a very considerable size. We may particularly cite as a proof the gelatinous mass which fell near Groepzig in Saxony, the extent of which was upwards of five feet and as large as the human body; for it is not less difficult to admit that these balls may be formed in the air than the aërolites themselves. If we endeavour to combine all these facts, we shall be inclined to think that a kind of formation of metals takes place in the air, in the same way as we see plants and organic bodies in general have the faculty of changing the nature of the substances which they absorb*. It is well known that hydrogen gas† can at a certain temperature volatilize some metals; but it is scarcely possible that this gas can give the form of vapours to the metallic particles of aërolites, since it is found in the atmosphere in an unappreciable quantity only. Besides, hydrogen will never vaporize the nickel, the iron, or the various earthy substances which enter into the composition of those stones. Nor can heat give the form of vapours to those substances; for in that case it would be very difficult to conceive their composition, which is in general nearly identical. Finally, how could vapours which are raised into the air so arrange themselves as to form such a compound mixture? We may add, that the regular form of the metallic particles seems to indicate a kind of fusion, which does not agree with their feeble degree of oxidation, as M. Proust has remarked.

Admitting that during the ignition of the metallic nucleus all these metallic parts are formed, we can then imagine the small extent of the nucleus in comparison with the immense size of the ball of fire from which it comes. In fact, the metallic nucleus is nothing else but the residue, or as it were the *caput mortuum* remaining after the great burning, and the gluey mat-

* We may see the experiments which have been made on the formation of metals and earths, in plants cultivated in sulphur and charcoal, and twice watered with distilled water: 1st, In the Memoir by Schrader, which gained the prize from the Berlin Academy, and an extract from it in the second volume of Gehlen's Chemical Journal. 2d, In the Memoir by Crell, entitled *Pericula Genesin Carbonis puri, quem vocant, &c. in Plantis vegetantibus investigantia.* This memoir was read to the Society of Gottingen, and an extract of it was published in the *Journal de Médecine* of Salzburg, 8th April 1811. The experiments of Crell and Schrader have been made with the most scrupulous precision.

† Arsenic unites with gaseous hydrogen, as first observed by Scheele: Potassium is dissolved in hydrogen—a current of hydrogen or azotic gas determines the volatilization of sodium. Ritter has even advanced that a great number of metals may be combined with hydrogen gas by the action of a strong Galvanic pile; but this is not proved.—Gilbert's Annals. We also read with advantage the observations of Corradori as printed in Brugnatelli's Journal.

ter like pitch, observed around certain aërolites, must be considered as parts which have not been inflamed. If the substances of which the balls of fire are composed, which precede the fall of aërolites, were not very combustible, these balls would not be of great extent, nor would the ignition be of long duration. The meteorolites themselves do not contain any very inflammable substances, since they have been burnt before their fall. We also add, that the phosphate of iron sometimes covers aërolites, as in the case of that which fell in Russia in 1807. It must also be observed, that nothing is wanting in the slimy ores of iron, but nickel, to resemble in point of composition atmospheric stones, as M. Vauquelin has remarked*. As these ores are almost always formed in the midst of marshes, we may in some measure thereby see the slow formation of aërolites. In fact, what we have already said, shows wherefore in several cases cloudy weather precedes the fall of the stones; for the cloud contains all the substances of which they are formed, and in this sense the phænomenon may to a certain extent be compared with the solution of salts.

It seems, therefore, that in considering the force which sustains the balls of fire as resulting from the inflammation or formation of vapours, we do not admit a rash hypothesis. In fact, in all cases where these balls burn with little violence, the matter of which they are composed soon falls, while it reascends the instant the inflammation becomes brisker. This is evident, from the effects of a *fusée*, in which the power of ignition raises and even supports a great weight. This power also exercises its action on aërolites; and as it is opposed to gravity, it obliges the stone to follow a mean direction between the two impulses which are impressed upon it. Observations also show that aërolites fall in proportion as the fire dies away, and that when, as at Connecticut, the burning increases and explosions take place, the stone ascends and rises.

From these facts we may conceive why all these phænomena are frequent in the hottest months and so rare in winter; and wherefore they appear in the evening frequently accompanied by storms. The causes of these phænomena, in appearance so remote, have nevertheless some approximation: thus rain is nothing but the precipitation of the water which is continually rising into the air; and aërolites probably only depend on the precipitation of an infinity of substances which are incessantly evaporating, and the reaction of which upon each other may form new combinations. This hypothesis will not appear gratuitous, if we pay attention to the immense quantity of com-

* *Annales du Museum d'Hist. Nat.* tome viii. p. 459.

pound

pound substances which organic bodies, stagnant waters, and all bodies while under decomposition, exhale without ceasing, and which are lost in the air without our knowing any thing as to what they become. It is therefore fair to inquire what are the methods resorted to by nature to counterbalance this perpetual evaporation, and to purify the atmosphere from all these volatilized substances. In fact, it is probable that nature employs other methods of purifying the air, and probably the organic bodies are the most powerful to which she resorts. The plants in particular seem intrusted with this office: it seems even that they absorb mucilaginous substances, which, as demonstrated by Messrs. Dupuytren, Thenard, and Moscati, exist in great abundance in the air. Vegetables feed upon it; and this cause, with several others, makes us conceive how it is possible that plants when put into substances incapable of furnishing their alimentary juices nevertheless grow and vegetate.

Such are the principal proofs, or rather the most constant facts, which render probable the hypothesis of aërolites being formed in our atmosphere. We may even go the length of saying that these proofs are strong enough to entitle the matter to be still further investigated: and we are far from thinking with M. Bigot de Morogues in his recent book, that it is rash to consider this supposition as probable. We confess with equal frankness, that the hypothesis of which we have given an account is liable to many and strong objections*; but is it clearly demonstrated, as M. Bigot de Morogues advances perhaps too gratuitously, that aërolites have once been small celestial bodies? If this opinion, which has been hazarded by the most eminent geometricians, was demonstrated, certainly it would be absurd to come forward with suppositions to explain a fact so well ascertained. We do not think, however, we have fallen into an absurdity of this kind; for in all the explanations hitherto given of the most singular phænomena, there never was the slightest evidence which follows a demonstration.

What we have said of M. Bigot de Morogues does not impugn the general usefulness of his work, which is besides the most recent we have on the subject of aërolites. We could have

* One of the strongest objections to the hypothesis of the formation of aërolites in the terrestrial regions is the total absence of oxygen in the stone which fell at Lissa, and which has been analysed by Klaproth. It is in fact singular, that the molecules of iron and martial pyrites should have resisted a short inflammation without beginning to oxidate. But in other aërolites, like that which fell at Alais, the charcoal which they contain burns instantly, and the silex obtained from it does not go into a jelly as in other meteorolites; which proves that they have not undergone a great degree of heat. This last fact is not very favourable to the idea of aërolites being darted from volcanoes in the moon.

 wished

wished, however, that the author had contrived to make us a little more acquainted with the opinions of foreign writers on the subject of aërolites, particularly the English and Germans. He seems to us to have hit upon a happy idea, in the division of his work into sections, which refer to the æra at which such and such opinions prevailed. Thus, he shows us how much down to the sixth æra the public opinion varied as to the reality of the phænomenon of aërolites, and how much in early times the marvellous accounts of the falls of stones were exaggerated by superstition, frequently becoming religious and accredited mysteries. When the sciences began once more to flourish, their followers were so much prejudiced against phænomena which seemed to them so much at variance with the laws of nature, that they disdain to pay any attention to them; whereas the historians of that time have recorded a fact of the kind, of which the Emperor Maximilian was an eye witness*. But at a period when every thing which could not be accounted for by reason passed for an invention of superstition, the learned sought to annihilate by specious reasoning, the reality of a fact which the authority of ages could not make them believe, because they could not conceive it. Nevertheless, in the midst of those disputes, which the great name of Gassendi † could not terminate, there fell a great quantity of stones at Lucé in 1768, in the very heart of France; and notwithstanding this fact, the evidence of which is indisputable, the Academy of Sciences persisted in regarding it as one of those popular prejudices which were unworthy the notice of men of science. New falls of stones which took place in India attracted the attention of the learned, but still without triumphing over prejudice; and it required no less than the great quantity of aërolites which fell at l'Aigle, and at the very gates of Paris, to convince every one finally of the reality of this singular phænomenon. From the above time (1803) observations have been so much multiplied, that there is no fact at present better supported. This phænomenon is even so common, and so frequent, that on seeing it recur at periods so near each other, we are still more inclined to regard them as formed in our atmosphere. However the case may be, it is easy to perceive, from the rapid sketch which we have drawn, that the history of aërolites is connected with that of our errors and our prejudices, and that it is even interwoven with the history of the world.

* On the 7th of November 1492, near Ensisheim, an aërolite fell near this prince at the moment when at the head of his army he was about to give battle to the French army.

† Gassendi gave an account of the aërolite which fell on the 27th of November 1627 on Mount Vaisor in Provence.

XLII. On

XLII. *On the Affections of Light transmitted through crystallized Bodies.* By David Brewster, *LL.D. F.R.S. Edin. and F.S.A. Edin.* *In a Letter to Sir* Humphry Davy, *LL.D. F.R.S.**

Dear Sir,—In a former paper† on "Some Properties of Light," which I took the liberty of addressing to you, and which the Royal Society honoured with a place in their Transactions, I attempted to give a brief abstract of a set of experiments on the Properties of transparent Bodies in refracting, dispersing, and polarising the Rays of Light. An account of the instruments and methods employed in these experiments has since that time been published in my "Treatise on new philosophical Instruments."

From the general object of these researches, however, I have been allured into a new field of inquiry, by the discovery of a singular property of light transmitted through the agate, and the prosecution of the views which it suggested has led to some very extraordinary results, which, while they seem to conduct us into the very mysteries of physical optics, exhibit at the same time a series of appearances which far surpass, both in splendour and variety, all the phænomena of light under its usual transformations. In again soliciting you to communicate these observations to the Royal Society, I trust I need offer no apology. They are closely allied with that science which you have so widely extended by the most profound and brilliant discoveries; and it is probably from the cultivation of this department of physics, that philosophy will be enabled to unfold the secrets of double refraction, to explain the forms and structure of crystallized bodies, and to develop the nature and properties of that ethereal matter, which, while it enlivens all nature by its presence, performs also a capital part in the operations of the material world.

The different subjects of which I mean to treat in the following letter may be included under five heads.

I. On the polarizing power of the agate.

II. On the structure of the agate as connected with its optical properties.

III. On the peculiar colours exhibited by the agate.

IV. On the depolarization of light.

V. On the elliptical coloured rings produced by obliquely depolarizing crystals.

I. *On the polarising Power of the Agate.*

I have already shown, in a former paper, that a ray of light

* From the Philosophical Transactions for 1814, part i.

† Phil. Trans. for the year 1813, p. 101.—Phil. Mag. vol. xlii. p. 286.

transmitted through a plate of agate cut by planes perpendicular to the laminæ of which it is composed, suffers polarization like one of the pencils formed by double refraction. If the light thus polarized is incident at a particular angle upon any transparent body, so that the plane of reflection is perpendicular to the laminæ of the agate, it will experience a total refraction; if it is transmitted through another plate of agate, having its laminæ at right angles to those of the plate by which the light is polarised, it will suffer total reflection; and if it is examined by a prism of Iceland crystal turned round in the hand of the observer, it will vanish and reappear in every quadrant of its circular motion.

The pencil of rays to which this remarkable property is communicated is surrounded by a large mass of nebulous light, which extends about 7° 30′ in length, and 1° 7′ in breadth on each side of the bright image*. This nebulous light never vanished with the bright image which it inclosed, but was obviously affected with its different changes, increasing in magnitude as the bright image diminished, and diminishing as the bright image regained its lustre. From this circumstance I was led to conjecture "that the structure of the agate was in a state of approach to that particular kind of crystallization which affords double images, and that the nebulous light was an imperfect image arising from that imperfection of structure."

On the supposition that this conjecture was well founded, I imagined, in conformity with the general analogy of all doubly refracting crystals, that the bright image and the nebulous light were produced by two different refractive powers, and I expected to separate the one from the other by forming the agate into a prism with a considerable refracting angle. Every attempt of this kind, however, was fruitless; no perceptible separation of the images was effected by any of the prisms which I employed, and I was therefore obliged to abandon this mode of investigation.

Having procured a plate of agate remarkably thin and transparent, I admitted a beam of light from the sky into a dark room through a narrow rectangular aperture. When this aperture was viewed through the agate, it was surrounded with a very considerable nebulosity; and by interposing a prism of Iceland spar between the agate and the eye, and giving it a motion of rotation, the nebulous light became very dense when the bright image vanished, and almost completely disappeared when the

* On each side of the bright image I have observed a condensation of the nebulous light resembling two imperfect images of the luminous body. These imperfect images, which increase in number by inclining the agate, are slightly tinged with the prismatic colours, which evidently belong to that class of phænomena which have been so ably treated by Dr. Thomas Young, in his late work on Medical Literature.

the bright image had reached its greatest brilliancy. The bright and the nebulous images, therefore, comported themselves exactly like the two images formed by doubly refracting crystals; and the small portion of nebulous light, which surrounded the bright image at its maximum lustre, was obviously produced either by the imperfect polish of the agate, or by its not being cut exactly at right angles to the plane of its laminæ.

It will be seen from a subsequent section of this letter, that light polarized by the agate, or by any other means, is depolarized, or partly restored to its original state, by being transmitted in a particular direction through a plate of mica, or any other crystallized body. I therefore interposed a plate of mica between the agate and the Iceland spar when the nebulous light had nearly disappeared, and having adjusted it to the depolarizing position, the nebulous light was instantly revived round the bright image, while the other bright image which had disappeared resumed its place in the middle of the other nebulous mass.

When a pencil of light polarized and afterwards depolarized, in a manner to be afterwards described, is transmitted through a plate of agate, the *red** rays go to the formation of the bright image, while the *green* rays compose the nebulous light, so that we have a *red bright image* enveloped in a *cloud* of *green light*. By turning round the agate 90° the bright image is formed by the green rays, while the nebulous image consists of the red rays, so that we have a *green bright image* encircled by a mass of *nebulous red light*. If in the place of the agate we substitute a doubly refacting crystal, it will always be found that the ordinary image is green when the extraordinary one is red, and that they assume these colours alternately during the motion of the prism round the axis of vision.

From these experiments, we may consider it as demonstrated, that the nebulous light has the same relation to the bright image, as the first has to the second image of all crystals that have the property of double refraction †. It does not appear, however, that the nebulous image is produced by a greater refractive power than that by which the bright image is formed. There is on the contrary every reason to conclude, in opposition to the analogy of all doubly refracting ‡ crystals, that the agate

* The *red* and the *green* are complementary to each other. The same result is obtained if the *blue* and *yellow*, or any other two complementary colours are used.

† See Edinburgh Trans. vol. vii. part ii.

‡ It will be seen from a subsequent paper, that many other bodies both of mineral, animal, and vegetable origin, have the property of forming two images, polarized in an opposite manner, but not produced by two different refractive powers.

gives two images and polarizes them like other crystallized bodies, while the one image is placed exactly in the centre of the other.

II. *On the Structure of the Agate as connected with its optical Properties.*

When we examine a piece of transparent and well polished agate, we perceive a number of bands or stripes, which are the sections of a succession of laminæ that are sometimes parallel, but in general concentric. These laminæ are often of a milky white colour when seen by reflected light, and sometimes nearly as transparent and colourless as glass, and the white laminæ commonly alternate with the transparent ones. The laminæ which are white when seen by reflected light, are brown by transmitted light, and the intensity of this brown colour increases with the thickness of the plate of agate. The transparent laminæ exhibit three varieties of structure.

The *first variety*, which appears to be the coarsest, consists of a number of small serpentine lines like the figures 333333, lying parallel to each other, and closely resembling the surface of standing water when ruffled by a gentle breeze, or the sandy bottom of a slow moving stream. These serpentine lines are always arranged in a direction parallel to the laminæ, and are seen very distinctly even when the agate is so thin as the 150th part of an inch.

The *second* variety of structure differs from the first, only in the serpentine lines having a much smaller size; and the laminæ which have this structure appear the finest and most transparent.

The *third* variety has no serpentine lines, and does not appear to differ from other semi-transparent bodies. It admits the light more copiously in all directions than any of the other structures; and as it does not polarize it in a similar manner, we may consider it as possessing, in a different way, that kind of crystallization which polarizes the incident light by separating it into two pencils.

The white veins sometimes exhibit the first variety of structure; but in several specimens the veins appear to be fibrous in their structure, the fibres stretching at right angles to the laminæ through the whole of their thickness.

These different structures will be better understood from fig. 1 and 2 of Plate IV. Fig. 1, represents the specimen of agate with incurvated veins which I have noticed in a former paper*. It is composed of two veins, AB, CD, and three transparent portions AEB, ABDC, and CFD. The transparent portions ex-

* See Phil. Trans. 1813, part i.—Phil. Mag. vol. xlii.

hibit

hibit the *second* variety of structure, though the small serpentine lines are not so distinctly marked as in other specimens. The two veins AB, CD, are both white when seen by reflected light. The breadth of AB is one-tenth of an inch, and its radius of curvature $1\frac{1}{2}$ inch, and it consists of *four* smaller veins *mn*, *op*, *qr*, *wx*. The light reflected by *mn* is a paler white, and the light transmitted by it a lighter brown than in the other parts of the vein. The light which *op* reflects is of a brighter white, and that which it transmits of a deeper brown than in the other parts of the vein, and at the junction of *mn* and *op* there are several tufts of fibres of the same character as *op*. The other divisions of the vein *qr* and *wx* are of an intermediate character between *mn* and *op*. The vein CD resembles the division *mn*, and possesses, like AB, the fibrous structure already described. The thickness of the plate AEDC is one-fiftieth part of an inch; AC is three-tenths of an inch; and a line AC forms an angle of about 25° with a plane perpendicular to the laminæ.

Figure 2, represents another specimen of agate of a different character. It consists of transparent portions AB, BC, CD, DE, EF, separated from each other by white veins B*b*, C*c*, &c. and distinctly exhibiting the *second* variety of structure; and of other transparent portions FG, GH, HI, IK, KL, separated by similar veins G*g*, H*h*, &c. but exhibiting the *first* variety of structure. All the veins possess a structure approaching to that of the first variety, G*g*, K*k* exceeding the rest in the intensity of the light which they reflect and transmit.

If we measure the quantity of light transmitted through a plate of agate containing veins, it will be found to be a maximum when the direction of the incident rays is parallel to the interior surfaces of the veins. When the light, however, is transmitted through a part of the agate of an uniform transparency, and perfectly free from veins, the same result will be obtained, the intensity of the light being a maximum when its direction is parallel to that of the laminæ.

If AB, fig. 3, be a section of the specimen of agate represented in fig. 1, and *mn*, *op*, the direction of the laminæ inclined 25° to the surfaces of the plate, rays of light incident in the direction RS parallel to *mn* are more copiously transmitted than when they are incident in any other direction. When the pencil of light falls in the direction TV, its lustre suffers a great diminution: the light gradually assumes a red colour, and vanishes altogether when the obliquity is considerable. But if the pencil is incident at the same angle on the opposite side, as PQ, its lustre suffers very little diminution, and its colour is not sensibly altered.

These

These facts admit, to a certain extent, of an easy explanation, if we suppose that the plate of agate consists of laminæ *mn*, *op* imperfectly transparent, alternating with laminæ *cd*, *ef*, which are more pervious to light, a structure which is indicated by the existence of a bright and a nebulous image. In this case the intensity of the light will obviously be a maximum when the ray RS is parallel to *mn*, and a ray PQ will, within certain limits, suffer less diminution of lustre than a ray TV falling with the same angle of incidence on the other side of RS.

When RS and *mn* are perpendicular to the surfaces of AB, and when PQ and TV form equal angles with the perpendicular RS, their intensity should be equal; but this is by no means the case, for the transmitted light which is incident on the side T of RS appears to have a different character from that which is incident on the other side. We must therefore suppose that there is some other peculiarity of structure in the agate, connected probably with that particular kind of crystallization which polarizes light, to which this curious fact must be ascribed*.

The intensity of the light transmitted by the agate is likewise affected by its polarizing property. If a ray R*r*, Plate IV., fig. 4, is incident upon a piece of agate AB, so as to be polarized by reflection from the second surface, then, since it is polarized during its passage from *r* to E, the bright image will suffer total reflection at E, while the nebulous image will be transmitted like common light in the direction EG. If the agate is now turned round 90°, the nebulous image will suffer total reflection at E, while the bright image will penetrate the second surface at E like common light. When the incident ray R*r* has different obliquities and the agate intermediate positions, the intensity of the transmitted light will be more or less affected by its polarizing power.

The preceding observations on the laminated structure of the agate enable us to give a satisfactory explanation of some singular appearances exhibited by that mineral.

In the specimen shown in Plate IV. fig. 5, the black lines represent the veins, and consequently the direction of the laminæ, and the dotted lines *ab*, *ac*, *cm*, *ck*, &c. are drawn through the vertices of the angles made by the veins; and consequently by the laminæ whenever they change their direction. When light is transmitted through a piece of agate of this description, the planes A*acm*, *mck*, *ngf*, *nhg* have the appearance of being differently inclined to one another, and transmit different quantities of light. If the veins and the laminæ preserved the same inclination to the surface of the plate of agate when they changed

* See Edinburgh Transactions, vol. vii. part ii.

their

their direction at the lines *ac*, *cm*, *de*, the phænomenon which has been mentioned could not take place; but whenever the laminæ change their direction, their inclination to the surface likewise changes, and therefore the intensity of the transmitted light experiences a corresponding variation as the rays have to traverse different lengths of the imperfectly transparent laminæ.

When the veins and the laminæ are incurvated like those in the portion A*acm*, their inclination likewise changes; but as this change is gradual and not sudden, as in the former case, the intensity of the transmitted light suffers a gradual variation, and the portion A*acm* has the appearance of being concave. When the laminæ therefore are arranged in a circular form, they will resemble a number of dimples, the apparent concavity of which will in some cases depend on the curvature of the laminæ, and will exhibit the phænomenon of the *hammered agate.*

III. *On the peculiar Colours exhibited by the Agate.*

In my former letter on the polarizing power of the agate, I noticed the existence of a coloured image which appeared on each side of the common colourless image, and which was polarized in a similar manner. I have since observed the same phænomenon in other specimens; and though I have not been able to discover its cause, I trust the following observations will be of some service to future inquirers.

In the specimen represented in Plate IV., fig. 2, the colours appear only when the rays of light are transmitted through the veins B, C, D, E, F, G, H, I, K, or through the coarse grained portions F*g*, G*h*, H*i*, I*k*, K*k*L, and when these parts are covered no colour is perceived. If the eye, therefore, is placed behind any of the coarse grained parts, and close to the agate, a colourless image of a candle will be visible, and on each side of it a highly coloured image forming an angle of $10\frac{1}{2}°$ with the colourless image. The colours, which are extremely brilliant, are blue, green, yellow, and red, reckoning from the common image. A second image coloured in a similar manner, but considerably fainter, is distinctly seen, forming with the colourless image an angle of about 21°. When the agate is held some inches distant from the eye, the colours appear diffused over the surface of the coarse grained portions, and when the light is strong, the phænomenon is uncommonly brilliant. When the vein B*b* is a pale blue, at a certain distance from the eye, C*c* and D*d* are of the same colour, E*e* is greenish, F*f* is yellowish, FG*gf* is pale red, and the red colour is more intense towards L*k*. By a gentle motion of the agate the colours of these portions instantly change, a particular colour being always produced in the same

portions

portions at a particular angle of incidence. The veins Gg, Hh, Ii, and Kk are, however, green when the surrounding portions are red, and red when the surrounding portions are green; from which it follows that these veins produce a particular colour at a different angle of incidence from the adjacent portions. In another specimen of agate, very like the preceding, the same phænomena are distinctly visible, and the coloured image forms the same angle with the common image. In a third specimen, belonging to Robert Ferguson, Esq. of Raith, the colours are exhibited in the most splendid manner. A semi-transparent and irregularly elliptical zone, about six inches and a half in circumference and three-tenths of an inch broad, has the *first* variety of structure, and forms the coloured image at a distance of $13\frac{1}{2}^\circ$ from the common image.

In the specimen represented in Plate IV., fig. 1, the colours are visible only in the vein AB; but here the angle of the first coloured image with the common image is 28°, while that of the second image, which is very faint, is only a little greater. The other vein CD, which to all appearance has the same structure as AB, and which differs from it only in being a little thinner, exhibits no colours; but there is a small stripe *st* at its edge where the colours are very distinct. This circumstance induced me to think that the colours depended on the thickness of the plate, as well as upon its structure; but upon grinding a hollow place *mvw* in the vein AB, so as to make the agate remarkably thin, I found that it gave the same colours as before. A similar experiment was made with another piece of agate, and the result was the same, though the thickness of the plate could not exceed the 400dth part of an inch. The colours indeed were rendered more brilliant by the increased transparency of the agate, but in other respects they experienced no change. In another specimen, of which it is unnecessary to give a particular description, the coloured image formed an angle of about 34° with the colourless pencil, and the different veins produced the same colour at different angles of incidence.

In attempting to explain these appearances, I at first imagined that the colours arose from the polarization of the transmitted rays, and that they were analogous to the colours of plates of mica and topaz which I have described in another place. I found, however, from several experiments, that the coloured image is equally distinct in every position of the agate; that it is alike produced by polarized or depolarized light, and that it suffers no change either when examined by a plate of agate or by a doubly refracting crystal.

The phænomenon which we have described must therefore be be considered as a new case of the production of colour; and though we do not pretend to point out its cause, yet it obviously depends upon a particular structure which is posssessed only by some portions of the agate, and admits of such variations as to produce the same colours at different angles of incidence.

IV. *On the Depolarization of Light.*

In the fourth book of my Treatise on new Philosophical Instruments, I have already shown that almost all transparent crystals possess in two positions the singular faculty of depolarizing light, or of depriving it of the property which it acquires by transmission through the agate, while in other two positions of the depolarizing crystal, the polarity of the light suffers no change. Thus in Plate IV., fig. 5, let ABDC be a piece of mica or of any other crystallized body interposed between a plate of agate and a prism of Iceland spar when one of the images has vanished, and let GH be parallel or perpendicular to the laminæ of the agate when the vanished image continues invisible. This line I have called the *neutral axis*, as no effect is here produced upon the polarized light. By turning the mica round, the vanished image will gradually appear; and when the line AD comes into a vertical position, it will be restored to its full lustre, and will never again vanish, whatever be the position of the Iceland spar. The line AD I have therefore called the *depolarizing axis*, as the light in passing through it has been deprived of the polarity communicated by the agate, and which prevented it from penetrating the rhomboid of Iceland spar.

By continuing the motion of the mica, it will be found that EF is also a neutral axis, and BC a depolarizing axis. The depolarizing axes are common to almost all crystallized substances; and what is very singular, I have discovered them in horn, gum arabic, glue, tortoise-shell, caoutchouc, goldbeater's skin, amber, mother of pearl, camphor, spermaceti melted and cooled, bees' wax melted and cooled, adipocire melted and cooled, manna, oil of mace, acetate of lead melted and cooled, human hair, bristles of a sow, human cornea, cornea of a fish, cornea of a cow, and imperfectly in some pieces of plate glass.

Plates of mica, however, while they possess the properties of all depolarizing crystals, exhibit phænomena peculiar to themselves. If the neutral axis GH of a plate of mica is inclined forwards so as to make a considerable angle with the horizon, the image that was formerly invisible will start into existence, and therefore the neutral axis GH is accompanied with an oblique depolarizing axis N*n*. This oblique axis is also possessed by topaz, rock crystal, and many other crystallized bodies. In making the same experiment with the depolarizing axis of the

the mica, I observed the image to vanish in the direction M*m* and P*p*, which I considered as oblique neutral axes; but I have since found that this was owing to the polarization of the pencil by oblique transmission, a property of light which I had not then discovered.

We have hitherto considered the depolarization of light as effected by two separate bodies, one of which polarizes the incident rays, while the other deprives them of the polarity which they have thus acquired; but in all bodies that possess oblique depolarizing axes, light may be polarized and depolarized by the same crystal. Thus if AB*ab*, Plate IV., fig. 7, be a plate of topaz having DE for its oblique depolarizing axis, and if a ray RR′ of common light is incident at R′ with such an obliquity that it is polarized by being reflected at C from the posterior surface *ab*, then the ray *rr′* will be depolarized in its passage from C to *r* along the oblique axis of depolarization, and the emergent ray *rr′* will be depolarized light. Hence it follows that the angle DC*b*, which the oblique depolarizing axis makes with the posterior surface *ab*, is nearly equal to the complement of the angle OC*r*, at which light is polarized by reflection at C *.

[To be continued.]

XLIII. *New Outlines of Chemical Philosophy. By* Ez. Walker, *Esq. of Lynn, Norfolk.*

[Continued from p. 350, vol. xliii.]

The causes from whence arise all those various phænomena of our atmosphere, commonly called meteors, have never been investigated in a satisfactory manner. It is probable, however, that they are the effects of some universal cause. It is already known

* Since the preceding section was written, I have performed a very extensive series of experiments on the depolarization of light, and have thus been led to a satisfactory generalization of the phænomena. In this theory the phænomena are referred to the general principle of polarization: such bodies as have neutral and depolarizing axes are supposed to form two images polarized in an opposite manner, and either produced by the same or by different refractive powers; while those which depolarize light in every direction, like gum arabic, caoutchouc, &c. are composed of films or layers, each of which is a doubly polarizing crystal, the neutral and the depolarizing axes of one film not being coincident with the neutral and depolarizing axes of the rest. In a separate memoir, which I have drawn up for the consideration of the Royal Society, I have given a full account of this theory, of the experiments on which it is founded, and of the new views to which it leads respecting the formation and structure of organized matter.

known that lightning is an electrical phænomenon, and that electricity is the cause of many others; but how electricity should produce the ordinary winds, rain, hail, snow, clouds, vapours of all kinds, hurricanes, whirlwinds, waterspouts, and all the various changes of our atmosphere itself, with regard to its specific gravity, rarefaction, heat and moisture, as indicated by the barometer, thermometer, and hygrometer, is a philosophical question that cannot be solved, till the laws which obtain between matter and the elements of electricity be more fully investigated.

The electric spark is seldom seen in nature, except in a thunder-storm, or in the silent summer lightning. But those two elements which produce the spark, produce various other phænomena, that are daily taking place in our atmosphere. Nor does their influence stop here; for they are the springs that keep in motion the *living principle* of both animals and vegetables, and cause them to perform all their various functions. And as neither animal nor vegetable life can be brought into existence without heat, it follows, that even the living principle itself depends, in some measure, upon those elements which produce all electrical phænomena. Although these effects are not produced by the electric spark, yet they are the effects of the same causes, whose operations are rendered more mild, and less perceptible, by acting at a greater distance.

When an excited surface is brought near an insulated conductor, a spark passes between them; this is called communicated electricity: but when an excited surface is placed at a greater distance from the conductor, so as not to produce a spark, the effect is called electricity by position or induction.

The distinction that has been made between these two effects is, that communicated electricity is permanent, but inducted electricity ceases on removing the excited surface. But this hypothesis of inducted electricity is erroneous, although it has been adopted by Professor Robison, and other writers on electricity. The learned Professor recommends "several brass conductors, each supported on an insulated stalk and foot. They should be about an inch and a half, or two inches long, and about three-fourths of an inch in diameter, with rounded ends, and well polished to prevent all dissipation."—Ency. Brit. Sup. vol. i. p. 571.

But in a conductor of these dimensions, the two elements can be separated only to a short distance, and, consequently, will reunite the instant that the excited surface is removed. Hence it seems highly probable, that the Professor's erroneous conclusion arose from the imperfection of his instruments.

Indeed,

Indeed, the Professor himself seems to have had some doubts respecting the results of his experiments; for he observes, that "the reader who is at all conversant with electrical experiments, will be sensible that these experiments are *delicate*, requiring the greatest dryness of air, and every attention to prevent the dissipation of electricity during the performance. This, by changing the state of the conductors and electrometers, will frequently occasion irregularities. The electrometers are most apt to change in this respect, it being scarcely possible to make them perfectly smooth, and free from sharp angles. It may therefore happen, that when the conductors have affected them *for some time*, by the action of the disturbing electric, the removal of this electric will not cause the electrometers to hang perpendicular; they will often be attracted by the conductors, and often repelled*." Now it is evident, from these irregularities, that the Professor's experiments are very far from being conclusive.

Mr. Bonnet's electrometer has also been used to prove that electricity by induction is not permanent, but vanishes as soon as the exciting cause is removed. But this is still a more imperfect instrument for this purpose than the Professor's short conductors. For when an excited surface is brought near the cap of this instrument, without producing a spark, the element contained in it, of the same kind as that possessed by the excited surface, is repelled from it, through the gold-leaves and tinfoil, into the earth. And as soon as the exciting surface is removed, an equal portion of the same element, as that which was repelled from the cap, returns into it again from the earth, restores the equilibrium, and all electrical signs vanish, there being a free communication between the cap and the earth.

The conductor I use in making experiments on inducted electricity consists of a brass rod 12 inches in length, a quarter of an inch thick, with a brass ball one inch in diameter screwed upon each end. This is mounted upon a glass rod 14 inches in length and two-tenths of an inch in diameter. Now, as electricity is carried off from the surfaces of bodies by the air, or the conducting particles that float in that fluid, the less surface any body has, the more perfectly it will insulate: consequently a glass rod of two-tenths of an inch in diameter is 25 times more perfect than another rod of the same length of one inch in diameter; for as 4 is to 100, so is 1 to 25; the surfaces of cylinders of the same length being to one another, as the squares of their diameters.

In a former paper I gave an account of some experiments

* Ency. Brit. Sup. vol. ii. p. 573.

made

made with this instrument, from which it appears that electricity by induction is permanent, and does not vanish on removing the exciting cause*.

But to ascertain the truth of this principle with still greater precision, I made a great number of instruments (more than twenty) of a more perfect construction than that above described. Some of these remained electrified by induction for two or three hours; but others retained the electrical element which they had received by induction, for almost *three days*.

Exp. 1. To determine the sensibility of these instruments, I placed one of them close to an electrometer constructed according to Mr. Bennet's directions†; and after having slightly excited a barometer tube, I placed it at the distance of about two or three inches from the top of each. The indexes of mine rose to 180 degrees, being the extent of the scale, and remained permanently electrified; but no *permanent* effect was produced on Mr. Bennet's, nor even any *perceptible* effect, when the air was damp, and unfavourable for electrical experiments.

Exp. 2. Two of these electrometers, which were differently constructed in some respects, being placed upon a table, and an excited glass tube carried over them at the distance of a few inches, one of them received the same element as the excited tube, but the other received the contrary element, and they remained permanently electrified‡.

This seeming paradox may be explained thus: If a very small piece of metal be properly insulated, the quantity of electricity which it contains in its natural state, may be deemed almost nothing; and therefore it receives electricity from the excited surface, of the same kind as it would receive by direct communication. But when a brass ball of one or two inches in diameter is insulated, and an excited surface brought near it, the two elements, diffused over its surface, begin to be separated. The element of the same kind as that of the excited surface is repelled into the atmosphere, but the other element is detained upon the surface of the ball; and consequently, as soon as the excited surface is removed, the ball remains permanently electrified, and in a state contrary to that of the exciting cause.

Now, as the laws of nature are the same, whether their operations appear in our experiments or upon the grand theatre of the universe, the above experiments may, therefore, lead us to an explanation of some meteorological phænomena, which are inexplicable upon any other known principle.

Suppose a very dense cloud (A) be electrified either by its

* Phil. Mag. vol. xlii. p. 215. † Phil. Trans. vol. lxxvii.

‡ A further account of these electrometers will be given at some future opportunity.

proximity to the earth, or by some other means; and let this cloud, containing thermogen, be carried by the wind under another cloud (B) in its natural state, then this cloud will become electrified by induction; for the thermogen which it contains will be expelled from it into the air above; and when the cloud A is carried away by the wind, the cloud B will remain electrified, and in a state contrary to that of A, like the brass ball in the second experiment.

The air above the cloud B having received an additional charge of thermogen, and this element having a strong attraction for moisture*, will attract moisture from the surrounding air, and thus a new cloud will be formed.

When the cloud A is carried away from under the cloud B, let it be supposed that it passes over or under a very thin cloud, then will this cloud be electrified by induction; but it will receive the same element as that of the cloud A: the reason of which is shown by the second experiment.

As an excited barometer tube will produce an electrical effect at the distance of four or five feet, it may be supposed that a cloud highly electrified may affect other clouds at the distance of some miles. And hence we may infer that all those various changes, which are daily taking place among the clouds, may arise from that unerring law of nature called inducted electricity†.

Lynn, Oct. 10, 1814. EZ. WALKER.

[To be continued.]

XLIV. *Memoir upon the compound and smooth or simple Eyes of Insects, and on the Manner in which these two Species of Eyes concur in Vision.* By M. MARCEL DE SERRES, *Professor of the Sciences in the Imperial University.*

[Continued from page 191.]

III. *On the Mechanism of Vision.*

THE eyes of insects, according to the foregoing observations, are formed in a very different manner from those of the vertebral animals, or even the *molluscæ*, among which there also exist distinct humours. Nothing similar is seen in the eyes of insects which, deprived of every humour, appear as it were to feel the light almost immediately. Although formed in a manner so peculiar, these organs are nevertheless eyes, as we may be

* Phil. Mag. vol. xliii. p. 252.

† Dr. Franklin discovered that the clouds would change from positive to negative electricity, several times in the course of one thunder-gust.

convinced

convinced by squeezing them, or rather by rubbing off from them a black stratum which might intercept the action of the rays of light. The insects subjected to this test act completely as if blind, and can no longer direct their efforts rightly for any of the purposes of their existence. These experiments have been so often repeated, and are so easily performed, that there is now no naturalist who doubts that the parts called eyes, in insects, really serve all the purposes of vision. But how can these eyes concur to vision, and give the insect the faculty of distinguishing objects clearly? These organs, we have said, are constructed in such a way as to fit them to receive the images from objects, by the simple shock of the rays which these objects reflect; and from this way of feeling there must result an extremely lively impression. Insects not having, like vertebral animals, a contractible pupil, it seems that, as a consequence of this organization, vision ought to be very perfect among them, on account of the great number of rays which fall continually on the facets of their eyes. Thus these animals seem to distinguish objects at very considerable distances; but the images are painted or stop on their choroid, or they pass beyond, which is very difficult to ascertain, although the first opinion seems most probable.

One of the most learned naturalists of Geneva, M. Prevost, has recently hazarded an opinion very different from that which we here propose, for he thinks that insects are completely *myopes*. He remarks in the first place, that if we prepare two lenses of the same form and substance, their local distances will be in proportion to their diameters, and in general that all the images of points similarly situated will be also similarly situated. This principle being granted, and true in itself, M. Prevost applies it to insects; but in our opinion the consequences he draws from it are not well founded. "If the eyes of insects," he observes, "were of the same materials as ours are, and of forms precisely similar, these animals would be singularly myopes; for, in order that the image of any object may be painted exactly on their retina, the distance from the object to the eye would also require to be equally small with respect to the distance required for the human eye, as is the diameter of their eye with respect to the diameter of ours." That this reasoning may be applicable to insects, there must be a perfect similitude between the matter and the form of their eyes and ours; but we know that there is not the slightest relation between these organs. In fact, the description which we have given of the two species of the eyes of insects, proves that these eyes do not present, as admitted by M. Prevost, different transparent humours, nor ligaments destined to move them. As to the applications in which M. Prevost indulges, and which make him conclude that insects ought to have a distinct view within very narrow limits, they cannot be just, since they rest upon facts which are not exact. Thus the same naturalist says, that the eyes resembling in shape those of bees cannot distinguish objects further off than five centimetres and one millimetre; and finally, those of the ant species further than five millimetres and one-tenth millieme of a metre *. But it is evident from what has been said, that it is impossible to bring any proof in favour of this assertion. Besides, in a problem of this kind, we must pay attention not only to the absolutely diminutive size of the eyes, but also to their relative diameter, as well as to the greater or less convexity; for an eye the diameter of which would be greater than that of another might nevertheless take in less distinctly objects at a certain distance, if at the same time its convexity were more considerable. This is also what observation teaches us in a direct manner, by proving to us that nature has always combined in the kinds of insects which have the most extensive eye-sight, the sphericity of the cornea with the size of the eye. The *libellulæ*, the *mantes*, the *cicindelæ*, as well as most of the *lepidopteræ*, the *hymenopteræ*, and the *dipteræ*, furnish striking examples; and if the size of their eyes is favourable to make them perceive objects at a greater distance, the sphericity of their cornea is not less advantageous for rendering distinct the impression of external objects, by uniting a greater number of rays, and hindering them from removing from the axis of vision. There is no person, in short, who, in observing the motions of insects, has not remarked, when you wish to catch them, how acutely they see you even at a considerable distance: there are even whole species which cannot be caught until you keep out of their line of vision. It cannot be said, however, that the insects are then guided by their sense of smelling, for this sense serves them probably only for better getting at their prey when they are still far from it.

We differ with M. Prevost also when he says that, in order that vision should be useful to insects, they must only see that clearly which passes close to them, probably even at the distance of their feelers and their mouths, a distance at which the human eye sees with such indistinctness, that it may well be said it does not see at all. In the first place, there is no similitude whatever between the human eye and that of insects, and consequently

* Those who have studied the habits of insects must have remarked, that there are certain *lepidopteræ*, some *dipteræ*, and even *orthopteræ* and *hymenopteræ*, who fly when we approach within the distance of about six metres.

sequently we are not warranted in establishing between these two organs any comparison. All that we can infer is, that it is as necessary for insects to see external objects, as it is for animals of a higher order. The carnivorous insect ought to have the faculty of perceiving its food at a certain distance, as well as the bird of prey, which suspended in the air would, perhaps, be condemned to die of hunger, if the acuteness of its vision did not ensure its existence.

It must however be observed, that if we only took into the account the great convexity of the cornea of the eyes of insects, we might well regard these animals as myopes; for we know that the more convex the crystalline is in the eyes of the vertebral animals, the more the species which present this arrangement are myopes, particularly if the cornea is very salient, if the humours are very dense, and finally, if the eye is too much sunk. The myopism then depends on the rays being united before reaching the retina: this inconvenience may therefore be remedied by the help of concave glasses, which retard the union of the luminous rays. But the insects which present a very prominent and convex cornea, are they also myopes as a consequence of this arrangement, as would be the case with animals in which the images fall upon the retina, after having traversed humours of different density? Although it is very difficult to solve so intricate a question, it would nevertheless appear that the convexity and protuberance of the cornea ought not to render the insects myopes, since the luminous rays have not to unite rather at one point than at another in order that the effects of vision may be obtained, the latter being effected not because an image is painted on the retina, but by the direct impression of the luminous rays on the optic nerves. The convexity of the cornea being favourable to the collection of the rays, and hindering their dispersion, this form was perhaps the most convenient in the structure of the eyes of insects. It was at least the best adapted for giving them keen sensations, since it tends to make the rays of light fall more perpendicularly. This form is also the most common in this order of animals; and even a considerable number of insects which perceive objects very far off, present their cornea very convex and prominent, such as the *papillons* and the *cicindelæ*. It may also be observed, that the concavity of the cornea, by allowing only a small number of rays to enter the eye, ought on that very account to procure less lively and feebler sensations. Thus, we only observe this arrangement in a small number of insects, and merely in the tolerably smooth eyes; but in these species, the compound eyes and the lateral simple eyes may supply the want of the concave eye.

 If

If the mechanism of vision, such as it is in insects, seemed even to the most eminent anatomists of the present age so difficult to comprehend, it would seem that the way in which they proceeded to the dissection of the compound eyes (for few interfered with the smooth eyes) has been in a great measure the cause. The case would have been different, if, in their dissections of compound eyes, they had not always proceeded from the inside to the outside, a method of operating which may lead into error; for, however little we disturb the optic nerve, the filaments which it gives off, drawn a little back by being disturbed, are no longer exhibited on the tunic of the cornea. If, on the contrary, we carefully remove the cornea, we see in an evident manner the numerous nervous filaments which pass through the tunic of this membrane, and spread a little in order to form the peculiar retina of each facet. From that instant, we need no longer explain how the light is able to act on the retina through an opaque varnish.

If we set out from the organization of the eyes of insects, it would appear that we might conceive that vision is produced among them in the following manner:

When the light meets a diaphanous body terminated by a curved surface, which gives it access into its interior, it undergoes a refraction. If its rays being parallel meet the surface of this body, and its medium be denser than that in which the incident rays move, the broken rays will approach the perpendicular by converging upon each other. But if the rays of light fall obliquely, making a very wide angle, the more obliquely will they fall, and the less will they approach the perpendicular. These are precisely the different effects which light undergoes in falling on the cornea, which unites with transparency a convex surface, and a denser medium than that in which the luminous rays move.

In fact, the luminous rays, direct or reflected, which issue from a visible body, and reach the eye, form different cones, the points of which are at the object, and the bases on the cornea. Those which fall on this membrane in an oblique direction, and by forming a very wide angle, are reflected, and do not traverse. Those, on the contrary, which fall under a convenient angle (an angle which in the human eye is estimated at 48°), pass through the cornea, or the facets which compose it, and undergo from it a refraction, which ought to bring them near the perpendicular. It results that these nervous filaments, situated immediately under, are struck by the great quantity of rays which reach them, and which, as a consequence of their direction, are concentred at the most sensible part of the optic nerve, if we may so express it, or at the expansion of this nerve, so that these filaments may afterwards transmit to the brain the impression of the rays of light, or the image.

The great number of facets which compose the cornea, is no obstacle to what we have said; but each of these facets ought to be considered as a cornea, which exercises its action on the rays of light, and makes them undergo the changes necessary for the brain to receive their impression. Nor ought their number to make us suppose that insects ought to see objects very much multiplied; for, whatever may be the number of the nervous filaments which correspond to each facet, they all set out from the spreading of the optic nerve, which we have considered as a retina sufficiently analogous to that of the red-blooded animals: it is on this retina that are painted the images perceived by the filaments; perhaps even this retina is only destined to transmit to the brain the impression produced by the rays of light on the optic nerves. This is the more probable, because images can hardly be supposed to exhibit themselves from behind an opaque membrane. The great use of the retina would therefore be to centralize the impression, and to render it unique, if we may so express ourselves.

From what we have seen, if the force and number of the rays of light had been very considerable, these rays might by their too great excitability injure the organ of sight. But nature, equally admirable in her minutest details as in her most beautiful works, has guarded against this inconvenience; and the varnish of the choroid, as well as the choroid itself, are the organs which hinder the multiplicity of the luminous rays from deranging or altering the sensibility of the nervous filaments. The varnish of the choroid and the membrane of the choroid appear, therefore, destined to absorb the excess of the luminous rays, and to diminish the too great excitability which a very strong light would necessarily have produced on the optic nerves, according to the conformation of the eyes of insects. The black and opaque bands which we observe in certain species, like the more or less coloured spots in others, seem chiefly intended to absorb the excess of the luminous rays, while those less thick and more transparent facilitate, on the contrary, the passage of the light.

It is easy to perceive how necessary this arrangement was to insects: in fact, the latter not having, like most of the vertebral animals, pupils with the property of contracting and dilating, and which, as a consequence of this arrangement, permit these animals to receive only the rays the most approximating the perpendicular, or the axis of vision, had occasion for an opaque membrane to absorb the useless rays, or such as might even be hurtful to vision. Thus it is particularly by the privation of this membrane, that we may judge of its influence on the sense of sight; and observation proves that it is wanting in all the *lucifugæ*.

According to this way of considering the varnish of the choroid and the choroid itself, we see that the opacity of the latter, always very great, may be no obstacle to vision. It is even possible that this membrane should serve to render it more complete, by preventing the sensibility of the optic nerves from being affected by the almost immediate action of the light on their texture.

As to the air tracheæ, their chief use seems to be to sustain the spreading of the optic nerve, and to circumscribe it within limits essential to the mechanism of vision. Besides, it is more than probable that the choroid is formed by a heap of tracheæ furnished by the large trachea which surrounds it. These tracheæ, which are distributed over the cellular texture of the choroid, adhere by its upper surface to the varnish, and by its inferior to the spreading of the optic nerve. This organization seems even to be proved by the examination of these various parts. The large circular trachea, placed almost on the same plane with the choroid, furnishes in its texture a very great number of small tracheæ which lose themselves in it, since we can find no trace of them after this membrane. Finally, by a prolonged maceration, we may assure ourselves that the choroid is formed by a close texture, of which, perhaps, the great number of tracheæ which lose themselves in it, is the cause. We might also remark that, in insects, the tracheæ, in being distributed over the cellular texture, form by their union with this texture various membranes, and even several of a considerable extent. The cellular tunic alone of the intestinal tube is a proof of this: it is singular enough that the choroid, formed in the red-blooded animals by a triple vascular texture, is, of all the parts of insects, that which receives most tracheæ. In short, the choroid is not the only membrane of insects in which we might find a similar resemblance. As the air is the only fluid which has any circulation in them, all the parts which ought to have either a great sensibility, or a great contractibility, generally present a great number of tracheæ. Thus we may always estimate *à priori*, either the force of a muscle, or the sensibility of an organ, by the tracheæ which we see them receive.

The situation of the optic nerve, with respect to the eye, seems of no consequence to insects. In fact, the numerous nervous filaments corresponding to the facets of the cornea, render it indifferent that the rays of light should fall with more or less exactitude on the insertion of the optic nerve, since the latter can only

only receive the impression from it by the nervous filaments furnished by its spreading.

From what has been said, it is evident that the eye in insects does not present so complex a structure as the same organ in animals of a superior order. In the eyes of insects we do not observe different humours which by their density can change the direction of the rays of light, by operating a refraction, always in proportion to the nature of the mediums which they pass through. Nevertheless, if the structure of the eye is very simple in this order of animals, it seems to accommodate itself very well with the properties and laws of light, as well as with the object which this organ ought to attain. In fact, the cornea, by its transparency, its convexity, and its more considerable density than that of the air, gives in the first place, on account of its transparency, a passage to the rays of light, while, as a consequence of these two other properties, the rays undergo a refraction, which tends to render convergent the broken fasciculi, and to approximate them to the perpendicular. As the rays which traverse the cornea strike at the same time the nervous filaments, or the retina of the facets of the eye and the tunic of the cornea, they may be perceived by the nerves, and the latter may transmit the impression which they experience to the expansion of the optic nerve. It would, it seems, be in this point that all the rays should be concentrated, in order to form the image; but as, in order to do this, they would have to pass through an opaque membrane, it is difficult to admit it. We must, therefore, suppose that the sensation is there concentred; and probably this species of retina has no other use than that of centralizing and bringing to one point all the impressions perceived by the optic filaments.

The protuberance of the cornea seems to be frequently in insects in the ratio of the smallness of their eyes: and this ratio is any thing but indifferent. In short, the more protuberant the cornea, the more numerous are the incident rays; consequently, the rays which reach the nervous filament being in greater number, vision is produced with a force similar to that which a greater dimension in the eyes could excite; with this difference, however, that insects in this case do not see so far. We may cite as a proof the *locusta lilifolia* and *acuminata* of Fabricius.

Lastly, it remains to make known the singular arrangement observed in the compound eyes of the *libellula vulgaris*, and of the *locusta lilifolia*, as well as to say a few words on the structure of the eyes of the *lucifugæ* species. The compound eyes of the *libellula vulgaris* and of the *locusta lilifolia* present a singular arrangement on account of the tunic of their cornea, which is red in the upper and posterior parts, and of a clear green in the inferior and anterior parts. These two colours are

are so decided, that the eye appears externally as if formed by two distinct parts. Should this effect take place on account of the great convexity of the cornea, and even of the position of the eye of this species? For this position and this convexity render the approach of the rays of light easier in the upper part than in the lower, because in this part the eye is perpendicular with respect to the rays, whereas the lower is on the contrary very oblique with respect to them. We also observe that the upper part presents a red colour—a colour which has less refrangibility than the green of the lower part, and which also gives less light. The lower part, to which the approach of the rays of light was very difficult, also presents a green colour, which has more refrangibility than red, and which also gives a greater intensity of light, since, according to the elegant experiments of Herschel, green rays are almost as brilliant as yellow. Thus, the red, sending back rays less brilliant than the green, and being in the point of the eye where the rays of light arrive most easily, would have been placed in this part to absorb, by its greater opacity, the excess of these very rays, which must have injured by their too great excitability the optic nerves. The green, on the contrary, presenting more refrangibility, and a greater intensity of light, would augment the excitability of the optic nerves in the lower part, and in this way correct the difficulty which the rays might experience in reaching this part of the eye. It is possible that what we have now said may be well founded: however this may be, we can only regard the explanation of this fact as probable.

This arrangement is also visible in the *locusta gigantea*, in which the eye presents externally all its upper half of a reddish brown, and its lower half of a very lively green. The centre of the eye, *i. e.* the intermediate part between those two shades, is of a bright red. These various shades are owing to the diversity of colour in the tunic of the cornea. We may even remark, that in proportion as the green tint approaches the lower part of the eye, it takes a clearer shade, and verges more and more towards the white. As to the smooth eyes, this species has only two very small convex ones of a slight red, and situated on the crown of the head between the compound eyes, and above and in front of the antennæ.

There are some *coleopteræ*, like the *blaps*, the *pedinus*, and certain *tenebriones*, which are completely lucifuge, and which, like the *blatta*, cannot support daylight. Thus, on exposing the species of these different genera to a strong light, they are seen to shun it by lowering their heads. Insects being deprived of the pupil, the explanation which has been given of this phænomenon in the nocturnal animals cannot be applicable to them.

Thus,

Thus, as we have already observed that this genus presented neither choroid nor circular trachea, might we not presume that the excess of the rays of light which arrive on the optic nerves, not being absorbed by an opaque membrane like the choroid, may injure vision considerably, by increasing too forcibly the excitability of the optic nerves? These insects are as it were rendered stupid by the excess of light: for a contrary reason, a less brisk light not increasing their excitability too much, ought it not to be more favourable for enabling them to perceive objects in a distinct manner? We might also add, that the nervous filaments which attach below the facets of the cornea, are here very short, so that they are nearer the optic expansion to which sensation is transmitted: having less intermediate between them and the general retina, this impression may be greater. Their sensibility being besides acuter, they require less light to paint in a clear manner the images on the retina, images which are soon afterwards perceived by the brain, on account of the proximity of this organ to the expansion of the optic nerve. We must also remark, that it is in the eyes of the *lucifugæ* alone that the image can be produced, since the impression made by the rays of light on the optic filaments has not an opaque membrane to pass through to be transmitted to the brain.

As to the black point which is most frequently observed at the exterior of the compound eyes, a point which seems moveable, and situated in the interior of the eye, so that it should have some relation with the ball of the eye, it seems to be produced by the point of the choroid corresponding to the small circle which gives a passage to the optic nerve, since in this part, the furthest removed from the eye, the deeper violet-coloured tunic ought to produce this point nearly of the breadth of the circle. It is clear that this point is in proportion to the size of the circular aperture through which the optic nerve passes. We see it larger in all the species in which this aperture is formed by the adductor muscle of the mandibule, as in the *locusta* and *libellula*. It is, on the contrary, less considerable in those which have formed it by a circular trachea, like the *gryllus*, the *truxalis*, and the greater number of the *dipteræ*, in which this aperture is still smaller. The species which have their tunic entirely black, like most of the *coleopteræ* and the *hymenopteræ*, as well as the *acheta*, &c. do not appear to have any; for it cannot be seen, on account of the colour of the tunic. It is, on the contrary, more visible in the species in which this tunic is of a clear colour. In short, the optic nerve does not oppose this arrangement; for the choroid covers all the internal parts of the eye, except in the points where it is traversed by the nervous filaments. As to the movement which we observe in it, it is easy to convince ourselves

ourselves that it is not a real movement. It seems only to take place, because the point being always fixed, and in the middle of the eye, consequently answering to all the facets of the cornea, on whatever side we observe each facet, the latter being capable of making it what it is, there results that kind of movement which seems to be in the black point or the eye-ball, but which depends on that made by the observer.

The size of the compound eyes is worthy of being noticed. This size is variable when we compare it with that of the body, but it seems always more considerable in this respect than in the vertebral animals. We observe in fact, in insects, that the greatest dimension of the compound eyes, in comparison with the body, is as 4 to 1, the medium as 10 to 1, and the smallest 60 to 1. I have found the latter proportion in two genera only, and I consider it therefore as extremely rare.

To conclude: the dimensions of which I have spoken, prove how large the eyes of insects are in proportion to their bodies. If it is with difficulty that we can conceive how vision can be effected in these insects, by means of their compound eyes, we shall experience much more difficulty when we contemplate the smooth eyes. These eyes are still more simple than the latter; they are even so simple, that we might fairly doubt if they performed the function of eyes at all, if experience did not demonstrate it in an evident manner. The greater number of the smooth eyes present a convex form, which is generally observed in the cornea of the compound eyes also: sometimes, however, the cornea of the smooth eyes, instead of presenting this structure, is more or less concave. This last form may nevertheless present some inconveniences in the sense of sight; for, if the light, in passing from a rarer into a denser medium, meets a concave surface, its rays, however parallel they were, become divergent; a disposition not very favourable to a distinct vision. In fact, so far from the rays diverging too much, it would seem that the optic nerve placed towards the centre of the cornea can scarcely receive the impression of them. As to the plain surface which some smooth eyes exhibit, although it is less favourable to a greater affluence of the rays of light,—it is not an obstacle to vision, since the light passing from a rarer medium into a body, which presents a plano-transparent surface, and a denser medium, its rays will approach the perpendicular at their point of immersion. We may also observe that, the smaller the smooth eyes are, the less plain are their surfaces: thus the *mantes*, for instance, which have very small eyes, have them projecting in a remarkable manner. The size of the smooth eyes is, besides, very variable in the genera which are most known: thus, it is scarcely possible to become acquainted with the connexion which exists

exists between the projection of the smooth eyes and their smallness.

The light arriving therefore on the external membrane of the smooth eyes, or upon their cornea, undergoes various changes which are connected with the form of the cornea. If it be convex, as most generally observed, and the direction of the rays be oblique, by making a very wide angle, they are reflected, and do not traverse it. If the light, on the contrary, falls on the cornea at a convenient angle, its rays pass through that membrane, and undergo from it a refraction which brings them near to the perpendicular. Finally, when the cornea is plain, the rays of light will continue their route, by approaching the perpendicular at the point of immersion; whereas, if it were concave, supposing even that the luminous rays were parallel, they would become divergent, and would remove from the axis of vision.

The light which has passed through the cornea, arrives immediately on the kind of retina formed by the optic nerve, a continuation of the small optic nerves. This retina, shaken by the approach of the rays of light, can transmit the impression of them to the brain. But the few rays which reach the smooth eyes, either on account of the sometimes unfavourable disposition of the cornea, or on account of their smallness, would have been still diminished, if there had been under the nerves a blackness and opaque coating as observed in the compound eyes. Thus, in the smooth eyes this coating is most generally of a clear colour, or even sometimes completely white. We may therefore presume that the blackish tunic of the compound eyes is destined, by absorbing a part of the rays of light, to diminish the sensibility of the optic nerves, while the almost colourless varnish of the smooth eyes, far from absorbing the luminous rays, sends them back, and thus increases the excitability of the retina formed by the spreading of the small optic nerves. It seems at least that it is in this respect that the smooth eyes may be useful to vision; for their smallness is so great, that without this arrangement they never could procure sensations very distinct.

Besides, the insects which have smooth eyes only, have a greater number of them; and as their life is generally less active than that of the species which possess two kinds of eyes, we may presume that it was not necessary to them to perceive objects at distances so great. We observe also that insects which have smooth eyes only, keep almost constantly in dark places, or at least do not go great distances, being also deprived of the organs adapted for flying. The *apteræ*, and the numerous class of the larvæ of the *lepidopteræ*, furnish a striking proof of this.

All the experiments which I made to determine to what extent the smooth eyes may be useful to the insects which present the

the two kinds of eyes, have scarcely given me any information as to the destination and utility of the former. I removed with the greatest care the smooth eyes from the *vespa gallica* and *vulgaris*, as well as from the *apis violacea* and other *hymenopteræ*, and from the *apis philanthus* and *scolia*. At the moment of their being removed, the privation seemed to retard their movements a little; but soon afterwards they acted as before. I never saw the wasps whose smooth eyes I had removed or varnished over, at all embarrassed in taking flight again. The case was the same with the *orthopteræ* which I submitted to similar tests: in short, all continued to move as before.

When, on the contrary, I passed a blackish varnish over the compound eyes, or when I burst them, I saw the insects act completely as if they were blind, turning about in all directions, without being able to guide themselves. Thus they brushed against the wall, or any obstacle which I placed before them.

All the insects which I subjected to this kind of test seemed to draw their legs very frequently over their heads, as if to remove what hindered their eye-sight. In this state we also saw them make more frequent use of their antennæ, by directing them always forward and on one side, and feeling with the greatest celerity the objects which opposed their progress. This greater mobility of the antennæ announced pretty well their uses: but in order to render them still more evident, I took some *locustæ* which have the antennæ very long, and I touched these organs with oxy-muriatic acid. The insect was noways affected thereby; but as soon as this acid came near the eye-lids, and long before it could produce an immediate action on the eye itself, the insect darted away with rapidity, showing a great degree of anxiety. This experiment, which I repeated several times, and which always succeeded, seems to confirm an opinion which I formerly hazarded on the use of the antennæ and the palpebræ in certain classes of insects*.

In short, if we remove from the insects which have not compound eyes, their smooth eyes, or if we cover them with an opaque varnish, we see them act as blindly as those which are deprived of their compound eyes. This experiment proves that, when insects have no other eyes than simple eyes, the latter perform the offices of compound eyes. As to the smooth eyes united in the same individual to the compound eyes, they seem to be of very little use. Perhaps they are merely destined to serve for the vision of objects which strike directly the point where these eyes are situated. Thus, the lateral eyes placed on the lateral summits of the head, would be destined to show the insect

* Vide my Memoir on the Sense of Smelling, and on the Organs which seem to be the Seat of it, in the *Annales du Museum d'Hist. Nat.*

all

all external objects in this direction, while the smooth eye in the front and centre would inform them of the obstacles which they might expect to find when advancing in a straight line. It is to be observed that, in this arrangement, the lateral smooth eyes are very little controled by the compound eyes, and that the eye in the centre is always calculated more directly for any obstacle which should oppose the advance of the insect. Sometimes, however, the smooth eyes are arranged in the form of a triangle on the crown of the head; when they can only be useful in enabling the insect to perceive objects placed above it. Indeed, those which exhibit this disposition lower the head so as to enable them to see straight forward. Bees and certain species of wasps, as well as the *mantes*, which have their smooth eyes on the summit of their head, know very well how to incline it, the better to distinguish objects placed before them.

The facts which we have detailed, seem positive enough to permit us to conclude,

1. That we must never judge of the extent or perfection of any sense, from the simplicity of the organ destined to produce it; for, if we regard only the little degree of complication in the eyes of insects, we might presume either that these animals do not see at all, or at least that they have not a very distinct vision. Facts, however, prove not only that they are not *myopes*, but also that they perceive objects at great distances.

2. That the eyes of insects have no relation with those of any other class, and that this organ, like all those of this order of animals, cannot be assimilated, nor even compared, to any other which performs the same functions in a different order of animals. Thus, we always run the risk of establishing inaccurate relations, when we endeavour to compare the organs of insects with those of the other invertebral animals, and *à fortiori* with those of the vertebral. In fact, insects are so formed, on a particular plan, that we ought never to establish any parallel with respect to their organs, except between the species of different orders. We may therefore say that, correctly speaking, these animals ought to be compared with each other only, because, in the immense chain of organized beings, they alone present the remarkable peculiarity of having no other fluid in circulation but air. This disposition of not presenting any ramified vessels but those destined to give play, through all the parts of the body, to the impression of this subtle fluid, the aliment of life as it is of flame, ought necessarily to have brought with it a crowd of differences in organization, which we would seek for elsewhere in vain. The nervous system is the only one which still shows that all the articulated animals have been formed on

one and the same plan; and although it undergoes fewer modifications than all the other systems, we see it exhibit peculiarities which evidently announce the absence of a centre of action in insects.

While this memoir was printing, I ascertained that the perfect insects which exist in water, exhibit some peculiarities in their compound eye. Thus, as I have already remarked, this eye is always of a dark colour, which is owing jointly to the thickness of the tunic of the cornea, to that of the varnish of the choroid, and finally, to that membrane itself. The varnish of the choroid, seen at its greatest thickness, appears to be of the deepest black; but when we stretch it, and reduce it to thin layers, it passes to the darkest violet. The thickness of the choroid is very remarkable in these species, particularly in the *dyticæ* and the *hydrophylæ*. In both, the circular trachea is totally wanting: its place is occupied by a common trunk, from which issue two principal branches, which proceed to form around the optic nerve a network of tracheæ larger than the branches which furnish them. Finally, I have remarked that in most of the insects which live in water, the retina is very near the cornea, a disposition which is also observed in the species which are not properly *lucifugæ*, but which habitually fly at sunset only, such as the *geotrupæ*.

I have already observed that certain species presented several different shades in their compound eye; but, what is most singular, there are some in which this organ exhibits in its middle part a very narrow stripe of a colour totally different from the rest of the eye. This stripe, generally of a deep tint, is almost always placed in the middle of the eye, so that it separates it into two equal parts. But why is the tunic of the cornea thus of a different colour? It is difficult to account for this. This disposition, however, is not very rare; we see it even very decidedly in the *locusta gigantea*, the *phasma rossia*, as well as in certain species of *syrphus* and *tabanus*. The *locusta lilifolia** presents even several parallel stripes of a reddish brown, whereas the rest of the eye is of a whitish green: what is most particular is, that its stripes do not exist in the middle of the eye. I have described in the course of this memoir the compound eyes of the *cloportæ*, thinking that they were true insects, *i. e.* articulated animals deprived of a circulation of blood, but furnished with tracheæ. Subsequently I have made new researches as to the organs of circulation of a great number of

* All that has been hitherto said on the *locusta lilifolia* ought to be referred to the *locusta falcata*. These two names have been confounded by mistake.

invertebral

invertebral animals, and these inquiries have proved that the *cloportæ*, which I had placed among the insects, belonged to the class of *arachnoides*; and that *phalangia*, which I had presumed to have the same organization with the spiders, ought to be classed with the insects, since they present tracheæ and a dorsal vessel without vascular ramifications. As to the *cloportæ*, they have a true heart provided with vessels, the blood of which goes to receive the impression of the air, into particular organs not ramified, and which we may consider as species of lungs. With respect to the organs of circulation and respiration, the *cloportæ* are nearer the scorpions than the spiders. In fact, the latter have a heart very much enlarged at its base: although this heart extends afterwards to the extremity of the body, it by this enlargement resembles that of the *crustaceous decapodes*, with this difference, however, that the position of these two organs is not the same. The heart of the *crustaceæ* is situated in the breast, while that of spiders is in the abdomen and the side of the back. As to the heart of scorpions, we do not see that it has any very sensible enlargements; in this respect this organ more resembles the heart of the *cloportæ* than that of the spiders. But it is truly worthy of remark, that the organs of respiration are multiplied in proportion as the heart takes an uniform diameter. Thus in the spider, in which the heart is decidedly swollen, there exist only two lungs: in the *cloportæ* the heart, without showing any sensible enlargement, as in the spider, nevertheless presents a diameter a little more considerable towards its upper part: thus we observe they have four lungs: finally, in the scorpions this number amounts to eight; but the heart in all these animals is of a diameter nearly uniform throughout its whole extent. To conclude: in all the *arachnoides* the lungs are always symmetrical, and are never found but in pairs. It is possible that the form of body may also have some influence on that of the organs of respiration, and particularly of circulation. Thus the greatest diameter of the heart in certain species of spiders would supply what was wanting in length. Nevertheless, this cause ought not alone to have influence, since in all the spider race the heart extends from one extremity of the body to another. The heart of the spiders begins, in fact, at the bottom of the abdomen; but as it sends two principal vessels into the breast, we may say that it extends from one extremity of the body to the other.

As to the nervous system of the true spiders, it is always composed of a cerebriform ganglion, situated in the upper part of the body, which we may either call head or breast, because it performs functions analogous to those two parts. This ganglion, situated above the œsophagus, surrounds it most frequently

 by

by prolonging itself afterwards into two cordons, which form new ganglions, from which other nerves issue wholly as if from the brain. But in the insects, we know that there exist as many ganglions as there are rings, while in the true *arachnoides* this number is always far less considerable. Besides, in the articulated animals, the nervous system in general undergoes few variations; it even shows in an evident manner, that all these animals have been formed on one and the same plan, which has merely undergone some trifling modifications adapted to peculiar circumstances.

From what we have observed, there will be some changes necessary in the classification of the articulated animals, and the class of insects will be better distinguished from that of the *arachnoides* than it has been hitherto; for these animals differ from each other in their organs of respiration and circulation, and even, although in a manner less striking, in those of reproduction and nutrition. This branch of the subject will form a separate memoir, on the organs of circulation and respiration of some families of articulated animals. In the mean time we shall observe, that the *arachnoides* are divided naturally enough into two principal families, the oviparous *arachnoides* and the viviparous *arachnoides*. It is besides very singular, to find this mode of gestation in animals whose organization is also very simple; for their system of respiration may be assimilated, under certain points of view, to that of some crustaceous *decapodes* with long tails: as to their circulation, it is purely pulmonary. The division of the *arachnoides* which we have mentioned, is, as we see, very different from that hitherto adopted; indeed this division will fall of itself to be still more subdivided, in order to place each genus in the most natural order. In this classification the *arachnoides* will always come after the *crustaceæ*; but the order in which the insects have been usually arranged must be totally inverted. Thus, instead of terminating the series of insects by the *apteræ*, as several naturalists have done, we must on the contrary commence with them, since most of the *apteræ* have a greater affinity with certain *arachnoides*.

The common spiders having ramified respiratory organs or tracheæ, and being thereby deprived of an organ of circulation furnished with vessels, are true insects; I ought, therefore, to say a few words with respect to their eyes.

The eyes of the *phalangium* are situated nearly like those of the *crustaceæ*, with which the common spider has besides some relations, either from the disposition of their bodies, and of their organs of movement, or, finally, by the volume of their hepatic vessels, or their livers. Thus, the position of the smooth eyes of the common spider (no others are observed in this genus) is considerably different from

from that of the eyes of the other *apteræ*. The cornea is all of a piece, as in the smooth eyes: it seems externally of a brilliant black; but this colour does not belong to its texture; it is produced by the tunic of that membrane which presents this shade. This cornea is very convex, rounded, and polished. Placed in a projection of the head, the eyes of the spider are very close to each other, being only separated by a deep sinus. The eyes are placed a little on one side of the projection which forms the coriaceous envelope, so that they ought not to be able to distinguish objects except above them and laterally. As they are immoveable as well as the parts on which they are situated, it must be difficult for the *phalangium* to perceive objects placed in a straight line with the body. This position of the eyes is the more remarkable, as it is the only one of this kind which exists in insects. It is not found in the *arachnoides*, and is only seen in certain species of *crustaceæ*, and still under many modifications.

The eyes of the *phalangium* are protected by an interorbitary arcade, and this forms above them a projection for defending them against the impression of external objects. These eyes, so far as their diminutive size has permitted me to judge, are formed by a cornea of one hard piece, thick, convex, and transparent. It seems, however, to be black when viewed externally; but this is owing, as we have already said, to the colouring of the tunic placed under this membrane. This tunic is besides very thick. We see it traversed by a nerve which corresponds with the concave surface of the cornea. The choroid and its varnish are completely black: probably, the texture of this membrane is formed by tracheæ, nevertheless it is not surrounded by a circular trachea. There proceed towards this part numerous tracheæ, but they are there ramified, instead of forming a principal trachea. This is nearly all that I could ascertain respecting the structure of the eyes of this kind of spider, so similar to the *arachnoides* in external character, and yet so different in the organs of respiration and circulation.

Having been able quite recently to ascertain the structure of the compound eyes of the crepuscular insects, such as the *sphynx* and the *noctuæ*, I ought to state that nothing which I have said on the vision of insects in general is applicable to them. The latter have, in fact, an organization so particular in the structure of their eyes, that there is necessarily a great difference in the way in which vision is effected. I thought I recognised in these eyes humours of different densities, and even a species of crystalline; we know that there exists nothing similar in the other insects.

These are the differences which exist between order and order,

 and

and which render the anatomy of insects so intricate, particularly when we wish to deduce general conclusions from observations made upon a great number of individuals. We may also add to these difficulties, which are immense, and which we can never be certain of having surmounted, those which result from the uncertainty in which we still are, respecting the organization of the great class of articulated animals. Indeed, these difficulties are the greater, because the classes of articulated animals are much less striking, and less clearly separated, than those of the other ramifications. Thus we cannot have recourse to analogy to judge *à priori* of the organization of these animals. The anatomist who should take into the labours of classificators the basis of his own researches, would run the risk of falling into greater errors, if he examined but one organ only, and he would surely bring together animals separated by their whole organization.

To conclude: we may have fallen into errors in the course of our investigation; but, as truth was the object of all our efforts, we trust we shall be forgiven.

XLV. *On a Substitute for Coffee, proposed by a French Author.*

To Mr. Tilloch.

Sir,—Trusting that you will always have the candour, in your valuable publication, to award the priority of any discovery to those who may have a just right to claim it, I take the liberty, through that channel, of suggesting, that the discovery of the seeds of the *Iris pseudacorus*, or common yellow water flag, as an excellent substitute for coffee, was not made by M. Levrat, as stated in the Philosophical Magazine for August, page 144, but by myself in the year 1809, as may be seen in the twenty-second volume of Nicholson's Philosophical and Chemical Journal, where my account of it is published. And as that paper was soon noticed by Morveau, in the *Annales de Chimie*, the subject might have been suggested to M. Levrat by either of these sources; and I am happy to find that my opinion of the usefulness of these seeds is likely to be acted upon for the benefit of our species.

I remain, yours, &c.

Wisbech, Sept. 6, 1814. Wm. Skrimshire, Jun.

XLVI. *Bio-*

XLVI. *Biographical Sketch of the late Sir* Benjamin Thompson, *Count of Rumford.*

Sir Benjamin Thompson, Count of Rumford, one of the ten foreign Associates of the Academy of Sciences of Paris, Vice President of the Royal Society of London, member of many other scientific societies, &c., was born at Rumford, near Boston in America. He attracted the notice of a professor of natural philosophy in the university of Cambridge near Boston, who having remarked in him a great degree of curiosity and intelligence, took charge of his education. He married a lady possessed of property in that country, by whom he had a daughter who still lives in the city of Boston. In the war which terminated by the separation of the North American States from the British dominion, he attached himself to the British interest, and raised a regiment. He was patronized by Lord George Germaine, who held a high situation at that time. After the war he came to London, and received the honour of knighthood from the King. He was for some time one of the under Secretaries of State. He went to travel in Germany, and at Munich was noticed by the present King of Bavaria, into whose service he entered, and there attained an eminent situation; and to his death he enjoyed the munificence of that prince. Whilst employed by the Elector of Bavaria, he formed a system of protective police for the city of Munich, and reformed the establishments for the maintenance of the poor. The public garden at Munich in the rural style was laid out at his suggestion, as were the pleasure grounds on the space formerly occupied by the ramparts at Manheim. Having returned to London, he was active in establishing soup kitchens for the poor, and travelled into different parts of the British empire, with the intention of disseminating his contrivances for the distribution of heat. He suggested the plan, and ardently assisted in the first formation, of the Royal Institution in Albemarle-street. He gave to the Royal Society of London a thousand pounds, of which the interest is to be annually presented to the author of the best Treatise or Experiment on the Subject of Heat. Since the peace of Amiens, he lived in Paris: he there married the accomplished Madame Lavoisier, widow of the celebrated chemist: this union lasted for a short time, and was terminated by a separation. He resided from that time to the day of his death at Auteuil, in the house formerly possessed by Helvetius. This house and garden are agreeably situated on the bank of the valley through which the Seine flows; it enjoys a view of the woods round Meudon. On the opposite bank of the valley he amused himself in decorating this habitation, and modelling it according to his fancy, as he

was

was wont to do in every house where he took up his residence. He was fond of employing the rule and the compass in drawing the plans for these alterations and constructions. One of the last of his mechanical schemes, was the applying broad-rimmed wheels to his coach, with a view of diminishing the jolting that arise from the inequalities of the pavement, as explained in his paper on the subject in the Moniteur.

For many years previous to his death, he was affected with a cough proceeding from a morbid state of the mucous membrane of the larynx: in other respects he enjoyed tolerable health. His death (at Auteuil near Paris, in August last) was the consequence of a fever. His age was about sixty-five years.

The faculty for which he was most remarkable in respect to science, was that of contriving instruments and experiments, as may be seen in his experiments to determine the force exerted in the Explosion of Gunpowder (published in the Philosophical Transactions), in those on the subject of Heat (published in various papers in the Philosophical Transactions, and *Mémoires de l'Institut*), and in his contrivances for the distribution of heat in dwelling-houses and manufactories (published in his collection entitled Philosophical Essays).

Having passed much of his time in the army, and in the service of princes, his hours could not be employed in constant study, like those of a man who becomes profoundly learned in mathematics, natural philosophy, or chemistry; but he reflected and speculated ingeniously on some parts of natural philosophy. He spoke German well, and French with fluency. His general knowledge was improved by the conversation of distinguished men of learning, whose company he sought in London and Paris.

He was fond of show in his domestic establishment. Studious of fame, eminence, and power, he could not but expose himself to opposition and detraction,—and he felt acutely the attacks of his adversaries. In his domestic connexions, he wished too much to regulate the actions of his associates by his own will.

He was above the middle size, well proportioned, not tending to obesity; his complexion fair; his features prominent; the character of his countenance dignified and pleasing; his usual manner and tone of voice mild.

XLVII. *On Chain Cables or Moorings.*

The great importance of safe cables for ships is known to every one in the slightest degree acquainted with maritime affairs. In circumstances of danger, the preservation of the cargo is is often a matter of great magnitude, but that of the lives of the seamen is infinitely greater. Like almost every thing connected with navigation, the means resorted to have, till lately, been confined to improvements in the manufacture of cordage, and some of these have been great; but either from prejudice or want of consideration, a change of the material for one of greater strength, and less liable to be ruptured by strain, or worn away by friction, seems never to have been even thought of.

The first, we believe, who made any attempt to introduce a stronger and better material for anchor cables and moorings, was Captain Samuel Brown, of the Royal Navy. He had the merit of at once adopting the best that could possibly be thought of, both in point of strength and œconomy. We mean good tough wrought iron; and had he not fallen into an error in principle in the construction, his invention, as applicable to naval purposes, would have almost defied the possibility of any further improvement.

This gentleman was so confident of the correctness of his ideas respecting the superior strength and consequent safety to be derived from iron chains in place of ropes, that he equipped a vessel of 400 tons, the Penelope, (and which we saw some years ago in the West India Dock,) with iron rigging, stays, cables, &c. in which he proceeded on a voyage to Martinique and Guadaloupe, and in four months returned to London in perfect order, after experiencing every severity necessary to demonstrate the efficacy of iron in place of hemp. But the introduction of iron ground tackle, we consider as of much greater importance than any thing connected with the rigging.

Since that time, iron cables have been introduced not only into different ships in the Royal Navy, but in the merchant service, and with great success; for though some did give way in severe weather, especially of those first made, it is but justice to state, that even in these cases the hempen cables of the surrounding ships had all given way hours before, and that in most instances the ships so furnished have kept their anchors, when other ships parted and drove. This important fact has been so clearly established by reports from the different captains who have tried iron cables, that we confidently anticipate the day as not very distant, when hemp will be entirely discarded from the ground tackle of every British ship.

We have said that, but for an error in his principle of construction, Captain Brown's substitution of chain for hemp cables would have been perfect. This error arose from a prejudice natural to persons who are not thorough mechanicians, or who overlook those mathematical dicta which ought to guide every mechanical arrangement—an idea that a certain portion of *elasticity* should be given to the chain. To attain this ideal advantage, a certain degree of twist, equal to nearly one-fourth of a revolution, was given to each link; so that, when a strain comes upon the chain, it never finds any part of any portion of it in that situation which would present the greatest resistance to a change of figure. In such a chain, every strain makes an effort to bring every link into that form which it ought to have had in its first construction: and in proportion as the strain effects this, so far the links have been weakened, by having the particles of which they are respectively composed placed in a new order, at the expense of the corpuscular attraction exerted by these particles individually for those to which they are most contiguous.

That this derangement of particles, and consequent diminution of strength, does take place in twisted links, is plain from what happens in proving the chains composed of such links: "a cable for a ship of 400 tons will stretch during this operation, in a whole cable nearly *thirty feet!* and will recover about *ten**," when the strain is taken off! What a derangement is here! When we look at this, and contemplate the injury hereby inflicted on the materials, we confess that we are not surprised that some of these chains have given way. That any of them should stand after sustaining such an injury, furnishes, perhaps, the strongest evidence that could be adduced, of the superiority of iron over hemp for the purposes of a cable.

It is the more surprising that Captain Brown should have fallen into this mistake respecting elasticity, having himself detected and exposed its fallacy as to hemp: "There cannot (says he) even be any certain advantage deduced from the portion of elasticity which cordage is known to possess; for the force which caused its extension may be extended for a considerable time after the cable has been stretched to its utmost limits:"—of course, under a further strain it must break. He might have added, that every lengthening of a rope by strain is accompanied with the rupture of a certain number of its fibres: every repetition of the force ruptures more of them, and thus in time it becomes unserviceable. There is no stretching without this partial rupturing: and it is equally true, that no change in the relative position of the particles of matter in the link of a chain can be induced without a proportionate rupturing, injurious to its strength, taking place, though not perceptible to the eye.

Were it even true that the *giving* of a hempen cable was in

* Observations on the Patent Iron Cables invented by Captain Samuel Brown.

its

its favour, the iron cable, from its superior gravity and the consequent weight of its curve, (an advantage justly appretiated by Captain Brown,) possesses more capability of *giving* (*i. e.* of lengthening the distance between the points of resistance) by the first effect of every strain, namely, an effort to straighten the chain, than any cable can by stretching. The elasticity, therefore, which Captain Brown gives to his chain, presents no one benefit to compensate in the slightest degree for the injury done to the iron, by giving it a form unfavourable to the resistance of violence.

The defect, and it is a most serious one, which attaches to the construction of Captain Brown's chain, has been most happily obviated in another mode of construction, for which the inventor, Mr. Thomas Brunton, of the Commercial Road, has likewise obtained a patent. In Mr. Brunton's chain cable, that arrangement which can most effectually resist every solicitation to change the form of any of the links—or, in other words, that form of link which shall present the substance of the iron in the best possible position for bringing the whole mass into equal action when assailed by an external force, has been most successfully adopted. Nor do we hesitate to state our firm conviction, that if two chains made of iron of the same size and quality, one on Capt. Brown's construction, and one on Mr. Brunton's, were tried against each other, the latter would tear the former asunder, while itself would remain uninjured. In a word, such are the science and skill displayed in the construction of Mr. Brunton's link, that we think it by no means possible to improve it, the whole strength and substance of every link being brought into equal action on every application of a strain.

Mr. Brunton's specification of his invention is at once so explicit, and at the same time drawn up in such a popular form, as to preclude the possibility of his principle being misunderstood. We shall give his description in his own words.

Specification of Mr. Brunton's *Improvements in the Construction of Chain Cables or Moorings.*

"To convey correct ideas respecting my improvements in the construction of chain cables or moorings, it is necessary that I should point out and illustrate the principles which should guide the workman in his operations. These, when thoroughly understood, will not only enable him to avail himself of my improvements after the expiration of the said term; but will qualify him to detect, and consequently to avoid, those errors and mistakes in form and construction which prevail, more or less, in all the chains that have hitherto been employed for cables or moorings. The object to be gained is the greatest possible strength

strength from a given quantity of materials, keeping in mind the direction in which the strain is to be borne. If the tendency of a strain applied to a link of a bad form be once properly conceived, a great step is gained towards the adoption of a good form. Let AB, fig. 1, (Plate V.) represent a circular link of a chain, the substance of the iron one inch. Let the outer circumference be fifteen inches, and let the inner circumference be nine inches. If receding forces be applied to the two links C and D (shown in section), pulling C towards E, and D towards F, the ultimate tendency of the effort of such forces is to change the form of the circular link into one which shall have round ends and parallel sides, as figure 2: but a very slight examination will show that, before this can be effected, the link must be destroyed: for in such a circular link the corresponding segments of the outer and inner circumferences are in the proportion of 5 to 3; and therefore every effort to increase the distance between C and D, or, in other words, to make the parts A and B approximate, must disturb the relative position of every particle of the metal, and operate to destroy its corpuscular attraction. Thus (in figure 1), the segment MN of the outer circumference being taken equal to three inches, the corresponding segment of the inner circumference will be one inch and eight-tenths of an inch. If this segment of the link is by the force of a strain to be changed from a curved to a straight form (as in figure 2), the corresponding segments of the outer and inner circumference must be brought to one length; to effect which, the matter contained in three inches of the outer circumference must be compressed into one inch and eight-tenths of an inch, or the matter which now occupies only one inch and eight-tenths of an inch in the inner circumference, must be made to dilate itself to three inches without losing its cohesion, or the required compression and expansion must be divided between the two; all of which are impossible without a derangement of the relative position of every particle in the mass.—To be brief: the matter in this part of the outer circumference may be conceived to present an infinite number of fulcrums, over which the said receding forces, by an effort to render the curve straight, must rupture and separate the matter of the inner circumference. Nor is this the only mischief that must occur, as will appear by a consideration of what must take place where the link folds round C and D; for the larger semi-circles in figure 2, each containing four inches and a half (nearly) of the outer circumference, answering to two inches and seven-tenths of an inch of the inner circumference of figure 1, must now correspond to the semi-circumference of the links C and D, which are each only one inch and a half (nearly); so that in these parts the effect, produced by the action

action of the said forces, would be the same as in the former, but reversed in its operation: that is, the matter in these parts of the inner circumference presents an infinite number of fulcrums, over which the outer circumference must be ruptured and separated by the said forces. A circular is therefore a bad form; but from the foregoing it is obvious, that if the parts A and B of the circular link, figure 1, can be prevented from approximating each other, the evil that has been pointed out will be lessened. Suppose a stay AGB to be introduced for this purpose, and, as before, let receding forces be employed in the directions CE and DF, what will be the effect? The circular link will now be able to resist a greater force than before, having two points of support; but the unsupported parts between the points C, B, D and A, will, by the effort of the said force, endeavour to assume a quadrilateral form, somewhat like figure 3: a change that cannot be effected without a derangement of the matter in the link, which must rupture and destroy it. Such stays as AGB (figures 1 and 3) have been used in chains; but such a stay only supports two opposite points in the link; and I have shown that the tendency of receding forces, applied as before described, is to straighten and consequently to rupture the parts that are still left unsupported.—My said improvements in chain cables or moorings are founded on considerations drawn from the facts that have been alluded to. If a circular link, instead of being supported only in the two opposite points A and B, have its opposite sides supported by a stay embracing two considerable and opposite segments, suppose HI and KL, by the stay HIKL, taking care to leave such openings as shall allow sufficient play for the links to be received into it, the link will be much stronger than with such a stay as AGB: but still the link will prove to be of a bad form; for the tendency of receding forces, applied as before, would break the piece MOKCH, over the point C, as a fulcrum; and the piece NPLDI, over the point D, as a fulcrum. And moreover, even if circular links could be made unobjectionable as to strength, they should be avoided, on account of the greater weight of metal which a given length of chain would require than when formed of links of a less exceptionable form. We have seen that the tendency of receding forces, applied to curved links, is to draw portions of them into straight forms; and hence it follows, that twisted links of every kind should be avoided where strength is required; for such links, even if their opposite sides be supported by an interposed stay, like AGB, must, by the application of a sufficient strain, untwist themselves to become straight, and thus have the arrangement of their particles disturbed. As the tendency

dency of forces applied as before mentioned to curved or twisted links is to convert the curves or distortions into straight portions, as above described; it follows, that links presenting in their original construction, straight parts between the points of strain, are the strongest that can be made with an equal portion of metal; and hence, links with parallel sides and semi-circular ends would in every case be preferred, were it not necessary to the quality of good chain, that it should be able to resist lateral violence as well as a general strain operating by stretching. Suppose that by any accident the link figure 2 should have its ends drawn towards Y and Z, while a resisting body at X opposes its motion in the direction of the applied forces, the side of the link next to X would be bent inward; and if in such a link a stay like AGB were introduced, then the link would be solicited by the said force to assume a form somewhat like figure 4. From the preceding considerations it is evident that, of all the forms and constructions that can be given to a link, that form and construction which shall be able to convert a lateral into an end strain, by yielding proper support to the opposite sides of the link, is the one that should be preferred: and of such a form and construction is the link figure 5, with my broad-ended stay introduced between the sides of the link; for, if this link (which presents its principal substance and all its points of resistance in the same place) be drawn towards *a* and *b*, against an obstacle *c*, it is apparent, from a bare inspection, that the parts *de* and *df*, which are supported by the parts *ge* and *gf*, must be drawn asunder before the link can give way: for the matter in *eg* and *fg* cannot be made to penetrate itself, and the two sides are compelled to retain their relative positions by my interposed broad-ended stay *h*, a cross section of which through its middle is shown in figure 6. I need hardly add, that at the time that the stay *h* is introduced the link is wide enough to receive it; and the link being red hot at the time of its introduction, and being pressed home to the stay by a die or press, or any suitable mechanical means, takes a fast hold of it, and retains it in its place. Other ways of introducing and retaining in its place my broad-ended stay may be employed; but I have found the preceding exceedingly simple and efficacious. On my broad-ended stays I have only further to remark, that they should embrace the whole or the greater portion of the opposite curved parts of the middle of the link; and even if the middle of the link be made to form two opposite obtuse angles, the ends of the stay should not embrace much less than the proportion exhibited in figure 5. But in making the said ends to embrace any larger portions, provided sufficient room is left for the play of of the links received into it, there will be no harm; only the chain will thereby be rendered heavier, which may sometimes, though not generally, be desirable."

In the preceding specification, every position in which a link can be exposed to force has been noticed, except one, which we have shown in fig. 7: but it is fair to state, that any hempen cable exposed to a force to be overcome in a bend as small as the space at C, between the links A and B, would soon be cut in two; and that no obstacle of the same dimensions, as a projecting angle of rock or coral, would present sufficient resistance to break the middle link, having no cohesion of parts at all comparable to that of the iron.

The broad-headed stay we consider as an improvement of the first importance in chains. Captain Brown employed sharp-ended pins in the middle of his links, the two ends of which evidently present fulcrums over which the link may be broken; but the broad-headed stay, introduced by Mr. Brunton, and embracing as large a portion as convenience will permit of the sides of the link, tends, as much as any thing can, to give that undisturbable rigidity (if we may use the expression) which is indispensable, whatever uninformed people may imagine, to the perfection of a chain intended to possess the greatest possible strength that can be obtained from a given quantity of material.

The public, we think, have been laid under great obligations both to Captain Brown and Mr. Brunton—to the former, for introducing the iron cable, and combating the prejudices of the public in favour of hemp—to the latter, for perfecting the cable chain. It is impossible to anticipate the advantages which will accrue to the public from the general adoption of iron cables. Many, many! will be the lives which will be saved by this invention; not to speak of the saving of property, which, though of great importance in a national point of view, is but of secondary consideration. Such is our opinion of the safety to be derived from the use of iron in place of hemp, that we have no doubt whatever, that, were ships generally furnished with a good scope of chain, of proper weight, and of the best construction, not one instance out of a hundred that now occur, of ships being lost on a lee shore, would take place. In rocky anchorage, hemp is cut to pieces in a short time in rough weather; but chain receives no other injury than that of a little rubbing or polishing, and the weight of the bight of the latter gives amazing ease to the tossing vessel—giving way to the swelling wave that elevates the ship, and then acting by its gravity to keep her as stationary as circumstances will permit.

The

The following are the proportions of the chain cables on Mr. Brunton's principle, compared with hemp cables. We have copied them from one of the cards circulated by Brunton and Middleton, the manufacturers, and who warrant them:

inch diameter iron for	inch rope	proof tons
$\frac{7}{8}$	9	12
1	10	18
$1\frac{1}{8}$	11	26
$1\frac{1}{4}$	12	32
$1\frac{5}{16}$	13	35
$1\frac{3}{8}$	14 or 15	38
$1\frac{1}{2}$	16	44
$1\frac{5}{8}$	17	52
$1\frac{3}{4}$	18	60
$1\frac{7}{8}$	20	70
2	22 or 24	80

The above proofs to which the respective sizes are subjected, would break the hempen cables for which each is substituted; but the chains would bear twice the strain stated, before giving way. They are never, however, exposed, in proof, to a greater strain, and for a good reason—Every strain greater than that to which the vessel for which it is intended could bring upon it, would be an unnecessary exposure to a partial rupturing of some part of the material; and consequently would take from its primitive strength; which it ought fully to possess when exposed to the regular performance of its duty.

On this point we may notice one circumstance so plain that it must strike every man of common sense; but there are so many fools in the world, that too much care cannot be taken to guard against the consequences which might flow from their inattention. A chain made for a certain strain—say as a substitute for the cable of a ship of a given tonnage—should never, on any pretext, be employed for a vessel of greater tonnage. This is a point of so much importance, that it should not in the smallest degree be left to the discretion of any individual to depart from it. The best means, perhaps, that can be devised to prevent an abuse of this kind, and the consequences that might flow from it, would be, for the underwriters at Lloyd's to insist on the exhibition of a proper certificate, to prove that the scale of size has not been departed from, before they admit the ship to be properly equipped. If this be attended to, the chain cable will continue capable of bearing the strain for which it was intended, however often repeated, till a failure is induced by fair tear and wear, or the actual decay of the material, which would probably require a term longer than the ship itself would last. How many hempen cables would be worn out in that time! If we are rightly informed, West Indiamen, on an average, require their cables to

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be renewed every two years, and East Indiamen require a renewal every voyage. In point of œconomy, therefore, as well as safety, iron cables are far preferable to those of hemp. T.

XLVIII. *On Fire Damp in Coal Mines.*

To Mr. Tilloch.

Sir,—A valuable paper at the commencement of the thirty-fifth volume of the Philosophical Magazine, on the subject of *Damps* in Mines, has made me and others very anxious to hear further from the very intelligent and practical author of that paper, particularly since the dreadful accidents which have successively happened at Felling Colliery near Newcastle. I am induced to trouble you on this subject at present, in consequence of having just read in a periodical work, the recent opinion of a gentleman at Newcastle, ascribing the copious generation of hydrogenous gas in Felling Colliery, to the decomposition of the water, brought in contact with a large surface of bad and pyritic small coals, that are dug from under the marketable seam of that colliery (in the process of holing or undermining the coals, as I undertand), and are left in the old workings.

To which very probable source of the evil, as it appears to me, the editor of the work alluded to, has opposed the following remarks: viz. "I do not see how the presence of pyrites in coal should occasion or increase the evolution of carburetted hydrogen, which there is every reason to consider as the only *fire damp* that ever makes its appearance in coal mines:" and then he goes on to state, that fire-damp is peculiar to *deep* coal-pits, and is unknown in shallow ones!: and that from the comparative levity of inflammable air, explosions would be unknown, "if the mines were ventilated according to the well known principles of hydraulics;" that is, if this gas was let or drawn off *at the highest point:* without his seeming to be aware, that the accidental and progressive falling of the roofs of most coal-pits, occasions numerous higher places or dome-like cavities, above the old hollows, and even over the gates and passages in too many instances, wherein it is impossible to prevent the lighter gases, where they abound, from accumulating. It is often to the falling of a large flake from the roof of a work, and driving the hydrogenous gas down before it to the candles of the workmen, that explosions are to be attributed. Hoping, through your Magazine, to hear the opinions of some well informed practical miners on these subjects, I remain, yours, &c.

Oct 1814. An Engineer.

XLIX. On

XLIX. *Question respecting the best Process for obtaining Copper in a pure State.*

To Mr. Tilloch.

Sir,—I beg leave, through the medium of your publication, to request the favour of any of your correspondents to inform me of the best process for obtaining copper in a pure state, fit for alloying gold, and for manufacturing delicate gold-coloured articles, without occasioning brittleness in the gold. The fine copper employed in the trade for the purpose of melting with gold, was formerly imported from Germany, and is now not to be procured. The ductility of gold alloyed with the fine copper now kept on sale, may indeed be restored by repeated melting and hammering the alloy; but these processes occasion loss. I, as well as others, have tried the processes recommended in the most popular works on Chemistry: the gold which they afford is not pure. Sir Humphry Davy, in his Elements of Chemical Philosophy, page 415, mentions a method very little different from those of other writers: but the gold alloyed with copper thus obtained is always brittle; whereas the German or Dutch copper does not impair the ductility of gold in any degree whatever. It would be doing a material service to a certain class of manufacturers, to have instructions from some of your correspondents to manufacture this article.

I am, sir,

Your most obedient humble servant,

Birmingham. David Lloyd.

L. *On Fulminating Silver.*

To Mr. Tilloch.

Sir,—In your last number, the folly and danger of allowing mischievous boys the use of packets containing fulminating nitrate of silver are very properly exposed. All knowledge however is useful, although its occasional misapplication may be partially injurious. This is particularly applicable to the use of fulminating powder, which may be converted into an instrument of household protection, in times like the present, when immense numbers of individuals feel more inclined to rob than work for a living. Its application to this purpose is equally simple and safe. Let prepared packets of this fulminating powder, such as were lately sold for idle and foolish purposes, be placed round windows and doors likely to be assailed by robbers, every night, and removed every morning; and should any attack be be made, or any of those windows or doors be forced open, the consequent explosion of the fulminating powder must serve at once to alarm the robbers, and awaken the inmates to a sense of their danger. Were it once generally known that all houses are supplied with these instruments of alarm, very few or no robbers would venture on breaking into them by the doors or windows. Nor would there be the smallest danger in such explosions; for, however properly the magistrates acted in prohibiting the misuse of fulminating balls, &c. it is certain that, if properly prepared, neither paper nor wood could be ignited by their explosion. The ingenious Mr. Accum, of Compton Street, has prepared fulminating silver, and made it up in various safe forms, which could be used for the purpose here proposed; and it would well become the magistrates to show as much zeal in this useful application of a chemical discovery, as they did in preventing its abuse. But, whatever may be the opinions of magistrates, it is certain that every prudent housekeeper will adopt some plan of protecting his property from the nocturnal depredations of trained bands of robbers, and gladly avail himself of at least a cheap means of alarm, if not of defence against danger.

Yours, &c.

CHEMICUS.

LI. *Dr.* Spurzheim's *demonstrative Course of Lectures on Drs.* Gall *and* Spurzheim's *Physiognomonical System.*

The introductory lecture gave a clear and comprehensive view of the subject proposed to be explained in the course; and Dr. Spurzheim stated that he had called his lectures demonstrative, because he intended to show by sensible characters on real skulls the basis on which his physiognomical system is founded. He defended the science of physiognomy, after Lavater, by observing that all men are physiognomists, and that they only want the knowledge of reducing their observations to a system, to give them permanency and greater accuracy. To methodize those observations is the principal object of his plan. The faculties, he observed, never depend on the temperaments, which are merely physical; but the manifestations of the mind are known only by the organization of the body: we cannot perceive mind, but only its effects on the body; and from these effects we judge of its existence and its powers. When the same effects are uniformly attended with the same characters of mind, we deduce our knowledge of the latter from the former; although, in fact, it may be that the physical effects are only the visible consequences of the operations of the mind, which is itself invisible. This deduction of the character of mind from the visible appearances of the body, neither favours materialism nor fatalism, as alleged by the opponents of the science; the organs of the brain being only the material conditions of the mind's peculiar manifestations. This Dr. S. proves, by observing, that although the five senses exist in all men, yet they are not necessarily obliged always to be active: we are not under the necessity of perpetually enjoying odours or music, although the senses of hearing and smelling must always exist in the living body. The anatomists, and not the moralists or divines, have been the first to accuse physiognomy of having an injurious tendency. The moralists possess most knowledge of human nature, are the most enlightened and best informed class of men: the philosophers, solely engrossed with some favourite pursuit, judge of all other men by their own feelings; whereas the moralists observe the manners and actions in general, compare, reason, and draw just conclusions on the conduct of men in society. Yet physical and moral truth are the same; both must be founded in nature: laws and religious opinions are permanent, according as they have this foundation, or are only founded on extraneous circumstances. Dr. S. proved the advantages of physiognomy to artists, physicians, teachers, moralists and legislators, as all knowledge is useful when well applied. He showed various skulls and models, to prove that where there was little or no brain there was a corresponding want of mental faculties; the heads of idiots without almost any forehead illustrated this truth, especially when contrasted with those of philosophers, as Bacon, Newton, Shakespear, Milton, Voltaire, Haydn, &c. The skulls of malefactors were also contrasted with those of divines; the general difference between the skulls of males and females clearly ascertained. Women in general are less profound reasoners than men, and indulge more in feeling; their skulls are therefore longer and smaller, they are less elevated before, and project more behind, than men's. The ancients correctly observed this difference, as appears by the statues of their gods and goddesses, heroes, philosophers, and gladiators; the latter have always the back part of the head strongly marked and protuberant. The bust of Nelson, if correct, he observed, differed from that of the heroes of antiquity; but its accuracy is more than problematical.

Lect. 1. Dr. S. exhibited Lavater's supposed gradation of the head of a frog to that of the Apollo Belvidere, through nearly 50 changes. The object, he said, of his lectures is double; to present anatomical demonstration; and the proofs that mind manifests

manifests itself only by the organization. Mind is not material, the brain being only its instrument or organ: the bone or skull is not the brain; neither is craniology to be understood as treating of the cranium, but of the physiology of the brain. The skull only bears the marks of the brain, whence its form and existence are inferred. The brain contains all the faculties; which we know only by their manifestations: a man may possess a faculty or disposition for music; but we cannot say whether sacred or profane music be his study: we may discover the susceptibility or predisposition, but not the precise character or application of that power; it is the latent, and not the determinate action, which is indicated by the character or configuration of the brain. Physical diseases of the brain are not accompanied with corresponding ones of the mind; all the cerebral parts are double: as we have two eyes, two ears, and two nostrils, so have we two brains: hence a partial loss of brain destroys not the mental faculties, any more than the loss of an eye or an ear, the sense of seeing or hearing. Our observations, however, are here very imperfect, as there may be many defects in the faculties of persons having wounded brains, which are never noticed by casual observers.—Case (the 14th, seen by the lecturer) of a hydrocephalic patient, a young man of 19, in London, whose skull is greatly enlarged with water; it measures 23 inches in circumference, and appears like a turban on an agreeable countenance. Water separates the lobes of the brain horizontally, and not perpendicularly; it never lodges next the cranium, but always in the interior parts of the brain, which seldom contains above two spoonfuls.—Ossification of the brain, an imaginary disease. If ossification could be proved, the whole craniological system must be abandoned, as Dr. S. attaches more importance to the brain than all preceding physiologists; he considers it not only the seat of the mind, but of all the moral sentiments and affections. Two Italian anatomists found an ox with what they supposed an ossified brain: they acknowledged, however, that the animal was not only without brain, but also without nerves; although he appeared in a perfect state, and with all his senses! What is called ossified brain, is nothing but bony excrescences found in the skull: the lecturer has seen nine different specimens of such osseous matter, preserved in cabinets, erroneously supposed to be ossified brain; but many of them were of very different figures, and much larger than the original brain of the animals in whose heads they were found.—Small brain of an ox, crocodile, tiger, and tortoise weighing 300lbs, contrasted with that of man. Brain is absolutely necessary to mind and to moral sentiment; it is not in the viscera, but in the brain, where feeling exists: al-

though

though all languages have the expression "a good heart," yet the feeling is in the head. So, of all the other affections, shame manifests itself in the skin of the face, yet we never say that the feeling of shame is in the skin: sorrow makes us shed tears; but we do not thence say that it exists in the lachrymal glands: it is sympathy which produces these effects. Measure, or absolute dimension, is no criterion of mind: elephants have a larger brain than man, apes and dogs less than an ox. Artists err in supposing any relative proportion between the head, mental faculties, and the dimensions of the body. The Venus de Medicis has a head quite too small for nature; it would be the face of an idiot if animated.—Camper's facial angle taken from the jaw to the ear and the top of the forehead. The more acute this angle, the more intellect, or the greater the powers of mind. This conclusion untenable, and contradicted by facts: it would operate against two very intelligent Negroes now learning the British System of Education at the Borough-road School, in order to practise it in their own country (many of the weakest men in this country have this facial angle very acute). No proportion of parts can be established, not even between the brain, cerebrum, and cerebellum; some have a large, others a small cerebellum, without any peculiar characteristic. Women, generally reasoning less and feeling more than men, have less brain in front, and more behind.—Every one has a peculiar talent, a bias for one thing or another; can follow one pursuit till fatigued with it, and then apply to another; one organ rests while another is occupied. This is owing to the plurality of the organs of the brain, which, when all are at rest, produce perfect sleep; when only a part rests, dreaming is the consequence.—Somnambulism prevails when many of the organs of sense are active, but when too many of them are still at rest to give a *will* to the person: somnambulists can often not only walk, but see and hear; yet, the whole organs and powers of the mind not being awakened from sleep, they have no will.—Visions are occasioned by transferring internal sensations to external objects: these, if permanent, become actual diseases, real insanity. This diseased state, the patient being perfectly well excepting in some one point, proves the plurality of the organs, and also the necessity of all to make a perfectly rational being. Dr. S. related several cases of this kind of topical insanity.

Lect. 2. Means of determining the faculties by the functions of the brain. Anatomy does not show these functions, and structure is no guide; but anatomical structure is not contrary to the functions or physiology of the brain.—No inferences to be drawn

drawn from partial sections of the living brain, as the animal is injured by either vertical or lateral sections, and consequently cannot evince its faculties.—No general organs of feeling or sensation can be indicated; the functions of the brain and the signs of the disposition of the mind are the same.—Every thing in nature is more powerful in proportion to its mass: the larger the muscular fibre, the greater the force or energy: the more considerable the nerve, the more energetic the function.—Necessary distinction between functions and the conditions of those functions, as they may be active or passive.—Temperament adds to energy, exercise improves the faculties: consequently the whole grounds of judging are very compound and complex.—Size is sufficient to indicate the function, although mere dimension of muscle is no index of strength: the same applies to the five senses, and to the organs of the brain. Size contributes to energy; and from the external configuration of the head, that of the brain may be ascertained.—Want of parallelism between the tables of the skull (the two hard bones on the outer and inner sides of the cranium, between which is the cellular substance called *diploe*) no argument against the accuracy of our knowledge of the shape, parts, and functions of the brain: the contrast of different heads and skulls with and without the hair and integuments only proves the latter to be much less, but not of a different configuration.—Nerves are large in proportion to their expansions; (olfactory nerves of a calf larger than those of a man) hence the functions are judged of by the external expansions, which determine the whole organization.

Practical application of this system.—Skulls too large or too small indicate disease or idiotism. Size of the antique not found in nature. The configuration only to be considered or relied on: bony excrescences or projections not to be confounded with organic developments. All persons have all the organs; but in some they are more developed than in others, indicating a particular exertion or bias of the mind. One exception, a child in Paris was found without the lateral convolutions. Fibres of the brain long or short, thick or thin: the long and thick have greater energy; the long and small, or thin, disposition, activity, but neither energy nor success. A groove in the posterior part of the skull indicates that the two hemispheres of the brain are separated a little, and a greater development may be expected on the sides. Anatomists often call every bony elevation above the eyes, a frontal sinus; but the sinus is contiguous to the eyes, and all elevations above it are organic developments of functions. Various depressions of the eyes outwards, downwards, (which swells the under eye-lids,) upwards, inwards, and projecting prominency, all of which are occasioned by the pressure of the brain and its functions, as highly developed organs, often push others out of their places. The most elevated points in a skull, placed in different positions, always indicate the centre of the organ. In examining a skull, notice first the most prominent parts: if there be only one peculiar prominence, it is easily discovered; if a great many, more acuteness and accuracy of observation are necessary. According as the convolutions are transverse or lateral, so are the elevations of the skull. Size indicates the organs, but not the application; the power, but not the mode of exercising it. Here the want of parallelism in the tables of the skull is by far too trifling to attract attention or calculation; the physiognomist must always have more conspicuous characters or marks to direct him. Great elevations on the skull always indicate a peculiar bias of the mind.

Lect. 3. There are three states in which the physiognomical system cannot be rigorously employed; in infancy, old age, and disease. The brain grows like the rest of the body; and as we cannot say what size and figure it may attain, neither can we determine how much it may have diminished by absorption in old age, nor how far the space between the tables is increased at any particular period. As the brain diminishes, the interior table adapts itself to it, and the space between the outer and inner table increases: hence in old age and chronic insanity, as well as infancy, no physiognomical judgement can be formed from the external configuration. Madmen have generally thicker, heavier, or denser skulls than sane persons; of which examples were shown. Many suicides have skulls of the same description: suicidism sometimes a disease, occasionally an epidemic, and often national. In Austria last year there were only 33 suicides; in Paris there were as many every month. At Hamburgh and in the North suicides are numerous. As to the causes of cranial configurations, they are foreign to this inquiry: we know little of original causes, it is quite sufficient to know facts: it is immaterial to the physiognomist, whether muscles, brain, or the fleshy integuments, occasion the configurations of the skull: it is enough that he knows, by innumerable observations deduced from experience, that such and such appearances are always accompanied by such and such characters of mind. It is however certain that the muscles do not, as some anatomists have supposed, change the form of the bones, as unborn infants, before the muscles come into action, have skulls of different forms. The brain is formed before the skull: as the former increases, it deposits osseous matter, which radiates from a centre, and by successive radiations

radiations a complete skull is formed. It was observed that the skulls of females are long, and project backward: now it is evident that this form cannot be owing to the muscles, which do not act horizontally. As the brain enlarges, so does it deposit osseous matter according to its own peculiar figure. This process is ascertained by observing the fact, that if an eye be extirpated, its orbit soon becomes contracted, in consequence of the deposition of bony matter. The ridge, or vertical elevation, often seen in the forehead of children, does not experience any diminution; but it becomes gradually less apparent by the increase of the adjoining parts. Cranial elevations and depressions have forms totally different from those of the insertion of muscles, and exist where there are no muscular insertions. The alleged mechanical pressure on the heads of the Caribs, as stated by travellers, Dr. S. thinks can have no influence in giving their foreheads that flatness for which they are remarkable. The skull is very strong, and well formed to resist injuries, consequently must require a very great force to change its natural form; and such a force cannot be applied to the forehead without a counter-pressure or opposite resistance, which would necessarily produce a very different form of the skull.—He next considered the physiognomical signs. Dr. Gall, while he followed the opinions of the schools, long laboured in vain to discover the organs; his greatest difficulty was to discover the faculties. There is no organ of instinct; and the opinions and language of philosophers respecting it, memory, judgement, imagination, affections, and passions, are more erroneous than the common language, which is consonant with nature. In the agony of disappointment and uncertanty, Gall said to himself, "I know nothing," and began *de novo* to study men, as a Linnæan botanist does an unknown plant. He observed men's actions, and compared them with their cerebral organization; examined individuals who excelled in some one thing alone, and traced the co-existence of this particular character with some specific appearance, or prominent organ, on the skull. He continued his observations, and ascertained, from an immense number of examples, that different external characters always accompany different characters of mind, and that the same configuration of the head is uniformly attended by sameness of intellectual character: he also examined the negative traits, and finally deduced from experience, that certain elevations on the skull have certain mental characters. If such an energy or faculty of mind, he argued, be always attended, as according to his experience it unquestionably is, by certain organs or configurations of the skull, then must the same conclu-

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sion be drawn, as in every other department of natural and physical science, that the characters of the mind are deducible from the organization of the head. By multiplying his observations on the developed organs and the corresponding intellectual faculties, Gall succeeded in forming and correcting his physiognomical system, which may be learned and improved by succeeding students of human nature, who should always begin with the more simple, and proceed to the more complex; from a head which has only one highly developed organ, to that which has many; and finally to those whose organs are all equally developed. Handel's organ of music, (a vertical and angular ridge rising from the extremities of the eye-brows,) for instance, is easily discovered. Experience, exercise, and incessant observations, aided by multiplied collections of skulls, are necessary to make an expert craniognomist. Observers are also assisted by the anatomy and physiology of the brain, by comparative anatomy, (for there is the same difference in the faculties of animals as of men,) by partial insanities, and by mimicry. It is however absurd to expect that physiognomy should be accompanied with the spirit of prophecy, or be required to tell whether a person should ever become insane. This is impossible: yet, if one faculty be indulged above all the others, and the person become diseased so as to debilitate him, insanity may be anticipated as a consequence. Thus, if the organ of self-love, or pride, (an elevation on the top of the crown,) be extremely developed, derangement may supervene. But there is no difference in the configuration of madmen's skulls: insanity is merely a disease, which produces no greater changes on the figure of the brain, than diseases in general in other parts of the body: if the eye, for instance, be inflamed, its external form is not consequently changed: if the thorax or any other part be diseased, a change of form does not necessarily follow.—National physiognomy and craniognomy. An aboriginal Frenchman, German, or Englishman, may change his pronunciation and language, but cannot the configuration of his skull. Mimicry or expression indicates the activity and energy of the faculties. As the organs of the brain are directed upwards, downwards, backwards, or forwards, so is the body: thus, a proud man always walks very erect,—and so of others*.

* The number of instances in which Dr. S. has pronounced rightly on persons has really astonished the medical men of the metropolis.

LII. *In-*

LII. *Intelligence and Miscellaneous Articles.*

BURNING OF METALS.

M. Vauquelin has communicated to the French Institute an account of the results of some recent experiments, which are highly interesting to assayists, gold- and silver-smiths, and all workers in the precious metals. This chemist, placing four grains of silver in a cavity of ignited charcoal, observed that, when he directed a current of oxygen gas on the metal, it produced a conical flame, the base of which had a yellow colour, the middle purple, and the top blue; and that, by collecting the disengaged vapour in a bell glass, he found the receiver covered with a brownish yellow crust, which was almost wholly dissolved in cold very dilute nitric acid. In this experiment the four grains of metal disappeared in less than a minute. M. Vauquelin thinks that the silver burned at the same time with the charcoal, and that it is to this cause the yellow-coloured flame of the latter must be ascribed.

ON THE PHOSPHORESCENCE OF THE COMPRESSED GASES.

M. Dessaigne has addressed the following letter on this subject to M. de la Metherie.—"Several years ago, M. Mollet of Lyons made known the curious fact of a light appearing at the mouth of an air gun, when it is discharged in the dark. In 1810, in a memoir on phosphorescence by collision, which I read to the Institute, after having made known several facts in which the luminous appearance is produced by the separation of bodies only, I had concluded that there were, with respect to the light concealed in bodies, two modes of excitation; the one which is the result of a pressure, and the other which is produced in the expansion.

"Subsequently the French chemists have made us acquainted with two mixtures, in which the luminous excitation also takes place by an expansive movement at the instant of their decomposition.

"I took a cylindrical glass vessel, and closed its upper orifice with a wet bladder, which I stretched and firmly tied down around it. I allowed this bladder to dry in the air until no appearance of humidity remained; after which I placed the vessel on the stand of an air pump, and made a vacuum in the dark. At the moment when the air by its pressure burst the bladder, in order to precipitate itself into the vacuum, *a very brisk flash of light illuminated the interior of the receiver.*

"This experiment had a pleasing effect when made at night: the light emitted is white and intense, like that of the combustion

tion of oxygen gas with hydrogen gas in Volta's eudiometer; but it is circumscribed in its breadth, and extends to the bottom of the vessel. We cannot compare it to any thing so well as to those flashes of fire which dart through the clouds in a storm.

"When the bladder bursts of itself before having made an entire vacuum, the light which is then emitted is weak, reddish, and appears at the bottom of the vessel only. In general it is the stronger and more abundant as the vacuum is more perfect when the bladder is broken. When the rupture of the bladder takes place simultaneously at two different points, we see two luminous points: in the contrary case we see one only.

"May not the lightning which precedes the thunder be produced in the same way?"

SUBSCRIPTION PROPOSED FOR MINERALS.

The Journal des Mines for May last contains the following notification: "The zeal with which the study of fossils and petrifactions has been prosecuted for several years, has suggested to the members of the *Bureau de Minĕralogie* established at Hanau in Wetteravia, the idea of forming systematic collections of petrifactions, which they offer to furnish in successive portions at stated periods; and they flatter themselves, in consequence, with obtaining the approbation and support of amateurs and the learned. These collections, it it said in the German Prospectus published on the subject, will present not only in isolated individuals all the fine remains of the organic world, the greater part of which are lost to us, but they will exhibit as perfect a view as possible, in pieces well characterized and carefully selected. Scientific travellers will be sent into countries which are richest in petrifactions, and will thus procure the means of supplying gentlemen who form collections, with series as complete as possible.

"But as an enterprise of this kind requires a considerable advance of money; and as it is desirable, before procuring from far distant countries, such as Italy, Switzerland, and France, the articles which have been already ordered, that a deposit should be made on which reliance can be placed, the plan of a subscription has been preferred.

"Every delivery will be composed of 50 specimens of the size of two inches by four, for petrifactions still adhering to the matrix, or a proportionate number of isolated specimens when the petrifaction itself is within or beyond this size: and the price of each delivery will be six reichs thaler (about 1*l.* 1*s.* English) to subscribers, and nine rix dollars (or 1*l.* 11*s.* 6*d.*) to non-subscribers.

"The undertakers pledge themselves to furnish specimens of an excellent quality; and none shall bé given twice, unless when the matrix happens to be different; which, on the contrary, will present a double interest to the amateur who makes the collection with a view to geognostic speculations.

"A specification will accompany every delivery, and with the last will be given an explanatory and recapitulatory catalogue.

"As to petrifactions of great price and rarity, one specimen will serve for several deliveries, and it will be valued accordingly. The deliveries will take place at intervals of from three to four months; so that in a few years the collection will be complete.—The subscription for the first delivery, which will take place in November 1814, obliges the subscriber to take the two following: but payment in advance is expected for the first only; and every subscriber is at liberty to decline taking more than the three. Any person procuring ten subscribers shall have a collection for himself gratis. It is proposed to exchange fossils with naturalists who reside in countries which abound in petrifactions, and who wish to procure this accommodation. Letters and money to be addressed (postage free) to *Hanau* by Frankfort on the Maine."

NEW VOLCANIC ISLAND.

A very singular phænomenon has occurred in the Russian province of Tschernemerk, in the vicinity of Alternzruk, immediately opposite the salt-works. On the 10th of May last, at two P.M. the weather being calm and serene, a noise was suddenly heard from the sea, about 200 toises from the shore. Flames issued forth, accompanied with explosions resembling those of cannon; thick clouds of vapour rose at the same time; and enormous masses of earth and huge stones were hurled with force into the air. Ten very violent eruptions took place successively within a quarter of an hour. Those that followed were protracted till after night. Then an islet was seen to rise from the sea, pouring forth from several mouths a bituminous matter, which subsequently assumed a firmer consistency. While this phænomenon was evaporating, a particular smell spread about the distance of ten wersts around: this smell had not any resemblance to that of sulphur. On the 20th, the examination of this islet was commenced. It was thought to be inaccessible, being surrounded on every side by hardened bitumen: at last they succeeded in penetrating to the interior. Its elevation is a toise and a half above the level of the sea, and its surface entirely covered with a stony whitish mass.

FALL OF METEORIC STONES IN FRANCE.

On the 5th of September, a fall of stones took place at Agen in France. M. Lamoureux, an eminent French naturalist, happened to be on the spot at the time, and was an eye witness of the phænomenon. He has transmitted a full account of it to the Institute, accompanied by several specimens of the stones which fell. It appears from his narrative, that at eleven A. M. the sky was clear, calm, and transparent, as is the case in almost all the southern provinces. Suddenly there appeared in the north-west of the department, at a great height, a sombre cloud moving slowly, and apparently of circumscribed dimensions, for at first its diameter did not seem to exceed a few feet. It speedily began to move more rapidly; the cloud revolved as if on its axis, with a noise similar to that of continual thunder; a terrible noise was heard, and the cloud burst asunder. At this moment the inhabitants of several communes were struck with terror, on seeing fall around them a shower of stones of considerable size, and which made holes in the ground several inches in depth. The stones collected resemble those which fell at l'Aigle, des Landes, and other places; but they are of a clearer gray, and of a less consistent texture.

BEES.

The French agriculturists, who attend to the management of bees, have recently employed with success a new method of obtaining the honey without killing these useful insects. By burning one of the mushroom species (the *lycoperdon stellatum*) at the aperture of the hive, the smoke produces a state of stupor, which lasts for a quarter of an hour only; and during this interval the bees may be safely put into another hive, and the contents of the old one removed.

M. Bergman, of Berlin, recently examined with great minuteness the bark of the *prunus padus*, and found that it contained a great quantity of prussic acid. Water distilled from this bark has an odour as strong as that of the leaves of the *laurus cerasus*, and an ethereated oil is obtained similar to that of *laurus cerasus* and bitter almonds. This water has a peculiarly energetic action on animals. Thus, a dog of middling size, which was made to swallow half an ounce, died in ten minutes; and another dog died in half an hour, after taking an ounce and a half.

The above distilled water, the infusion, and the bark in powder, have been employed with success by Dr. Bremer of Berlin, in gouty diseases and in some other cases. M. Bergman purposes to examine the bark of the *prunus padus*, when taken off the tree at different seasons of the year.

Mr. Tilloch.

Sir, An unfortunate omission in the manuscript which I sent you last month on the existence of alcohol in fermented liquors, has rendered the principal experiment in it unintelligible. Permit me to state, that the passage included between lines 34 and 39 in page 210 should have run thus:—"To make an experiment as unexceptionable as possible, I dissolved 36 troy ounces of sugar in 144 of water &c."

I have the honour to be, &c.

Dublin, October 1814. M. Donovan.

Mr. Thomas Forster is publishing a second and enlarged edition of his "Researches about Atmospheric Phenomena," illustrated with six copper-plate engravings of the clouds.

We take this opportunity of announcing the appearance of Facts and Observations deduced from long and extensive Practice on Liver Complaints and bilious Disorders in general, and on such Derangements of these Organs as influence the biliary Secretion; with some new and practical Observations on the various Appearances of this important Secretion; connected by an appropriate and successful Mode of Treatment: and the whole illustrated and confirmed by a numerous List of Cases. By John Faithhorn, formerly Surgeon in the Honourable East India Company's Service.

LECTURES.

The following arrangements have been made for Lectures at the Surry Institution in the ensuing season:

Mr. Wheeler, on Chemistry, to commence on Tuesday, Nov. 15th, and to be continued on each succeeding Tuesday.

Mr. Rippingham, on Eloquence, to commence on Friday, Nov. 18th, and to be continued on each succeeding Friday.

Mr. J. Mason Good, on Classical and Polite Literature, to commence on Friday, Jan. 6th, 1815, and to be continued on each succeeding Friday; and

Dr. Crotch, on Music, to commence early in February.

Meteoro-

Meteorological Observations made from September 26 *to October* 19, 1814.

Sept. 26.—Showery weather. Wind SW.

Sept. 27.—(At Farningham.)—Cloudy; some gentle showers; cool air; in the evening orange haze at twilight in W. below a cloud.

Sept. 28.—Rainy morning; fair afternoon, but cool; a sort of *cirrostratus* like windrows of hay by moonlight, at 11 P.M.

Sept. 29.—(At Tonbridge Wells.)—Low confused *cumulus* dragging over the hills, and involving me in a dense fog, early; windy day, with showers afterwards and the different modifications.

Sept. 30.—(At Clapton.)—Wind high from the east, with clouds. Thermometer 6 P.M. 54°. Barometer as high as 30·12, notwithstanding the wind and showers—a circumstance which we must attribute to the easterly wind*. By night flattish masses of cloud sail along rapidly in the wind.

Oct. 1.—The east wind blew strong. The clouds cleared off, and the day became quite clear and dry; a few clouds again at night.

Oct. 2.—Clear dry windy day; *cirrus* appeared early, and through the day played about the sky; about sunset there were large masses, the remains of *cumulus*, which put on a sort of lumpy irregular cirrostrative form. The western sky by night was orange-coloured: while viewing it from the Leabridge road, about six o'clock, I noticed above the pale orange, which extended some degrees above the horizon, a very beautiful crimson blush fading towards the zenith into purple: it came on suddenly, and was of short continuance; was very partial; and there was a clear light-blue appearance of the sky to the south of it: in some places there was a greenish colour. Barometer rising 30.25. The night very cool. Wind E.

Oct. 3.—Clear dry cold E. wind; *cirrus* played about throughout the day; some *cumuli* observed by moonlight at night. Barometer 30·35. Thermometer 11 P.M. 43°.

Oct. 4.—A difference in the electric state of the atmosphere to-day, and denoted by different clouds. To-day only *cumuli* prevailed, a few threads of *cirrus* in the afternoon. The change took place last night. To-day was rather warmer than yesterday. At night Barometer falling 30·29. *Cumuli* in the sky by moonlight.

Oct. 5.—Clear and *cumuli*, warmer than yesterday; and clear

* The Barometer is generally high with easterly and northerly winds, even under the same circumstances which with southerly and westerly winds would depress the mercury, namely, wind and rain.

night,

night, and a little mist near the ground. Therm. 11 P.M. 38°. Barom. fell to 30.00. Wind E. by N. then S. and SE. Calm.

Oct. 6.—*Stratus* early, then misty with *cumuli*, which thickened, increased; mixed features of many modifications appeared, and rain came in the evening. Wind gentle and variable; fair night again.

Oct. 7.—Fine clear morning with NW wind, and small fleecy *cumuli*; the clouds increased, and *nimbus* formed towards three in the afternoon with a shower.

Oct. 8.—Clear cool morning, with N. wind; afterwards clouds with ill-defined margins, both *cumuli* and a sort of clumsy *cirrostratus*; by noon the weather altered for the better, with well-defined *cumuli* with NW breeze, which blew gently all night. At 11 P.M. Therm. 40. Barom. 30·05.

Oct. 9.—Clear cold day with *cumuli*, &c.; very cold clear night. Thermometer below 30°. N.NW.

Oct. 10.—Clear morning; I discerned large white bars of *cirrostratus*; the night became partially cloudy, and warmer.

Oct. 11.—Warmer with southerly wind, and sky clouded with hazy atmosphere. The ground dry.

Oct. 12*.—Clouds, with a little small rain in drops; with southerly wind and hazy. The wind at night became rather high from SE. At 11 P.M. Therm. 55°†. Barom. 29·71.

Oct. 13.—Fair, and various clouds.

Oct. 14.—Warm cloudy morning; fair day with much *cirrus*, *cirrocumulus*, and features of *cirrostratus* variously mixed. Ground dry.

Oct. 15.—Warm, chiefly clouded and hazy, with gentle showers from SW; but the ground soon dried. Windy night. Thermometer at 11 56°.

Oct. 16.—Fair NW wind and diurnal *cumuli*. Thermometer 11 P.M. 44°.

Oct. 17.—Warm, with gentle showers.

Oct. 18.—(Observations made at Bookham.)—Rainy all day, except a few hours in the evening. W.

Oct. 19.—Rainy, and long showers with fair intervals all day.

Clapton,
Oct. 19, 1814.

THOMAS FORSTER.

* I noticed a bat flying about to-day at 2 P.M. in Oxford-street; which is rather an unusual occurrence, both as to place and time of year.

† By the Thermometer understand according to Fahrenheit's scale.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For October 1814.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather
Sept.	27	56	64	55	29·70	27	Showery
	28	56	63	51	·87	34	Cloudy
	29	52	59	50	·98	30	Cloudy
	30	56	58	50	30·05	40	Fair
Oct.	1	50	55	49	·09	42	Fair
	2	51	57	50	·13	46	Fair
	3	48	59	50	·17	51	Fair
	4	50	58	49	·23	57	Fair
	5	50	59	48	29·98	56	Fair
	6	40	56	49	·79	41	Cloudy
	7	43	52	40	·80	33	Showery
	8	40	55	42	30·05	40	Fair
	9	40	41	38	·08	42	Fair
	10	32	50	41	·09	40	Cloudy
	11	44	54	49	29·99	40	Cloudy
	12	49	56	54	·70	36	Cloudy
	13	55	58	55	·52	30	Cloudy
	14	56	65	52	·40	36	Fair
	15	51	59	50	·62	47	Fair
	16	47	57	50	·67	40	Fair
	17	48	56	49	·54	34	Cloudy
	18	47	52	47	·31	0	Rain
	19	48	51	45	·12	0	Rain
	20	45	52	41	·50	33	Fair
	21	39	54	50	·78	30	Fair
	22	54	57	50	·76	33	Fair
	23	50	50	39	·68	37	Fair
	24	38	48	46	·45	35	Fair
	25	47	47	40	·18	0	Rain
	26	41	49	45	·65	18	Cloudy

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

LIII. *On the Production of Aërolites, and other Solids, by the Union of aëriform Fluids.*

To Mr. Tilloch.

Sir,—The ingenious manner in which M. Marcel de Serres endeavours to throw some light on the formation or production of aërolites*, &c. induces me to make some remarks on the production of certain solids by the union of aëriform fluids or gases, and which still further induces me to think that in this branch of chemistry there are many discoveries yet to be made. We know that most vegetable as well as animal substances contain proportions of metals, as well as earths which are now known to be of metallic basis. M. Berthollet is said to have obtained gold from the ashes of vegetables; and this will not appear improbable to us, when we consider that gold is almost as general a metal as iron, though in such very minute quantities. We know also that iron enters into the composition of almost all of the vegetable kingdom, and this undeniable experiments have proved that it does from the atmosphere. Are we then to suppose that they enter in the form of acidulous vapours containing the metal or earth in solution, or by the union of certain gases, by which means other solids of a meaner consistency are undoubtedly formed? The ingenious botanist Mrs. Ibbetson has proved that certain plants are so constructed by nature as to accommodate themselves to an unnatural soil, by certain alterations in their vessels, which enable them to inhale moisture, or more likely the gases by which the aqueous portion is formed: and indeed we know that all plants inhale and exhale, thereby forming the whole of the aqueous, mucilaginous, as well as the more solid parts, by the destruction or decomposition and recomposition of various gases; and which has been proved by trees being suffered to attain double the weight of the earth in which they grew, while the latter was not in the least decreased. If it is argued to be from the water by which they were fed, it may be again said, that water is merely a compound of gases.

It appears to me, that the principal solid constituent of vegetables may be thus formed. Since we know that moisture is necessary to all vegetables, we know that by some means or other it enters its parts; and as experiment teaches us that most of the resinous, mucilaginous, and gelatinous substances, which all plants possess, may be decomposed into carburetted hydro-

* Phil. Mag. vol. xliii. p. 253.

gen, we must suppose, that leaves of all vegetables inhale a great proportion of carbonic acid gas, which coming in contact with the water of the plant, unites with the hydrogen, forming gums, resins, or other mucilaginous matter; which continuing to imbibe a greater proportion of carbon, becomes concrete, forming the wood; while the oxygen from the carbonic acid gas and water is exhaled by the plant, and greedily absorbed by the atmosphere; and hence may be the source of the oxygen which is known to appear on the surface of leaves when put under water.

Since we know that our atmosphere is of too adhesive a composition to admit of any foreign gases uniting with it in free space, we must naturally suppose they are forcibly repelled, and, whether compound or simple, either from their density remain on the surface of the earth; or the hydrogen not absorbed by vegetables may unite with other bodies for which it has an affinity, dissolving a portion of them, and then, by its levity, may ascend to the higher regions, carrying with it, in its ascent other gases also holding bodies in solution. Thus different constituents of solid bodies may congregate in the higher regions, where their particles uniting by attraction, by electricity, or by powers unknown to us, may form precipitable matter of greater density than the atmosphere.

Considering these circumstances, and the ocular proof which we daily have of solid substances being formed by the union of gases, why should we question how such as honey dews, gelatinous showers, &c. can be produced in the upper regions? And as we have many instances of decomposed animal and vegetable substances being converted into pyrites, undoubtedly from the absorption of sulphur and iron, in many instances, from the atmosphere, why can we not suppose this composition to take place above, from some substance of an inferior solidity, but perhaps equally capable of attracting particles of matter for the formation of aërolites, which, until having attained a certain degree of density greater than that of the surrounding medium, cannot be precipitated to the earth?

As I have before observed, iron is one of the most general metallic substances in nature, and I think there is little doubt but it is this which gives the almost universal green colorification to those parts of vegetables which are formed by nature to imbibe the gases, from the atmosphere, necessary for the constitution of the plant, as I have proved that in those leaves where the colour is greatest there is the largest proportion of iron. This indeed, we may observe by the fallen leaves of trees, &c. that where the colour has been most predominant while living, when dead, the

the leaves assume the deepest yellow; and when very dry, a dirty light red, which is undoubtedly the iron in the state of an oxide. But I in vain endeavoured, by watering different species of plants with a very dilute solution of iron in different acids, to obtain a greater degree of colour in the leaf; and I further made this experiment on flowers and such plants whose leaves were not of a direct green, and with different metals in solution; but was not able without the destruction of the plant to alter the colour, nor did the plants when submitted to a rough analysis appear to be the least altered in respect to their composition.

This was, perhaps, the delusive chain of the alchemists: but if followed up by some of your scientific readers in these more enlightened times, where actual experiment is unbiassed by superstition, it might throw much light on what, from being familiar to us, is in a great measure overlooked as a common phænomenon.

I am, sir, respectfully,

Your constant reader,

Stoke Newington. P. N.-J.

LIV. *On the Affections of Light transmitted through crystallized Bodies.* By DAVID BREWSTER, *LL.D. F.R.S. Edin. and F.S.A. Edin. In a Letter to Sir* HUMPHRY DAVY, *LL.D. F.R.S.*

[Concluded from p. 270.]

V. *On the elliptical coloured Rings produced by depolarizing Crystals.*

In a former work, to which I have already had occasion to refer, I have given some account of the colours which accompany the depolarization of light, and I have particularly noticed the remarkable fact, that when a beam of white depolarized light is transmitted through a doubly refracting crystal, the red rays go to the formation of one image, while the blueish-green rays go to the formation of the other image. In repeating and extending these experiments, I have been led into a new field of inquiry, which has already afforded a series of instructive results deduced from a class of phænomena unquestionably the most brilliant within the whole range of optics.

The plate of topaz which was used in these experiments, is about $\frac{103}{1000}$ of an inch thick, and has two natural faces which are parallel and highly polished. Its refractive power is 1·636; its dispersive power 0·024, and the angle at which it polarizes light by reflection 58° 8′. It is represented in section by AB*ab*, in Plate IV., fig. 8, DE being one of its depolarizing axes. If a beam of *common light* RR′ is now incident on the anterior surface AB at an angle of about 60° 38′, a part of the beam will

 penetrate

penetrate the topaz at R, and after reaching the posterior surface *ab*, it will be partly transmitted at C in the direction CF, and partly reflected in the direction C*r*, so as to depart from the point C almost wholly polarized by reflection; but in its passage from C to *r* along the oblique depolarizing axis of the crystal, it is depolarized and emerges at *r*, in the direction *rr′* deprived of the polarity which it had acquired by reflection at C. If the observer now looks into the topaz in the direction *r′r*, through a plate of agate having its laminæ perpendicular to the plane of the section AB*ab*, he will perceive about ten brilliantly coloured elliptical rings, four of which, with the two central spots, are shown in Plate VI. fig. 1.*

The following measures will convey a correct idea of their form and magnitude.

Breadth of the central spots including half the black space between them	1°	51′
Distance of the outsides of the central spots	3	42
Transverse length of each central spot	5	7
Extreme conjugate diameter of *first* red ring	7	24
Ditto second	11	6
Ditto third	14	48
Ditto fourth	18	30
Ditto fifth	22	12
Ditto sixth	25	54
Ditto seventh	29	36
Ditto eighth	33	18
Ditto ninth	37	0
Ditto tenth	40	42
Black space between the oval centres		14⅓

In order to convey a correct notion of the different colours which compose the elliptical rings, and which vary in different parts of the same ring, I have given in Plate VII.†, fig. 1, an outline of the first *six* rings with references to the following table, which contains the colours in five different parts of the semi-circumference of each ring.

Oval central spots.
1. *Light blue* with a purplish tinge fading into *white* above, and gradually deepening into *black* below.
2. *White* fading into yellow above, and *light blue* below.
3. *Yellow* shading off into *white* below, and red above.
4. Red, with a pink tinge, and shading into *yellow* below.

I. Order.
5. *Black* fading into light blue towards 6.
6. *Light blue* fading into *green*.
7. *Yellow* shading into *red*.
8. Deep crimson.

II. Order.
9. *Blue*, very little.
10. *Green*, very little, the green beginning a little below.
11. *Yellow* shading into red.
12. Crimson.

* I have counted fourteen of these rings when the light was polarized by oblique transmission through a plate of mica 0·127th of an inch thick. The colours are in this case much more distinct.

† This Plate will appear in our next Number.

III. Order.
13. Light blue, very little.
14. Green, very broad.
15. Crimson fainter than 12.

IV. Order.
16. Green, very broad.
17. Faint blue.
18. Faint crimson.

V. Order.
19. Very faint blue.
20. Very faint crimson.

VI. Order.
21. Still fainter blue.
22. Still fainter crimson.

I. Order.
23. Black shading off into light blue.
24. Dark green.
25. Yellow.
26. Deep crimson.

II. Order.
27. Blue.
28. Green.
29. Crimson.

III. Order.
30. Blueish-green.
31. Crimson.

IV. Order.
32. Very faint blue. Green begins here.
33. Very faint pink.

V. Order.
34. Still fainter blue.
35. Still fainter crimson.

I. Order.
36. Black mixed with a little green.
37. Ditto.
38. Ditto.
39. Dark green, a little yellow on its upper side.
40. Red, pinkish, not very bright.

II. Order.
41. Darkish green.
42. Faint crimson.

III. Order.
43. Green.
44. Blue, very little.
45. Faint crimson.

IV. Order.
46. Faint blue.
47. Faint crimson.

All the other fringes, without this, consist of blue and pink, which grow fainter as they recede from the centre.

I. Order.
a Black.
b Dark blue approaching to black.
c Light blue shading to black.
d Whitish.
e Reddish brown of an orange cast.

II. Order.
f Very dark blue.
g Light blue.
h Yellowish.
i Pink.

III. Order.
k Light blue.
l Yellowish green.
m Faint pink.

IV. Ord.
n Green, blue begins here and runs downward.
o Pink.

V. Order.
p Faint blue, green begins here and runs downward.
q Pink.

I. Order.
r Black.
s Faint blue shading into whitish.
t Whitish shading into faint brown.
u Faint reddish brown.

II. Order.
v Black.
w Dark blue shading into light blue.
x Brownish yellow.
y Dark pink, with a brown tinge.

III. Order.
z Pale blue shading into greenish yellow.
& Greenish yellow.
a' Pink.

IV. Order.
b' Blue not much.
c' Green.
d' Pink.

V. Order.
e' Green.
f' Light blue.
g' Faint pink.

VI. Order.
h' Faint blue.
i' Faint pink.

If the plate of agate is now turned round 90°, so that its laminæ are parallel to the plane of the section AB*ab*, a *second set* of elliptical rings will be seen as represented in Plate VI., fig. 2, which is on the same scale as fig. 1. and which contains only the four first orders of colours, and the central spots. This new set of rings is composed of colours which are *complementary* to those in the first set. By measuring the diameters of the *red* rings in the *second set*, it will be found that they correspond with those of the *green* rings in the *first set;* the *blue* rings correspond with the *yellow;* the *green* with the *red;* and the *yellow* with the *blue;* and in the outer rings the *blue* with the *pink*, and the *pink* with the *blue.* The central spots in the *second set* exhibit the same opposition of colours to those in the *first set;* but they are smaller, and placed at a greater distance; and the space around them which was formerly *black* is now *white.*

If instead of a plate of agate we employ a doubly refracting crystal, the *first set* of rings will, in one position of the crystal, be seen in the first image; and upon turning the crystal about its axis, the *first set* will occupy the second image, and the *second set* the first image, an alternation taking place in every quadrant of the motion of the crystal. This method of viewing the rings is in some respects superior to that in which the agate is used, as the nebulous image formed by this mineral injures, in some degree, the distinctness of the image; but on the other hand, the doubly refracting crystal requires to be cut into a prism with a large angle, in order to separate the two images which it forms; and therefore it alters the shape of the rings, and produces a complete change upon their colours*.

If the emergent rays *rr′*, instead of being transmitted through agate or Iceland spar, are reflected at the polarizing angle from any transparent body having its reflecting surface parallel to the plane of the section AB*ab*, they will exhibit the *first set* of rings; but if the reflecting surface is perpendicular to the plane of the section, the *second set* of rings will be visible. When the *first set*, thus seen by reflection, is examined through a prism of Iceland spar, it suffers no change either in the first or second image.

In these experiments the *first set* of rings is extremely distinct, as the polarizing crystal extinguishes the light RS reflected

* Since this paper was written, I have discovered a new property of light, in virtue of which it is polarized by oblique transmission through transparent bodies. Hence, in all my experiments on the coloured rings, I find it of incalculable advantage to polarize the light by bundles of glass plates, and to use them in every case where I formerly employed agate or calcareous spar.

from

from the first surface of the topaz; but the *second set* of rings is very faint, as the light RS is not extinguished by the polarizing body.

When we examine the transmitted light CF, either with the naked eye or with polarizing crystals, no coloured fringes are visible.

Such are the modifications which *common light* undergoes in its passage through topaz. The affections of *polarized light*, which now come under consideration, are still more varied and interesting. In my first experiments on this subject, I polarized the light by transmitting it through the agate; but I afterwards found it most convenient to communicate this property by reflection from the surface of a transparent body.

Let RR′, Plate IV., fig. 9, be a beam of polarized light obtained by reflection from any transparent body GH, the plane of reflection from GH being perpendicular to the plane of reflection from the topaz AB. A part of this beam will be reflected at C in the direction C*r*, and part of it transmitted at C in the direction CF, no light being reflected from the first surface AB. The rays transmitted at C having been polarized before their incidence at R′ are depolarized in passing from R′ to C along the oblique depolarizing axis, and the rays reflected at C are polarized by reflection from the surface *ab*, and again depolarized in their passage from C to *r* along the other oblique depolarizing axis.

If the observer now looks into the topaz in the direction *rr*′, he will perceive the *first* set of elliptical coloured rings, as represented in Plate VI., fig. 1. These rings are now peculiarly distinct and brilliant, and it was therefore from them that I drew up the table of colours referred to from Plate VII., fig. 1.

Let the ray *ri*′ be now received upon a plate of agate having its laminæ perpendicular to the section AB*ab*, and a *third set* of rings will be seen like those in Plate VI., fig. 3. This *third set* differs from the *first set* only in the central parts. All the rings have the same colours in both; but the central spots are much smaller in the third set than in the first, and the mass of darkness with which they are surrounded encroaches considerably upon the blue part of the first ring.

In the third set of rings the distance of the outsides of the two central spots is	3° 3′
Conjugate diameter of each spot	1 1
Ditto of the black space between the spots	1 0

The *third set*, indeed, may be considered as the exact counterpart of the *second* set, all the colours of the former being

 com-

complementary to those of the latter, and the central spots having the same form and magnitude.

If the plate of agate is now turned round, so that its laminæ are parallel to the section ABab, a *fourth set* of rings will be seen. This new set, which is represented in Plate VI., fig. 4, is by no means brilliant, but it is distinguished from all the rest by striking peculiarities. In its general structure it resembles the *first set*; but in the middle of each central spot there is a darker spot composed of blue and red, with a little green above the blue, and every ring is divided into two rings, each of which has the same colours as the original ring. This division of the rings occupies only a part of the semi-circumference of each, and is not seen beyond the third ring.

When the agate begins to move from the position which gives the *third set* of rings, into that which gives the *fourth set*, two blue spots and the divisions of the rings begin to appear at $a, a, a, a, a', a', a', a'$, Plate VII. fig. 2, and move along the lines abc, $a'b'c'$ till they arrive at $c, c, c, c, c', c', c', c'$, when the rings assume the appearance of the *fourth set*. If the agate performs another revolution of 90° from the position which gives the *fourth set* into that which gives the *third set*, the blue spots and the divisions of the rings move off in the direction c, d, e, c', d', e', till the rings assume the appearance of the *third set*.

The phænomena which have now been described may also be perceived, when the polarization of the rays rr' is effected either by a doubly refracting crystal or by reflection. In one position of the doubly refracting crystal the third set of rings is seen in the first image, and the fourth set in the second image, and they alternate in every quadrant of the motion of the crystal. When the ray rr' is reflected from a transparent body, so that the plane of reflection is parallel to the plane of reflection from the topaz, the *fourth set* of rings will be visible.

Hitherto we have attended only to the light reflected from ab, the posterior surface of the topaz; but the light transmitted at C exhibits also interesting phænomena. When the observer looks through the topaz in the direction FC, so as to see the polarizing surface GH, the *second set* of rings is faintly visible. They become extremely distinct, however, when viewed through a plate of agate having its laminæ at right angles to the plane ABab. If the laminæ are parallel to the plane ABab, the *second set* is converted into the *first set* with colours a little paler than when it was produced in the former experiments.

In the preceding experiments the plane of reflection from GH has been perpendicular to the plane of reflection from the topaz. We shall now describe the phænomena which take place when these

these planes are parallel to each other, an arrangement which is represented in Plate IV., fig. 10.

When the observer looks into the topaz in the direction rr', he will perceive the *second set* of fringes. If the rays rr' are transmitted through a plate of agate having its laminæ perpendicular to the plane of reflection, the *fourth set* of fringes will be seen, but they are very much fainter than they appeared in the former experiments. When the laminæ of the agate are parallel to the plane of reflection, the *second set* is faintly visible. The central spots are, however, rather larger than before, so that this set has the appearance of being the reverse of the *first* rather than of the *third* set.

When the light transmitted in the direction CF is seen by the naked eye, it exhibits the *first set* of rings. If it is examined through a plate of agate having its veins perpendicular to the plane of reflection, the *first set* is still visible; but when the agate is turned round 90° the *second set* is perceived.

All the preceding observations were made with a plate of topaz $\frac{103}{1000}$ of an inch thick. When the plate has a greater thickness the rings are much smaller, and when it has a less thickness the rings are extremely large, so that in very thin plates only a small portion of a ring can be perceived at once. We have already seen that with a plate $\frac{103}{1000}$ of an inch thick, the fourth red ring subtends angle of 18° 30′. With another plate $\frac{227}{1000}$ of an inch thick, the angle subtended by the same ring is 8° 24′. But since

$$\tfrac{227}{1000} : \tfrac{103}{1000} = 18^\circ\, 30' : 80^\circ\, 24\,;$$

it follows *that the conjugate diameters of the rings are inversely as the thickness of the plates.*

According to the Abbé Haüy, the angle formed by two of the primitive faces of the topaz is 124° 22′; and therefore the axes or longest diagonals of the primitive rectangular prism will form angles of 60° 31′ 15″* with a line perpendicular to the laminæ, a result which agrees so nearly with 60° 38′, the inclination of the axes of the coloured rings, that we can have no hesitation in concluding that *the axes of the coloured rings are coincident with the longest diagonals of the primitive rectangular prism.*

The rings which have now been described, as produced by *topaz*, I have discovered in *rock crystal*, *mica*, the *agate*, the *oriental ruby*, the *emerald*, *native hydrate of magnesia*, *amber*, *ice*, *sulphate of potash*, *tartrate of potash and soda*, *nitrate of potash*, *acetate of lead*, *acetate of lead melted and cooled*, *prussiate of potash*, *mother of pearl*, *bones of a cod*, *quill*, *the human*

* According to my own measurements, the angle is 123° 58′, which gives 60° 28′ 26″ for the inclination of the diagonals.

man nail, horn, tortoise shell, cornea of a fish, cornea of a cow, cornea of a man, spermaceti, Rupert's drops, gum Arabic, and *caoutchouc.*

1. *Rock crystal.* The only specimen of this mineral which I could obtain when I made the preceding experiments, was in the form of a double convex lens about $\frac{63}{1000}$ of an inch thick. It exhibited only segments of the coloured rings; but they were very large and brilliant, and afforded me the means of making a very interesting experiment with a plate of agate.

If a beam of common light is incident upon the neutral axis of this crystal, at such an angle that after reflection from its posterior surface it shall emerge in the direction of its oblique depolarizing axis, the light thus polarized by reflection and depolarized by transmision through the depolarizing axis will reach the eye in the state of white light. If this light is viewed through agate, one of the coloured segments, suppose *green,* will be distinctly visible; but if the agate is turned 90° round, the green colour will be converted into *red,* and in general the colour seen in one position of the agate will be complementary to that which is seen in the other position. When the light, however, is brilliant, another very singular phænomenon presents itself. If the bright image seen through the agate is *green,* the nebulous image, in which it is inclosed, will be *red;* and when the bright image is red, the nebulous image will be green, and in general the colour of the nebulous image will be always complementary to that of the bright image. If we employ a prism of Iceland spar to examine the depolarized light, the colour of the ordinary image is always complementary to that of the extraordinary image. We may therefore consider the preceding result as an *experimentum crucis,* which establishes the opinion respecting the structure of the agate, that has been given in another part of this paper.

2. *Mica.* The coloured rings are distinctly visible in mica, both when the light is transmitted perpendicularly through the plate, and when it is incident in the direction of its oblique depolarizing axis. The irregular structure of this mineral, however, and the impossibility of procuring laminæ with parallel and even surfaces, prevented me from investigating the phænomena of its coloured rings.

3. *Agate.* The only plate of agate in which I have observed the coloured rings, is cut in such a direction that it does not polarize the bright image. It possesses, however, the faculty of depolarization, and therefore must form two bright images, one of which lies immediately above the other. This plate is about $\frac{28}{1000}$ of an inch thick, contains no veins, and exhibits broad segments of coloured rings.

4. *Oriental*

4. *Oriental ruby.* This doubly refracting crystal affords beautiful rings, in which, owing to the colour of the mineral, the predominant colours are crimson, light blue, and blueish-green. The central spots were distinctly visible; and though the crystal was $\frac{12}{100}$ of an inch thick, the rings appeared to be larger than those given by topaz $\frac{103}{1000}$ of an inch thick.

5. *Emerald.* The coloured rings formed by this stone are principally blue and greenish yellow, the least refrangible rays being extinguished by the green colour of the mineral.

6. *Native hydrate of magnesia.* This mineral affords very distinct segments of coloured rings when the light is transmitted in a direction nearly perpendicular to the surface of the laminæ. Owing to the imperfect structure of the plates, I could not obtain a measure of the diameter of the rings.

7. *Amber.* As this substance possesses no crystalline form, and does not split into laminæ, I found it impossible to make any satisfactory experiments with it. The enormous breadth of its coloured rings was conspicuous in every specimen; but though I ground and polished more than twenty plates of it, I could not obtain one which exhibited any thing more than broad-coloured segments. With a parallelopiped of amber 0·566 of an inch long, 0·300 broad, and 0·367 deep, the coloured segments were visible in every direction in which the light was transmitted. They appeared most distinct through the thickness 0·367; and through the thickness 0·566 they were still so broad, that no more than one colour of each ring could be seen. In a piece of amber $\frac{6}{10}$ of an inch thick, the rings were broader than in a plate of topaz $\frac{1}{30}$ of an inch thick.

8. *Ice.* The difficulty of making experiments upon ice without melting it, the want of a crystalline form, and the impracticability of shaping into parallel plates, prevented me from obtaining any accurate results. The following experiments, however, will throw some light upon this subject.

A piece of ice $\frac{1}{10}$ of an inch thick gave rings much broader than those exhibited by a plate of topaz $\frac{1}{30}$ of an inch thick. The rings were also seen by the reflection of common light from the posterior surface of the ice, the light reflected from the anterior surface being extinguished by a prism of calcareous spar.

A piece of ice $\frac{10}{30}$ of an inch thick exhibited rings larger than those given by a plate of topaz $\frac{2}{30}$ of an inch thick. The breadth of one of the fringes shown by a plate of ice $\frac{94}{300}$ of an inch thick was nearly 5° 26′, which compared with the results already mentioned, gives $\frac{10.5}{300}$ for the thickness of a plate of topaz that would produce a fringe of the same magnitude. Hence the thicknesses of ice and topaz that give rings of equal size are as $\frac{94}{300}$ to $\frac{10.5}{300}$, or as 8·95 to 1, which is nearly the inverse ratio

of

of $(m-1)^3$ in ice to $(m'-1)^3$ in topaz, m and m' being the indices of refraction. If we take $m=1\cdot307$ and $m'=1\cdot636$, this ratio will be nearly as 8·9 to 1. In these experiments the two oval central spots were distinctly seen.

Light transmitted at an angle of 46° through a plate of ice 1·25 inches thick gives rings of the same size as when it is transmitted at an angle of 60° 38′ through a plate of topaz $\frac{103}{1000}$ of an inch thick. By calculating the real thicknesses in the direction of the transmitted light, it will be found that the thicknesses at which ice and topaz produce rings of the same magnitude are as 8·4 to 1, a ratio not very remote from that of $(m-1)^3$ in ice to $(m'-1)^3$ in topaz.

Light transmitted at an incidence of 36° through a plate of ice $\frac{68}{100}$ of an inch thick gave rings twice as large as those shown by a plate of topaz $\frac{103}{1000}$ of an in thick. These thicknesses will be found, after reduction, to be as 8·2 to 1, a ratio more remote than any of the former from that of $(m-1)^3$ to $(m'-1)^3$.

A plate of ice taken from the surface of a pool of water did not appear to depolarize light, when it was incident perpendicularly: but when the angle of incidence was considerable, the light was depolarized in every direction, and the coloured rings appeared even at great obliquities.

9. *Sulphate of potash.* A plate of sulphate of potash $\frac{135}{1000}$ of an inch thick gave fringes of colour, each of which was 4° in breadth, while another plate $\frac{168}{1000}$ of an inch thick gave fringes 3° 12′ in breadth. Now

$$135 : 168 = 3^\circ\ 12' : 4^\circ \text{ nearly,}$$

so that the diameters of the rings are inversely as the thicknesses of the plates, as in the case of topaz. The light was incident on the sulphate of potash at an angle of 41°, which gives $\frac{179}{1000}$ for the oblique thickness of the plate $\frac{135}{1000}$ of an inch thick. Now

$$\frac{179}{1000} : 1^\circ\ 51' = \frac{209}{1000} : 2^\circ\ 2',$$

the size of the ring that would have been produced by a plate of topaz $\frac{179}{1000}$ of an inch thick, so that the thicknesses of sulphate of potash and topaz that produce equal rings are as 1·85 to 1, which is not very far from the ratio of $(m-1)^3$ to $(m'-1)^3$. If we take $m=1\cdot509$ and $m'=1\cdot636$, this ratio will be as 1·95 to 1.

10. *Tartrate of potash and soda.* The neutral axes of this salt are parallel and perpendicular to the axis of the prism, and it possesses an oblique depolarizing axis along which the coloured rings are visible. The thicknesses of this substance and of topaz, at which equal rings are produced, are as 31 : 16, which

which is almost exactly the ratio of $(m-1)^3$ to $(m'-1)^3$. The value of m' in the tartrate being 1·515.

11. *Nitrate of potash.* This salt, which is remarkable for its optical properties*, exhibits along the axis of the hexaedral prism a series of beautiful miniature rings, *twelve* of which are distinctly visible. In a plate of the nitrate of potash $\frac{17}{100}$ of an inch thick, the fourth ring subtended an angle of 5° 45′, whereas, in a plate of topaz $\frac{461}{1000}$ of an inch thick, it subtended an angle of 8° 25′. But

$$5° \, 45' : 8° \, 25' = \tfrac{461}{1000} : \tfrac{675}{1000}$$

the thickness of topaz that would give the fourth ring a diameter of 5° 45′. Hence the thicknesses at which the nitrate of potash and topaz produce rings of equal magnitude are as $\frac{170}{1000}$ to $\frac{675}{1000}$, or as 1 to 3·97 nearly. But assuming the rings to vary as $(m-1)^3$, those formed by the nitrate should have been larger than those exhibited by the topaz in the ratio of $636^3 : 515^3$, or nearly 1·88 to 1. Hence the rings formed by nitrate of potash are $1·88 \times 3·97$, or 7·5 times smaller than they should be if their conjugate diameters had varied as $(m-1)^3$ †.

12. *Acetate of lead.* This doubly refracting crystal melts at a temperature not much greater than that which bees' wax requires, and it takes a long time to cool and crystallize. When it is formed by heat into a thin film between two plates, the crystals shoot from different centres, and exhibit by polarized light the most beautiful alternations of the prismatic colours. When the eye is kept at a distance from the plate, the colours radiate like the spicula of the salt, and vary at every inclination of the plate.

13. *Mother of pearl.* The coloured rings are extremely brilliant in this substance when the polarized light is transmitted almost perpendicularly; but they do not appear when it penetrates by an oblique path.

The other substances, which have already been mentioned as affording coloured rings by polarized light, exhibit only imperfect segments of the fringes; but in all of them these segments are distinctly visible, excepting in caoutchouc, where the colours are extremely faint.

It is highly probable that the coloured rings will be found in

* I have endeavoured to give a full account of these in the Transactions of the Royal Society of Edinburgh, vol. vii. part ii.

† The thickness of the plates of ice, sulphate of potash, and nitrate of potash, and the inclination of the incident pencil, were measured in the rudest manner, as my object was merely to ascertain in general if there was any connection between the magnitude of the coloured rings and the refractive power of the body which produced them.

a still

a still greater number of crystallized bodies. I have sought for them in vain in the diamond, native orpiment, Iceland spar, fluor spar, muriate of soda, carbonate of lead, carbonate of barytes, the sclerotic coat of the eye, the crystalline lens, and a great variety of other bodies, and in some of these with so much care, that they could scarcely have escaped my notice, if they did exist. It therefore still remains to be determined, what kind of crystallization is necessary to their production, and what relation exists between the magnitude of the rings and the refractive power of the body which produces them. In some of the experiments already described, the diameters of the rings seem to vary as $(m-1)^3$; but the anomalies exhibited by amber and nitrate of potash completely prove that this is not the law by which their magnitude is regulated.

I have the honour to be, dear sir,

Your most obedient humble servant,

David Brewster.

To Sir Humphry Davy, LL.D., &c. &c.

LV. *Reflections on the Inadequacy of the principal Hypotheses to account for the Phænomena of Electricity.*

[Abridged from a Memoir read to the Kirwanian Society April 1st, 15th; May 13th, and June 17th, 1812.]

By M. Donovan, *Esq. Sec.**

The discoveries of late years, made in chemistry by the agency of electricity have given increased interest to electrical investigations. It has been supposed that Galvanic phænomena are produced by electricity; and in order to explain them, recourse has been had to the hypotheses of Franklin and others. Since then this agent is considered to have so great a share in the operations of nature, it is of some importance that the explanations given of its effects should stand the test of reasoning and of experiment. It were indeed to be lamented, that the interesting series of phænomena called electro-chemical should depend for explanation on principles not founded in fact. Yet such I apprehend is the case. I have occasionally indulged myself in reflections on the theories of electricity, and I conceived that there is not one of them capable of sustaining even a superficial examination. As my objections are for the most part different from those which have hitherto been stated by various inquirers, I thought that to make them known might, perhaps, have some useful tendency.

* Communicated in its present form by the Author.

Amidst

Amidst the great variety of hypotheses that have been proposed to explain the phænomena of electricity, two only are deserving of particular attention. In that which bears the name of Franklin, it is conceived that the supposition of one fluid only is necessary: in the other, two are presumed to operate.

Previously to the consideration of Franklin's hypothesis, it will be necessary to state the main facts, upon which its principles are founded.

Phænomena.

1. Some bodies after certain processes, as friction, heating, &c. are found to be possessed of peculiar properties. By certain management the power which occasions these properties may be transferred to distant matter. In its passage it often becomes sensible to all the external senses.

2. This power when acting upon bodies at liberty of motion will cause them to approach or recede from each other according to circumstances.

3. Bodies differ from each other in their habitudes with regard to this power; some possessing as a primary property the power of producing the peculiar effects above mentioned. Others possess it as a secondary property, derived by some action exerted on them by the former class.

These two classes of bodies differently affect the motions of the power. The former class offers considerable resistance: the latter, none that can be appreciated.

4. This peculiar power is capable of producing its effects in different degrees of intensity. In a low degree it produces the effects (as 2): in a high degree, it is attended with heat and light, and occasions violent effects on the animal œconomy.

The principles brought forward by Dr. Franklin in explanation of these phænomena are partly hypothetical and partly theoretical. For the sake of distinction, I will in the following statement apply the term *assumed* to the principles of the former kind, and *inferred* to those of the latter: and those which are of a mixed nature, theoretical in their character, but ultimately referring to an hypothesis, shall be designated by the term *deduced.*

1. From phænomenon 1st, it is inferred that the power producing the preceding effects is a material substance *sui generis.*

2. As bodies acted upon in the same manner will always recede from each other (phænom. 2), and as bodies not so acted upon remain stationary, it appears that it is the electrical matter which causes the recedence; and that the ultimate particles of this electrical matter have separately and collectively a mutually

tually repulsive power. Hence it is inferred that electricity is an elastic fluid.

3. Concerning the place of this fluid in creation, it is assumed that every kind of matter contains electricity, and that a mutual attraction exists between the containing and contained matter.

4. Since it is inferred that the particles of the fluid are repulsive of each other, it appears that there must exist such a state as an equilibrium: for, as each particle repels all others surrounding with equal force, it follows that all the particles will find a state of rest when the forces are balanced.

5. In the preceding principle it is inferred that an equilibrium does exist. From this and principle 3d it is deduced, that the equilibrium once found will be diffused throughout all bodies, and that all bodies must contain as much of the fluid as its particles will admit.

6. It is assumed that this equilibrium may be disturbed, the absolute quantity remaining the same; an excess above the common standard being found in one part, and a corresponding diminution in the other. Or it may be disturbed in different bodies. Hence it is deduced that

7. *a*. In bodies containing an excess, the fluid being not equally resisted possesses great elasticity. A body already containing its natural quantity will admit no more; consequently the excess remains on the outside, and forms an atmosphere. Such bodies, as they move with their atmosphere, must repel each other. Bodies in the state of diminution must have some particles of their substance vacant; these particles will be attracted by the fluid surrounding them in the air: and such bodies will seem to repel each other by virtue of divellent attractions. A body with an excess must attract one in the natural state, to restore the equilibrium; and when this happens, both having still an excess, they will repel each other. Lastly, a body with a diminished quantity will attract one in the natural state to restore the equilibrium; and when this happens, both having now a diminished quantity, will seem to repel each other as before. Thus does the hypothesis account for all the phænomena of attraction and repulsion.

8. From phænomenon 3, it appears that glass and other electrics afford a resistance to the passage of electricity. Hence it is assumed that electrics are absolutely impermeable to the electric fluid. From this and from hypothesis 6, it is deduced, that if the equilibrium of an electric be disturbed, one part will contain an excess, and another part a corresponding diminution. Yet the eagerness of the excess to give, and that of the vacant particles of matter to receive, are forces operating to restore the equilibrium.

equilibrium. This cannot take place through the glass: but when the connection is made between the different states, by a proper substance, both are brought into equilibrium, with the production of light, heat, and violent effects on the animal œconomy, as in phænomenon 4.

9. Since the equilibrium between two bodies my be disturbed, and since friction is one of the causes that disturbs the equilibrium, it is assumed that electrics, when rubbed, suffer an enlargement of their pores: that the excess enters the increased capacity, and that when the attrition ceases, it is expelled by the closing of the pores to their ordinary dimensions.

The whole of the foregoing doctrines may be reduced to the five following propositions, and in this order it is, for convenience, proposed to examine them.

1. The power producing electrical appearances is an elastic matter, *sui generis*.

2. It attracts all matter, and is found equally distributed in all matter of the same kind. To these two propositions are to be referred the phænomena of attraction, repulsion, and equilibria.

3. Certain substances called electrics are impermeable to the electric fluid.

4. The disturbance of the equilibrium may be effected by a separation of the natural quantity into excess and diminution. This may happen in different parts of the same body, or in different bodies.

5. When this separation is effected by attrition between different bodies, one of which must be an electric, the pores of the latter being opened, will receive the plus quantity, and will give it out again when the pores close. To the latter three propositions are to be referred condensation and excitation.

1. With regard to the first proposition that electricity is matter, it is a conclusion which any one who judges from facts would be induced to draw. It passes from one body to another in a visible form. It produces such a concussion on the solid matter of the air as to cause a loud explosion. It is well known to perforate bodies which even offer considerable resistance: so a quire of paper will be pierced with as much ease as it would by a pistol ball. If passed through a tube of oil or water, it will split the tube into minute fragments: and if the fluid had been ink, it would have been dispersed with such force as not to sully a sheet of paper placed underneath. That any material agent affects these appearances has been called in question by Sir H. Davy. When folds of paper are perforated by an electrical discharge, there is a burr raised on both sides, and this Sir H. Davy considers as an evidence that nothing passed through,

as, if it did, it could only, he observes, penetrate in one direction. To conclude: because the passage is effected in two different directions, that nothing passed, appears to me an inference of a very questionable nature. Sir H. Davy attributes the phænomenon to attractions acting in opposite directions. But were this so, a battery discharged through a quantity of light powder should collect it round the wires when the circuit was interrupted; whereas it is dispersed in all directions. On the whole it is not likely that the opinion of Sir H. Davy will become general; it therefore need not be more minutely discussed.

That the electric matter is an *elastic fluid* is a position that does not appear to be sufficiently supported. The experiments which are supposed to countenance this principle are two, along with some others which do not deserve much consideration. First, bodies which beside their natural quantity contain an excess of electricity, in a free state, are found to repel each other; the repulsion being attributed to the particles of the fluid acting on each other. Secondly, the electric matter when issuing from a pointed body in open air, appears like a diverging cone of rays, which Dr. Franklin thinks must be caused by self repulsion.

The evidence in favour of both these opinions is very insufficient. In the former case, there is no certainty that there is an excess, and in the latter, proof is wanted that electric matter is then *issuing* from the point. With regard to the repulsion of bodies in similar states, it certainly takes place in many experiments. I have, however, observed an instance in which no repulsion, but an attraction takes place.

Let a pith ball be suspended from a glass rod, and let an excited tube be brought under it, but so as not to touch. If the tube be now moved a little to one side from under the ball, the latter follows the tube for a short distance, but as the suspended ball in moving describes a curve, its gravity at length overcomes the attraction of the tube, it separates a little, and having now received the state of the tube, it is immediately repelled to a great distance. When the tube is again approached, the ball is still repelled: but when the tube is forced still nearer, the ball is attracted, and continues to be so until the tube being removed as at first, it separates by its weight and is immediately repelled as before. The same effects may be produced by any other electric. The results may at first be doubtful, but by repetition they will clearly show that in this instance the electric matter causes either attraction or repulsion as the distance is small or great.

As to the diverging rays proceeding from a point, their evidence

dence entirely depends on the supposition that electricity is then issuing out. But this itself is no more than an assumption, therefore, the fact proves nothing: and even were the direction of the fluid admitted, it would still afford no evidence. For if the divergence be occasioned by the repulsion of the particles as an essential property, it is evident that the same effects must be produced in all cases where the fluid is at liberty. Where a property of matter acts on similar matter only, the effects cannot be dependant on any other body. Now whatever the divergence may be in air, which resists the electric matter, it must be much greater in a medium which offers little or no resistance, if the divergence depend on an essential property of the particles themselves. But quite contrary is the fact, for it is well known that electricity, during its passage through a long exhausted receiver, passes with scarcely any appearance of divergence: parallel rays traverse from one end to the other. The moment that air is let in, the divergence appears; evidently proving that it is the medium and not a property of the electric matter which produces the effect.

Thus it appears that these two facts do not bear out the supposition of elasticity which has been founded on them. I do not pretend to say that the electric matter is not elastic; it may, or it may not: I see no more evidence in favour of one opinion than the other. Yet on this principle rest the doctrines concerning the repulsion of similarly electrified bodies, the charging of jars, and equilibria.

2. The second principle is that electricity attracts all matter. All matter must then attract electricity, and therefore the latter is dispersed throughout all bodies. Franklin adds "we know the electrical fluid is *in* common matter, because we can pump it *out* by the globe or tube. We know that common matter has near as much as it can contain, because, when we add a little more to any portion of it the additional quantity does not enter, but forms an electric atmosphere. And we know that common matter has not (generally) more than it can contain; otherwise all loose portions of it would repel each other, as they constantly do when they have electric atmospheres*." This is proving one assertion by another which is no more than an hypothesis, and which does not even seem to be well founded as will presently appear. Had it been established that we do accumulate the fluid it might then be allowed that it is pumped out. That common matter has as much electricity as it can contain, he proves by the formation of atmospheres: but we may as well suppose with Æpinus and others, that electrical atmospheres do not exist.

* Franklin's Letters, 56.

Concerning the equal distribution of electricity which follows from the first and second principle, Dr. Franklin observes "that when a quantity of electrical matter is applied to a mass of any bigness or length within our observation (which hath not already got its quantity) it is immediately and equally diffused throughout the whole *." It is supposed that insensible electricity exists naturally in the pores of all bodies, that it can be abstracted and forced to exist in an insulated form, while the body remains vacant. Let us conceive a body either totally or in part deprived of its share of natural electricity, "when a quantity of electrical matter is applied, it is immediately and equally diffused through the whole." Now if the body in question be of glass thus negatively electrified, and if so much sensible electricity be imparted as was abstracted; it must be equally diffused as before. But it must be remembered, that the hypothesis supposes glass to be impermeable to electricity; "let it first be considered that we cannot by any means we are acquainted with force the electric fluid through glass †," nor into any part of it, for Dr. Franklin after grinding away 5-6ths of a phial found it no more pervious than before, demonstrating that it was not the middle stratum alone of the glass, as he first supposed, which refused a passage to the fluid. I am aware that he does not allow a similar electricity to exist simultaneously in all parts of the glass: it is enough if he allow that any part may be negatively electrified: for in this case the fluid must have left that part, which it could not do were glass impermeable: and if one part allow a passage, so must every other. Thus the doctrine of equal distribution is on all sides surrounded by difficulties; it is incompatible with the doctrine of impermeability: if one be admitted the other must be rejected.

The attraction and repulsion of light bodies comes under examination in this place. Dr. Franklin conceives that all matter is full of the electric fluid, and that a mutual attraction subsists between both. Now if matter can contain no more it is because it has no further attraction: for from the nature of an equilibrium all attractions must be saturated. This conclusion is confirmed by his statement that "if more be added, it lies without upon the surface, and forms what we call an electric atmosphere," which could not happen unless the attraction of the body to more were destroyed. From all this it follows that attraction is exerted between matter and its natural quantity only of electricity, but in the ordinary state of things this attraction is insensible. Facts, however, convince us that a sensible attraction exists between free electricity and matter which already

* Franklin's Letters, 55. † Ibid. 75.

contains

contains its natural proportion. And as this natural proportion is really an affirmative and abstractible quantity, why does not another affirmative quantity produce repulsion and not attraction? For an experiment to prove that, according to the hypothesis, a repulsion between natural and affirmative quantities does exist, we need only refer to Dr. Franklin himself. "Hang a silk tassel on the prime conductor, and bring an excited tube under the tassel, and the threads will diverge. Because part of their natural quantity is thereby driven out of them, and they become negatively electrified, and therefore repel each other*." From this he deduces the following principle; "an electric atmosphere not only repels another electric atmosphere, but will also repel the electric matter contained in the substance of a body approaching it; and, without joining or mixing with it, force it to other parts of the body that contained it†." How these conclusions can be reconciled to the fact, that a body in the natural state is attracted by an excited electric, is difficult to determine.

This experiment of the tassel leads the Franklinians into another difficulty. They have applied the attraction of electricity for matter to account for why bodies when repelled move with their atmospheres. "If two bodies be both of them overcharged, the electric atmospheres repel each other, and both the bodies recede from one another to places where the fluid is less dense. For as there is supposed to be a mutual attraction between all bodies and the electric fluid, electrified bodies go along with their atmospheres ‡." There *may* be an attraction between matter and its natural quantity, but Dr. Franklin's experiment with the tassel professes to show that there is a repulsion between an affirmative atmosphere and the natural quantity. Hence the surrounding media containing their natural portion should not permit the recedence of the balls.

In the present state of knowledge we suppose that an elastic fluid is an assemblage of material particles surrounded by atmospheres of repulsion. The co-existence of such particles must produce a state of uniform density which is called an equilibrium. In the idea of an equilibrium repulsion alone is supposed to operate, attraction must entirely be excluded. Hence if by any means a number of particles how great soever of the fluid can be accumulated in any body, that body must repel another which may contain a number however few. This exactly agrees with Franklin's conception of the case: he maintains that quantities of fluid with any difference whatever of density repel each other§. This principle is productive of a train of

* Franklin, 160. † Ibid. 155. ‡ Priestley's Hist. 56.
§ Franklin, 407.

contradictions; and nothing can be so effectually subversive of his whole system as the consequences which flow from it. It will be necessary to trace but a few of those instances in which facts are at variance with this principle. A body slightly positive, and one strongly so repel each other; the powers differing in density. A body slightly negative repels one strongly so, the powers also differing in density. A body positive differs from one negative only in density, and yet they attract each other violently. Suppose the interval between the minimum and maximum of density to be divided into seven terms, and the medium term 4 to be the natural density of all bodies: 1 and 2 repel, 2 and 3 repel, but 3 and 4 attract; 4 and 5 attract, but 5 and 6, or 6 and 7 repel. Again, 7 violently repels 5 with the difference 2, but when the difference increased by 1 more (total difference 3), a violent attraction takes place. From all this it is plain that the phænomena of attraction and repulsion are completely at variance with the theory: and from what follows this will appear still more strikingly.

To explain why a body positively electrified attracts one in the negative state, recourse is had to that principle which supposes the attraction of electricity to all matter. For the portions of matter which are deprived of their natural quantity must be attracted by any body which contains an excess: we shall see how far the explanation is applicable.

From the nature of an equilibrium it must be supposed, as has already been observed, that every particle of electricity is attracted by a certain number of particles of matter in a given body: let us say one to one. Franklin conceives that a strong power will repel the natural quantity in any body. If then a body A contain 10 particles of matter attracted by 10 of electricity, that body is in the natural state. If a body B containing 10 particles of matter, and 20 of electricity be brought near A, it ought to happen, from the principle in question, that a repulsion would take place. If one particle of electricity be taken from A, one particle of matter is left vacant, which must be attracted by B. But the latter attraction is exerted towards one particle only of matter; the repulsion is exerted towards 9; the bodies consequently still repel: and no attraction can take place until the vacant particles exceed in number those which possess their natural quantity of electricity. Such are the consequences which ought to happen, were the hypotheses well founded: it is plain how much they disagree with facts. Experiment shows that a body however slightly negative, will be attracted by one that is positive. In the foregoing example, if the body A in its natural state be presented to B with its increased power, no attraction ought to take place, as there is no vacant

vacant particle: or if a negative body be presented to a body in the natural state, there should be no attraction, yet in both cases the contrary is the fact in an eminent degree.

On this principle also of the attraction of electricity to matter has the repulsion of a negative body by another been explained. It is laid down by Franklin, that in this case when the bodies are partly deprived of their quantity, the vacant particles of matter are attracted by the denser fluid of the atmosphere, thus causing a divergence. But the surrounding air, like other matter, strongly attracts its quantity of electricity and the reacting attraction of the latter is exactly equal. From this it becomes probable, and it is allowed by Franklin in many parts of his works, that when electricity is present in the natural proportion only, it exerts no properties. How then can his other principle be reconciled, namely, that it is the natural portion of the surrounding media that causes the divellent attraction, or in other words the apparent repulsion.

But the divergence of the bodies shows that the natural quantity of the surrounding air has been abstracted, as otherwise these bodies in order to receive electricity need not diverge. This diminution of the natural proportion of the air constitutes a negatively electrified atmosphere. A ball surrounded by such an atmosphere may be conceived to lie in the central point of a circle. The attraction of the denser fluid in the air, beyond the negative atmosphere, is exerted on the ball: but as the attraction is equal on all sides, the ball remains stationary. Considering the extreme difficulty with which air* and other electrics part with their portion, it follows that if any sudden impulse be given to the ball which can impart the smallest eccentricity, the ball being no longer equally attracted, ought to fly to the nearest peripheral part of the negative atmosphere where the boundary of the denser fluid begins. For the motion of the ball is instantaneous, and the alteration of electricity in the air is very slow. It is almost unnecessary to remark, that this inferred result does not correspond with experiment.

If two pith balls suspended by a conducting thread hang in opposition, and if an excited stick of wax be brought over them they instantly separate by receiving the state of the wax. The first effect of the wax was to rarify the fluid natural to the balls; an attraction then commenced, between the balls and the contiguous stratum of air, and some time should elapse before this stratum would be deprived of its share. The succeeding strata should slowly part with their quantities, and the balls in diverging should occupy times proportional to the slowness of the

* Franklin, passim.

alteration produced on the air. But the fact is far otherwise: the divergence, amounting to several inches, being instantaneous when the wax touches the conductor from which the balls are suspended.

The imperfections of this part of the theory were not long concealed from the penetration of its ingenious inventor. He always mentioned his opinions with that hesitation and modesty which so eminently characterize the operations of superior minds. Cavallo, however, with a self-security which is ever apt to excite distrust, maintains*, that "by this hypothesis which is analogous to the other phænomena of nature, the electrical appearances are easily explained, and that there *is not a single experiment that seems to contradict it.*" He nevertheless was necessitated to give a different view of negative repulsion from Franklin's, and whoever examines it will probably consider it far less probable: he will find facts attempted to be proved by assumed principles which may either be denied or allowed, which rest for proof on the thing to be proved.

With regard to this long controverted speculative question, whether the recedence of negatively electrified bodies be occasioned by a mutual repulsion, or by the divellent attraction of the electricity naturally contained in the surrounding media, I I have made an experiment which, notwithstanding its simplicity, seems strongly to countenance the former of these propositions.

A filament stript from a downy feather was suffered to float in the air, and an excited stick of wax was approached. The filament was immediately attracted but soon after flew off to a great distance. By current of air the filament was carried off several yards from its former situation. The wax being suddenly approached within about 15 inches, the filament again flew off; and by following it with the electric it might be driven to any part. Upon this experiment we may reason as follows:

After the contact of the filament and wax, both must have been in the same state: the former flew off to where the fluid is supposed to have existed in a denser state. When it was carried to a distance of several yards, it must have entered a completely neutral atmosphere. When the wax was suddenly approached, it also must have passed into a neutral atmosphere; yet the filament instantly flew off. Must not this be a direct repulsion? It cannot reasonably be supposed, during the passage of the wax to the filament, that the former had absorbed all the natural fluid of the air: so quick an absorption from an electric, and by an electric could not take place; and even were this

* Complete Treatise, 107.

possible,

possible, the filament should have absorbed as much fluid as had been deficient in it, and so would be brought to the natural state.

If the foregoing reasonings be just, it would appear that the second principle of the theory does not accord with the phænomena which it is intended to explain, and that all arguments founded on it are contradictory to facts and amongst themselves. The third principle is next to be considered.

3. The impermeability of glass is a position which seems incompatible with the doctrine of plus and minus. If a plane of glass be made negative it should remain so for ever if electricity cannot return *into* it. But the previous question occurs, How did the natural portion get out, if glass refuse a passage to it? This perhaps cannot be answered without a multiplicity of hypotheses.

As to the fact, whether or not glass is *actually* impermeable, many experiments have been made, but they all appear to be of doubtful force, and may be explained in some manner without supposing that electricity passed through. I am not aware that this objection can apply to the following.

Having procured a sphere of very thin glass about 2¼ inches diameter, furnished with a neck 9 inches long, I coated its lower hemisphere outside with tin-foil, and poured into it mercury almost as high as the coating. A moveable wire proceeding from the conductor of an electric machine dipped down into the mercury. The flask was charged through this wire, the latter was withdrawn, and the neck was sealed, by a blow-pipe, so far as barely to admit the passage of the wire. I now found that the flask was as capable of giving a shock as before, proving that the heat used in sealing did not draw out the charge. The flask was again strongly charged, and its neck totally sealed. After an immersion of 14 days in water, the neck was cut off, the glass was completely dried, and a wire was pushed down into the mercury. A shock not exceeding 1-20th of the force of the original charge was the result: which shows that 19-20ths must have left the glass. The experiment was repeated with numerous precautions: the sphere was exposed to the air for six months: and at the end of this time not a trace of electricity could be detected when the neck was cut.

Dr. Franklin had made some trials with a similar object in view: but he was led to a conclusion directly the reverse of mine. Perhaps the greater thickness of the glass in his experiment, may reconcile the results.

Although it appears that glass is in the strict sense of the word, permeable to electricity, yet the fluid passes through it with so much difficulty and so slowly that Franklin's position might

might be admitted. But by admitting it, as much injury is done to the hypothesis as by denying it. In either case numerous incompatibilities with the other principles and with facts occur, as has been and will further be seen.

4. Under the examination of the fourth principle we have to consider the doctrine of plus and minus electricity. Franklin states that in the charging of a Leyden phial the following changes take place. Suppose that on each surface of the glass are 20 particles, constituting the natural portion of electricity, and that one surface will receive as much as the other loses and no more. If 1 be added to the inside it will have 21, and the outside only 19: and when the outside has lost all, the inside will have 40. Thus the phial when charged contains no more than when not charged, except that the quantity in the latter case contained on both surfaces, is in the former case, condensed upon one. The equilibrium cannot be obtained through the glass: but when a conducting communication is established between both surfaces, the excess of the one is eagerly received by the other, and the equilibrium is restored with violence. Each surface now contains its natural 20 particles and every thing remains in the same state as before charging.

The whole may be comprised in the three following positions.

1. Electrics part with or receive no more electricity at one surface than they can receive or part with at the other.

2. When an electric is *charged*, one surface contains the quantity of fluid which in the natural state is diffused on both; the other side is consequently vacant.

3. After the *discharge* the equilibrium is restored, and both surfaces cease to show electrical appearances, being precisely in the same state as before charging. With regard to the first position, experiments do not seem to afford much support. Mr. Wilson, having rubbed one surface of a large plane of glass in the middle with his finger, found both surfaces electrified positively. If the rubbed side had been previously roughed, both surfaces became negative. To these cases may be added the following. Let a solid rod of rough glass be excited with a cat's-skin rubber; the rod will be negatively electrified all round. Nevertheless it is probably a truth that one state of electricity cannot be called into action without the other's existence, at least, in some other body: but we must distinguish facts from hypotheses.

Relating to the second position, namely, that of accumulation and deficiency, I have made experiments which seem to oppose this doctrine. According to Franklin a complete charge in a jar is obtained when its inside surface (if charged through the ball) contains its own natural quantity with that of the outside. In this case the outside is in an absolute state of deficiency, and

as

as eager to receive as the inside is to give out. During the progress of charging, both surfaces ought to manifest their characteristic properties in proportion as the charging increases. Whether or not this is the case will be seen by the following experiments. The apparatus employed was an insulated jar from the stem of which projected a wire at right angles, and from the outside coating another, two pith-balls being attached to each by means of gilt thread.

The jar, perfectly dry, is to be placed on the insulating stand, and a chain is to be connected with the outside, so as to form a communication with the ground. The electric machine being in strong action, is made to pour in a stream of sparks until the jar be fully charged. The chain being removed, the balls connected with the inner surface will be repelled by excited sulphur, and those with the outer surface will be attracted. If the inside be now positive, why are the balls repelled by sulphur? and if the outer surface be negative, why are its balls attracted?

If the jar be weakly charged, the results are as described by Franklin, and it was no doubt on such that this philosopher made his observations. But were the doctrine of accumulation and deficiency well founded, the results should be the more decided as the jar is more strongly charged. Yet when the hypothesis supposes the jar to contain the greatest quantity of electricity, it is then that it evinces symptoms of containing the least.

The error does not relate merely to the surface occupied by the accumulation. Let the jar with its wires, balls, &c. be set on the stand as before, but not connected with the ground. Let the jar be charged*, and while the cylinder is still turning let the surfaces be examined: it will be found that excited sulphur repels the balls of both the inside and outside. This cannot be explained while it is supposed that the outside is then giving out electricity, urged by the repulsion of the fluid thrown in: for in this case if any body with a diminished quantity, as sulphur, be presented, an attraction ought to follow. When the charging is discontinued, another militating phænomenon occurs: the balls collapse, and after touching, again separate, but with a different power, for they are now attracted by excited sulphur.

Here then are strong instances which oppose the Franklinian hypothesis; it appears that the supposed excess may evince the symptoms of a state of diminution without any abstraction having taken place; and the diminution may appear to be an excess without any addition. Perhaps there are on the whole no grounds

* The wire on the outside acting as a point or as a communication with the ground, suffers the charge to take place.

for

for supposing such states as plus and minus: they are scarcely applicable to any facts, and they perplex some; the following which I accidentally observed, affords an instance. A pair of gilt pith-balls are suspended by gilt strings from a brass insulated socket. When a stick of excited sulphur is brought in contact with the socket, the balls diverge negatively. If the contact of the sulphur and socket be continued without stirring the sulphur, the balls will nevertheless in some time come together. If now the sulphur be suddenly withdrawn, the balls will separate to perhaps a greater distance than before; but they have diverged with a contrary state of electricity, they are now positive*. The first effect of the sulphur was, according to the Franklinians, to deprive the balls and surrounding air of their natural electricity, the balls then separated being negatively electrified. At length still retaining the negative state, they come together, the sulphur is then removed, and at the time when the balls and air have lost even their natural portion of electricity, they manifest the appearance of an excess!

With regard to the position that the Leyden-phial is under the same circumstances after the discharge as before charging, the following facts afford some doubts. Beccaria charged two glass plates, each coated on one side only, and applied them closely together with the uncoated sides facing: the whole resembling one coated charged plate. When a communication was formed between both surfaces, an explosion took place, but the plates cohered with great force.

I coated a glass plate on one side, within an inch of the edge, thus leaving an uncoated margin all round. The uncoated side opposite the foil was rubbed with an amalgamated leather. When the glass was well excited, I placed a corresponding metallic coating, by its glass handle, on the surface of the plate, and, by making my finger and thumb the circuit between the surfaces, I received a slight shock. By touching the metallic coating and raising it by its glass handle, a large spark was obtained: and by continuing this process, a considerable number of sparks were drawn, which for a long time seemed to increase in strength. The same appearance but in a less degree may be produced by charging the plate from an electric machine, and afterwards using it as an electrophorus. Do not these experiments indicate that after the discharge, the natural state was not resumed?

From all the preceding considerations it would appear, that

* This experiment requires attention, and will not succeed in all states of the weather. The same may be said of some of the foregoing experiments. If two balls do not succeed one will in a less striking manner answer. Its change of state may be ascertained in the usual manner.

positive

positive electricity is not an accumulation, nor negative a deficiency of this peculiar matter: that jars in order to become charged, need not assume the characters of the two states: and that when discharged the electrical appearances do not cease.

5. The fifth and last position relates to the excitation of electrics. Franklin's supposition is, that "glass, a body extremely elastic, must when rubbed have its rubbed surface somewhat stretched, or its solid parts drawn a little further asunder, so that the vacancies in which the electric fluid resides, become larger, affording room for more of that fluid, which is immediately attracted into it from the cushion or hand rubbing, they being supplied from the common stock. But the instant the parts so opened and filled, have passed the friction, they close again, and force the additional quantity out on the surface*."

This statement appears very objectionable. Granting that glass when rubbed may have the interstices between its particles enlarged, why should electricity rush into them? If any do enter it must be at the expense of the rubber. But the rubber itself has a strong attraction to its natural quantity; it should be left negative, and the glass would be positive; therefore, as there is so strong an attraction between such states, the equilibrium would be instantly restored. There is not even any reason why the equilibrium should have been disturbed: the repulsion is on every side equal, and Franklin allows that under such circumstances, there can be no disturbance. If the pores of the glass be opened, it does not follow that they should have a tendency to take in more electricity. There are much more efficient modes of increasing the distance of the particles, for instance by heating, and yet this is an operation seldom connected with electrical appearances.

Franklin supposes that no electricity can be received upon one surface of glass unless the other can part with an equal quantity. In the case of the excitation of the common cylinder, the inner surface having no connection with conductors can part with none: how then can its outer surface receive the great quantities that we find on it?

In the excitation of an electrophorus, the vacancies by rubbing may be enlarged: but when the rubbing ceases, they should close and expel all electricity. So contrary is this to fact, that by one excitation seven or eight hundred sparks may be obtained. But what is still more irreconcilable is that while the sulphur cake and metallic plate are in contact, if the latter be gently passed off along the surface of the sulphur, scarcely another spark can be obtained.

* Franklin, 78.

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It is unnecessary to enter more fully into the discussion of a position so discordant with phænomena and so limited in its application.

Such is a statement and short examination of the system laid down by Franklin to explain the phænomena of electricity: a system which from its novelty, ingenuity, and from the modesty of its illustrious inventor, was received with admiration by most philosophers of that day. The charms of novelty are now no more, the catalogue of facts is increased, neither prepossession nor prejudice are likely at this distance of time to influence judgement: the imperfections of the hypothesis may therefore be fairly appreciated. And yet this hypothesis is always referred to as the basis of the doctrines of Galvanism. In the present dignified and improved state of natural science every thing should be rejected without reserve, without respect to authority, that deviates from the standard of reason and experiment.

[To be continued.]

LVI. *New Outlines of Chemical Philosophy.* By Ez. Walker, *Esq. of Lynn, Norfolk.*

[Continued from p. 274.]

On Evaporation, Hail, Rain, Snow, and Dew.

It has been ascertained by observation that the mean annual quantity of rain is greatest at the equator, and decreases gradually as we approach the poles. Thus at

Granada*, Antilles, 12° N. lat. it is 126 inches.

Cape François *, St. Domingo	19°	46′	120 inches.
Calcutta †	22	23	81
Rome ‡	41	54	39
England §	33	00	32
Petersburg ‖	59	16	16

Hence it appears that the quantity of rain is influenced by the heat of the climate. But as the sun's rays have no heat in themselves, what is the reason that they generate heat upon the surface of our earth.

To solve this important question it is necessary to premise, that no combustion nor increase of temperature can be produced, unless thermogen and photogen be united to ponderable matter.

* Cotte, *Jour. de Phys.* Oct. 1791, p. 264.
† Asiatic Researches, 1 and 2 Appendix.
‡ Cotte, *Jour. de Phys.* Oct. 1791, p. 264.
§ Phil. Trans. ‖ Edin. Trans. ii. p. 244.

Now

Now it is evident from common observation, that the sun's rays have the power of increasing the temperature of bodies; hence it was supposed that the sun was the fountain of heat. But this hypothesis is now found to be erroneous, for the sun's rays have *no heat* in themselves*, but they possess the power of acting upon the elements of heat and light, thermogen and photogen, contained in bodies so as to increase their temperature.

When the thermogen and photogen which lie dormant in matter upon the earth's surface, are acted upon by the sun's rays, those elements attract each other, and as they have a strong attraction for moisture †, a chemical union takes place between them and the water upon the surface of the earth, and two new gases are formed. Photogen and water form hydrogen gas, which ascends to the upper regions of the atmosphere, far above the altitude to which any balloon can ascend. Hence the reason that no hydrogen gas has been found in the atmosphere near the earth's surface.

The new oxygen gas, formed at the same time by the chemical union of thermogen and moisture, being much heavier than the other gas, because it contains more moisture, rises to a much less altitude. Hence it follows that all the moisture which is carried off from the earth's surface is converted into oxygen and hydrogen gases, consequently the atmosphere contains a much larger portion of those gases at one time than at another, and therefore its weight is variable, as shown by the barometer.

When a Leyden jar is highly charged, the two elements which it contains attract each other, and a spark is produced with a loud report. In like manner, when the thermogen in the lower regions of the atmosphere attracts the photogen from the upper, the same phænomena take place, called thunder and lightning. The only difference between the operations of nature to restore the lost equilibrium, and the effects observed in our experiments, consists in magnitude not in principle.

Thunder, Lightning, and other Meteors arising from the Composition and Decomposition of the Air.

When oxygen and hydrogen gases are mixed together, and a single electric spark introduced, combustion instantly takes place; and as the portion of the gases thus mixed together in the atmosphere may extend over a circle of some miles in diameter, the lightning may be the aggregate of the elements of combustion contained in that space.

* Phil. Mag. vol. xlii. p. 370. † Phil. Mag. vol. xliii. p. 252.

Water

Water being 914 times heavier than an equal bulk of atmospheric air, it will be easily conceived, that when the oxygen and hydrogen gases, contained within a circle of many miles in diameter, are reduced into water, an immense vacuum must be instantaneously formed; and, consequently, the air rushing into it must produce a violent concussion, accompanied with a tremendous report.

Hence the reason that the wind blows towards a thunder cloud; and a fresh portion of oxygen gas being thus mixed with the hydrogen gas which remains unconsumed, a second report succeeds the first; and thus thunder will be continued as long as any hydrogen gas remains unconsumed.

The extent of the influence of the two elements upon each other to decompose the air, may be estimated, in some degree, by the breadth of the shower: not by its length, for that may be influenced by the wind.

The quicksilver falls in the barometer during a thunder storm, because the atmosphere is lighter, part of it having been converted into a heavy shower of rain; but when the shower is over and evaporation recommencing, the water being again converted into air, the atmosphere becomes heavier and the quicksilver rises. When the chemical union between water and the two elements of combustion is dissolved, those elements descend to the earth with the falling rain; hence the reason that the air becomes colder after a shower; and the reason that we have no thunder in a rainy season is this; the elements of combustion are conducted back again to the earth, as fast as they ascend into the atmosphere.

If the mean annual quantity of water which rises by evaporation be equal to the mean annual quantity of rain, there must be ten feet six inches of water converted into atmospheric air annually, in the lat. of 12° N. and therefore it might be supposed that the barometer would rise higher and its range be greater at the equator than in higher latitudes: but it is now known from experience, that the range of the barometer is least at the equator and increases as we approach the poles. This seeming contradiction, however, will vanish after the following extracts have been duly considered.

"The mean height of the barometer at the level of the sea, all over the globe, is 30 inches, the weight of the atmosphere, therefore, is the same all over the globe. The weight of the atmosphere depends on its density and height: where the density of the atmosphere is greatest, its height must be the least; and on the contrary, where its density is least, its height must be the greatest. The height of the atmosphere, therefore, must be greatest at the equator and least at the poles; and it must decrease

decrease gradually between the equator and the poles, so that its surface will resemble two inclined planes meeting above the equator, their highest part.

"A current of air is constantly ascending at the equator, and part of it at least reaches and continues in the higher parts of the atmosphere. From the fluidity of air, it is evident that it cannot accumulate above the equator, but must roll down the inclined plane, which the upper surface of the atmosphere assumes, towards the pole.

"As the heat in the torrid zone never differs much, the density, and consequently the height of the atmosphere will not vary much. Hence the range of the barometer within the tropics is comparatively small; and it increases gradually as we approach the poles, because the difference of the temperature, and consequently of the density of the atmosphere, increases with the latitude."—Thomson's Chemistry, vol. iv. p. 46.

It is abundantly confirmed by experience, that when the chemical union between those elements and moisture is dissolved, a portion of the air is reduced into its component parts, thermogen, photogen, and water, which return again to the earth, either in flashes of lightning, or in showers of rain, hail, or snow.

Metallic rods reaching above the tops of buildings are known to conduct electricity from the air to the earth; and indeed, almost every elevated object is a conductor when the air is highly charged with those elements; whence we may infer, that in a thunder-storm, a person is much safer in a town, than upon an open plain. I am the more inclined to believe the truth of this remark from some phænomena which came under my own observation.

I was, some years ago, riding along the sea-shore in a very dark night, when it lightned very much at sea, with distant thunder. A very heavy shower came on where I was, which produced illuminations far more brilliant than can be well described.

The small end of my whip, and the tips of the ears of my horse, were ornamented with small lamps or globes of fire: every other little projecting point about his head was ornamented in the same manner; and his mane was almost one entire blaze of light. I attempted several times to extinguish these lights by striking them with my whip, but without producing any permanent effect; for no sooner was the water struck off from the parts which were illuminated, than these lights were regenerated by the succeeding drops of rain. These illuminations continued as long as the shower lasted, which might be about a quarter of an hour.

 This

This shower came from the sea; it was about low water, and I was riding near the water's edge, consequently there were no high objects to attract this element of combustion before it reached me.

It will be easily understood how these lights were produced from the well known properties of electricity. The drops of rain, being highly charged with one of the elements of combustion, falling upon objects connected with the earth, the other element was attracted through those objects, and uniting upon the most elevated points, a slow combustion took place, producing the illuminations above described.

Heat always increases the attraction between the two elements of combustion; but a contrary temperature produces a contrary effect.

In the north polar regions, where the sun never shines during some months, the attraction between the thermogen of the air and the photogen in the earth being suspended, no heat is generated; hence the most intense cold prevails, till the rays of the sun revisit those latitudes.

And in our winter, the cold air from the north carries off the heat from the surface of the earth, and as the sun's rays seldom reach us at that season of the year, the weather becomes extremely cold, for the reasons mentioned above.

But when the wind blows from the torrid zone, being of a higher temperature than that from the north, attraction takes place between the thermogen, which it contains, and the photogen of the earth, the temperature of the air is soon increased; and if the earth be covered with snow, it is either reduced into water, or converted into atmospheric air. And vegetation under the snow is always found in a forward state, for the thermogen and photogen having attracted each other through the snow, the temperature of the earth is increased above freezing, although the snow still remains at that point.

This hypothesis will receive some support from the following extract taken from my journal.

"March 17, 1814. The snow which began to fall about the 6th instant has been wasting away for some days, mostly by evaporation. I have observed for the last two or three days, little holes in the snow in the garden, round the plants and sticks which appear above it. These holes are wide at the top and narrow at the bottom, reaching to the ground. Query—May not these sticks and plants conduct the photogen of the earth into the atmosphere, by which means so much heat is generated as to convert the snow into atmospheric air; or, to speak in more familiar terms, to cause evaporation?

"To solve this query by experiment, I took three rods; one of of wood, another of metal, and a third of glass. These were stuck through the snow in the garden into the ground, at proper distances from each other. Two days afterwards, I examined these rods, and found holes in the snow round the two first similar to those mentioned above; but there was not the least alteration in the snow round the glass rod, glass being a nonconductor of those elements."

But if this experiment be not sufficient to solve the above query, there are other well known phænomena, from which the same conclusion may be drawn.

Lynn, Nov. 9, 1814. EZ. WALKER.

[To be continued.]

LVII. *Further Experiments and Observations on Iodine. By Sir* H. DAVY, *LL.D. F.R.S. V.P.R.I.**

1. *On the triple Compounds containing Iodine and Oxygen.*

1. In this communication I shall have the honour of presenting to the Royal Society a continuation of the inquiries I have made respecting the chemical agencies of iodine, and the properties of certain of its compounds.

2. I described in my last paper the action of iodine on fixed alkaline lixivia, and the deflagrating salts it forms. In the first experiment which I made on these compounds, I employed the first crystals which fall down from moderately strong solutions of potassa and soda saturated with iodine, which had been purified by being repeatedly acted upon by distilled water: I now find that this process is not sufficient to free the triple compound from the double compound; and that to obtain them in a state of absolute purity, it is necessary to boil them repeatedly in small quantities of alcohol of specific gravity of from 8·6 to 9·2, which dissolves the double compound, but has little power of action on the triple compound.

The triple compounds, when purified, present some curious chemical phænomena, which a minute quantity of the double compound adhering to the crystals that I operated upon, prevented me from observing in the experiments I have already communicated to the Society. I shall describe these phænomena as they are produced by the triple compound of potassium, as this substance is most easily procured in considerable quantities, but as far as I have been able to observe, the phænomena presented by the compound of sodium are precisely analogous.

* From the Philosophical Transactions for 1814, part ii.

The triple compound of potassium purified by alcohol is almost tasteless, has no action on vegetable colours, is very little soluble in cold water, but more soluble in hot water; when it is thrown into concentrated nitric, or sulphuric, or phosphoric acids, it has no violent action on them. By heat it may be dissolved in them, and the solutions, when saturated, congeal and form crystalline substances intensely acid. When the substance formed by the triple compound and the nitric acid is strongly heated, the nitric acid flies off, and at the temperature at which it is entirely expelled, the substance itself begins to decompose and affords a little iodine and much oxygen.

If the solution of the triple compounds in sulphuric or phosphoric acids be heated strongly at the temperature at which the acids sublime, the triple compound itself is decomposed, and it affords oxygen and iodine, and leaves acid sulphate and phosphate of potassa. If when the mixture is rendered fluid by heat, a little sugar or other combustible matter is added, there is a violent action, and iodine is disengaged with great rapidity.

The triple compound dissolves without decomposition in solution of phosphorous acid; but on heating the solution, oxygen is attracted by it, iodine appears, and phosphate of potassa is formed.

When the triple compound is thrown into concentrated muriatic acid, there is an effervescence, the smell of chlorine is perceived, the fluid becomes yellow, and when evaporated yields the chlorionic acid.

When the solution of the hydroionic acid in water is poured upon the triple salt, iodine is instantly produced in great quantities.

Acetic and oxalic acids dissolve the triple compound without decomposing it. On heating the solution in oxalic acid, the acid becomes brown from the deposition of charcoal, and iodine immediately appears.

When the triple compound is thrown into solution of sulphurous acid, iodine is instantly produced, and sulphuric acid formed, and if the sulphurous acid is not in too large a proportion, the solution becomes yellow by dissolving by iodine; if more sulphurous acid is added, water is decomposed, and sulphuric acid, and hydroionic acid formed.

The *double compound* of potassium and iodine has no action on oxalic, acetic, sulphurous, or phosphorous acids, but when it is mixed with the triple compound it is instantly decomposed by them, and iodine set free.

The same double compound in its pure state is decomposed very slowly by muriatic acid; and to convert the greater portion into muriate of potassa (potassane) it is necessary that the acid

acid should be very frequently distilled from it, and a part always remains unaltered; when mixtures of the triple and double compounds are exposed to the action of muriatic acid, potassane (muriate of potassa) is instantly formed; and if the proper proportions are adopted, none of the double or triple compounds remain, and the results are potassane only and the oxychloric acid.

Mixtures of the triple and double compounds produce abundance of iodine when acted on by glacial hydrophosphoric acid gas; but the pure double compound affords only hydroionic acid gas, and this decomposition offers the best method which has yet occurred to me of procuring pure hydroionic acid. When the two substances are gently heated together, the hydroionic acid gas, which comes over in considerable quantities, forms a colourless solution when absorbed by water.

I have endeavoured to ascertain the composition of the triple compound of potassium. Seven grains that had been dried at the temperature of boiling water heated to redness in a small crucible of platinum lost 2·2 grains. Seven grains heated to dull redness in a small tube of glass lost 1·7 grain; a minute portion of iodine condensed in the middle part of the tube, but no violet vapour was observed in the upper part of it, and there was a very slight appearance only of moisture, so that the loss of weight in this last experiment must be principally ascribed to the expulsion of oxygen.

On a comparison of the results of these two analyses, it appears very probable that this triple compound is composed of one proportion of iodine about 165, one of potassium 75, and six of oxygen 90; which is a composition exactly analogous to that of the hyper-oxymuriate of potassa. The quantities that I used in my experiments were too small to render these results more than approximations, yet the similarity of them to those presented by the hyper-oxymuriates ought perhaps to render them more worthy of confidence.

I have attempted to obtain pure triple compounds from solutions of baryta and lime, and from magnesia diffused through water, by dissolving iodine in them by heat, and by evaporating the clear liquor until it began to deposit crystals. In this way I have procured substances which, when well washed in distilled water, afforded no iodine to nitric acid, which yielded chlorine and chlorionic acid when acted upon by muriatic acid, and which when distilled afforded much oxygen and some iodine, and left substances, which appeared to be mixtures of the earths with compounds that afforded iodine to sulphuric acid, producing a smell of sulphurous acid gas, and which probably consisted of the metals of the earths united to iodine.

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The triple compounds from lime and magnesia were soluble without affording iodine in sulphuric acid; but on evaporating the acid, at the time that the vessel of platinum in which the experiment was made became dry and almost red hot, the violet vapour was perceived. Even the triple compound from baryta did not afford iodine or oxygen by treatment with sulphuric acid, except under the same circumstances.

3. When I first discovered that the triple compounds dissolved in acids without effervescence, I thought it probable that the effect depended upon the formation of a compound of oxygen and iodine, similar to euchlorine, or the oxy-chloric acid, and which remained dissolved in the fluid; and on this idea I made a number of experiments with the hope of obtaining such a combination in a detached form.

I distilled the solution of the triple compound of potassium in sulphuric acid, but the only gaseous product I obtained was oxygen. Sulphuric acid and iodine condensed in the cool part of the apparatus, and the residuum was acid sulphate of potassa.

Conceiving that a compound of oxygen and iodine might nevertheless exist in the fluid, and be decomposable at a high temperature, I attempted to obtain it by acting on the triple compound of barium by sulphuric acid, and by evaporating the fluid obtained at a gentle heat, and suffering it to cool at different periods of the process; but in this manner of operating I gained no better results.

The triple compound of barium is scarcely soluble in water. Water that had been boiled upon it gave only a slight cloudiness to sulphuric acid, which possibly might be owing to some double compound mixed with it: the fluid when evaporated nearly to dryness afforded fumes which had the characters of those of sulphuric acid, and by a red heat yielded iodine, and left sulphate of baryta.

When the solid triple compound of baryta was heated in very small quantities of diluted sulphuric acid, the fluid separated exhibited acid properties, and when gradually evaporated left a substance which congealed by cooling, and formed a solid of a yellow colour deliquescent in the air, strongly acid, and which reddened vegetable blues, and did not afterwards destroy them. When strongly heated, the substance afforded the same results as the substance procured from the fluid just mentioned.

The residual solid matter obtained by the action of sulphuric acid on the triple compound of barium was treated a second time with sulphuric acid, yet notwithstanding, when heated to redness, it yielded iodine in abundance.

I have repeated these experiments very often, because M. Guy Lussac has stated that an acid compound of oxygen and iodine may

may be procured by dropping sulphuric acid into a solution of the triple compound of barium; but the conclusions of this ingenious chemist seem to have been founded upon the want of effervescence in the process; and his experiments were made at a very early period of the investigation, and probably before this time he has found reason to alter his opinion.

It is probable that a binary compound of iodine and oxygen may be formed, but the facts presented by the action of acids on the triple compounds are not sufficient to prove its existence.

When small quantities of very diluted sulphuric acid are digested on the triple compounds of potassium and barium, the fluid obtained is always acid, and always precipitates muriate of baryta. I thought it possible that the compound of iodine and oxygen might possess this property; but on collecting the precipitate and examining it, it appeared to be a mixture of the triple compound and sulphate of baryta, and from all the facts it appears that in the action of acids on the triple compounds new combinations only are formed *.

I take the liberty of proposing for the triple compounds the names of oxy-iodes, because, when decomposed by heat, they afford oxygen and iodine. Individually they may be named from their bases. Thus *oxy-potassame,* or oxy-iode of potassium, will signify the triple compound of potassium, oxygen, and iodine, and *oxy-barame,* or oxy-iode of barium, will denote the triple compound of barium.

2. *Some Observations on Hydroionic Acid, and on the Compounds procured by means of it.*

1. I have generally procured the hydroionic acid which I have used in my experiments by the process referred to in the last section, the action of hydrophosphoric acid on potassame, but I have sometimes employed the gas procured from moistened iodine by phosphorus.

The hydroionic acid gas is rapidly decomposed by being heated in contact with oxygen, and a solution of iodine and hydroionic

* When sulphuric acid is made to dissolve as much of the triple compound of potassium by heat as possible, the mixture congeals by cooling into a yellow transparent substance, extremely deliquescent, and very acid. On decomposing it by heat, *neutral* sulphate of potassa remains. Now as oxygen and iodine are the only substances driven off by heat, it may be asserted that the acid property of the mixture depends upon these two principles; yet this conclusion does not follow according to sound chemical logic: iodine alone destroys the alkaline properties of potassa, and oxygen and iodine in combination with potassium form a difficultly soluble and almost tasteless substance. This substance, the triple compound, has only a weak attraction for sulphuric acid, and it might be expected that in combining with sulphuric acid it would not deprive it of its acid properties.

gas in water is formed, and it is slowly decomposed by heat alone, affording a deep red-brown easily fusible substance, which seems to be a compound of hydroionic gas and iodine.

When condensed in water, it is instantly decomposed by solution of nitric acid and iodine precipitated.

The solution of hydroionic acid rapidly absorbs oxygen from the air, and becomes yellow, and at last deep tawny orange; and this absorption is assisted by light and heat, the hydrogen is attracted by the oxygen to form water, and the iodine formed is dissolved in the remaining acid.

The concentrated hydroionic acid will probably form a good eudiometrical substance; it does not render the vessels in which it is used cloudy like the hydrosulphurets by the deposition of solid matter, and it does not enlarge the volume of the residual air like some other substances.

The solution of the hydroionic acid is decomposed by being heated with the hyperoxymuriate of potassa, and iodine is produced.

Hydroionic acid gas, as I have mentioned in my last paper, is decomposed by all the metals I have exposed it to, except gold and platinum; and the same metals that decompose it in its gaseous state, likewise decompose it when it is in solution, requiring, however, in some cases, the assistance of heat. The fluid hydroionic acid tarnishes silver at common temperatures, and dissolves mercury slowly when boiled in contact with it.

2. It dissolves the alkaline and common earths, and forms with them compounds very analogous in their properties to the compounds they produce when acted on by muriatic acid.

I heated dry quick-lime in a small tube filled with hydroionic acid gas, a yellow fluid immediately formed, which was coloured by dissolving hydroionic gas and iodine, and a fusible compound soluble in water, and which had a bitter taste similar to muriate of lime, was produced.

I made the same compound by dissolving marble in the hydroionic acid; the compound when heated to redness became fluid, and when kept in fusion in contact with air emitted iodine, gradually lost it fusibility, and from being neutral became alkaline, so that at a high temperature iodine is partly expelled from calcium by oxygen. I proved this still more distinctly by fusing the compound in a close vessel, in which it was confined by mercury. There was no change. I admitted a little oxypotassame, and caused it to give off oxygen by heating it: as soon as the calcareous compound was fused in contact with oxygen, it instantly emitted iodine, and lime was formed on the surface.

The compound formed from hydroionic acid and baryta is an acrid

acrid bitter substance, very similar in its taste to *barytane*, (fused muriate of baryta,) not decomposable when heated to whiteness unless oxygen is present, but when it is heated in contact with oxygen, oxygen is absorbed, and a part of its iodine expelled.

Magnesia dissolved in hydroionic acid without effervescence, and the solution evaporated gave a solid substance, having a taste very similar to muriate of magnesia. Like that salt, it partly lost its acid by a red heat; but a portion remained not decomposable out of the contact of air, but which instantly afforded iodine when heated in contact with oxygen.

I dissolved glucina, ittria, and zircona in the hydroionic acid; they formed neutral saline compounds. The compound of hydroionic acid and glucina was less soluble and more astringent in taste than the muriate of glucina, and was entirely decomposed when heated in the open air, affording hydroionic acid and iodine.

The compound formed from ittria was more soluble, and highly astringent; that formed from zircona astringent, with more of bitterness. Both these salts were decomposed when heated in the atmosphere, at a low red heat; a smell of hydroionic gas was perceived, iodine was produced, and the earths remained.

3. I mentioned, in a note dated Montpellier, Jan. 10, (containing a correction for my last communication to the Society,) that the alkaline property which I at first supposed to belong to the compounds of potassium and sodium with iodine, depended upon some undecomposed subcarbonate of potassa mixed with the hydrate of potassa I employed, as the subcarbonate of potassa is decomposed by iodine and carbonic acid set free, I had not thought it probable that the subcarbonate of potassa could interfere with this result. But I find that if the subcarbonate exist at all in the lixivium, a portion of it always remains undecomposed. I find likewise, that when a solution of iodine in lixivium of potassa is rendered perfectly neutral, or even slightly acid by hydroionic acid, a strong red heat renders the solid substance obtained slightly alkaline, provided it be in contact with air. Whether the separation of iodine by oxygen, in this instance, depends upon some effect of the moisture contained in the atmosphere, or upon the continued action of fresh portions of oxygen on the same surface of the compound, it is not easy to say; but a similar effect I find is produced upon *potassile*, (fluate of potassa;) this substance gains the power of reddening paper tinged with turmeric, by being strongly heated in contact with the air.

The power of neutralizing acids does not belong to the true compound

compound of iodine and potassium, but depends either upon the subcarbonate not decomposed, or upon the alkali formed during the ignition of the compound; the pure double compound seems to have no power of action on the acids it does not decompose; I fused it in contact with sulphurous acid gas confined by mercury in a glass tube, the salt gained a slight tint of yellow, but did not absorb its own volume of gas: after this, it slightly reddened litmus, so that the acid must have had little more than a mechanical adhesion to the salt.

When *potassame*, or iode of potassium, is fused with boracic acid, there is a perfect mixture of the two bodies. In my first researches on this mixture, I conceived that they entered into chemical union, and formed a violet-coloured glass, and that the acid property of the boracic acid was neutralized by the new compound; but I since find that the violet colour of the glass is owing to the development of iodine, and when the application of heat is long continued, much iodine is disengaged, and the colour of the glass changes to olive, and borate of potassa is formed. When the glass is dissolved in warm water, an olive-coloured power separates, soluble when boiled in the caustic alkalies, so that there is great reason to suppose that it is boron, and that the boracic acid is decomposed by the attraction of the potassium combined with the iodine for oxygen, assisted by the tendency of iodine to assume the elastic state.

I fused the neutral compound of iodine with silica; no change was effected when the experiment was made in close vessels, but when the mixture was exposed to air, and intensely heated, a little iodine was evolved, some potassa formed, and some silica dissolved by it.

3. *On other acid Compounds of Iodine.*

1. I have made several experiments on the combination of iodine and chlorine, obtained by admitting chlorine in excess to known quantities of iodine in vessels exhausted of air, and repeatedly heating the sublimate.

Operating in this way, I find that iodine absorbs less than one-third of its weight of chlorine.

The compound of iodine and chlorine is a very volatile substance, and in consequence of its action upon mercury, I have not been able to determine the elastic force of its vapour. Hence the estimations of its composition from experiments on the quantity of chlorine absorbed in close vessels must necessarily be liable to error. In one experiment, in which I dissolved the sublimate, by admitting a small quantity of water into the retort, I found that eight grains of iodine had caused the disappearance of five and a quarter cubical inches of chlorine.

In

In another experiment, in which the sublimate was not dissolved by water, and in which the absorption was judged of by the admission of fresh quantities of the gas, twenty grains caused the disappearance of 9·6 cubical inches of chlorine, the barometer being at 30·1, and the thermometer at 57° Fahrenheit.

It seems probable, from these experiments, that the chlorionic acid consists of one proportion of iodine and one of chlorine.

The chlorionic acid formed by the sublimation of iodine in chlorine in great excess is of a bright yellow colour, when fused it becomes of a deep orange, and when rendered elastic it forms a deep orange-coloured gas. It is capable of combining with much iodine when they are heated together, its colour becomes in consequence deeper, and the chlorionic acid and the iodine rise together in the elastic state.

The solution of the chlorionic acid in water likewise dissolves large quantities of iodine, so that it is possible to obtain a fluid containing very different proportions of iodine and chlorine.

The pure solution of the chlorionic acid, when it is very diluted, loses its colour by being agitated for some time in contact with chlorine, and then, when poured into fixed alkaline lixivia or solutions of the alkaline earths, it causes the precipitation of substances having the characters of triple compounds or the oxyiodes. If it is coloured, or in its ordinary state, at the same time that the oxyiode is precipitated, much iodine appears, and it is impossible to render a *concentrated* solution colourless by agitation with chlorine, or to deprive it of its power of yielding iodine by acting on alkaline solutions. The chlorionic acid, when poured into a solution of muriate of baryta, causes a copious precipitate in it, which has all the characters of oxyiode of barium, and the liquor becomes very acid.

When the colourless solution of chlorionic acid is added to a strong solution of ammonia, a white powder is precipitated which detonates feebly by a gentle heat; and which, when decomposed in glass vessels, affords iodine and elastic matter which does not support combustion.

When highly coloured chlorionic acid is employed, the powder that falls down is black, and detonates with much greater force, and by the slightest touch or motion, and it appears to be the same substance as that procured directly by the action of iodine on ammonia, and which I have shown to be a compound of azote and iodine. Whether the white powder is a similar substance containing a larger proportion of azote, or whether it is a compound of ammonia with oxygen and iodine, or with iodine and chlorine, I have not yet been able to determine.

It

It is soluble in excess of chlorionic acid, and in this way may be separated from the black powder; it affords a little moisture during its detonation, but it is not possible to say whether this is formed in the process, or whether it is water adhering to the compound, for the temperature of its decomposition is so low, that a proper degree of heat cannot be applied to render it dry.

When two bodies so similar in their characters and in the compounds they form, as iodine and chlorine, act upon substances at the same time, it is difficult to form a judgement of the different parts that they play in the new chemical arrangements produced. When I found that the chlorionic compound formed a strong acid by solution in water, I at first suspected that water was decomposed and hydroionic acid and euchlorine formed; there was no effervescence in the process, and the proportions agree the supposition; but I find that solution of euchlorine instantly decomposes hydroionic acid and precipitates iodine, which is afterwards redissolved by the chlorine set free; and nitric acid, which decomposes hydroionic acid, has no action on chlorionic acid.

It was possible likewise that if water was decomposed, muriatic acid and a compound of iodine and oxygen might exist in the solution; I endeavoured to ascertain if this was the case by distilling the solution at different temperatures and collecting the products, but I obtained always the same fluid.

When coloured solution of chlorionic acid is boiled with hyperoxymuriate of potassa, it loses its colour, and chlorine is given off from it; but in this case it likewise gradually loses its acidity, and a substance which yields iodine by heat with much effervescence, and which is probably oxypotassame, is precipitated.

It appears to me most probable that the acid property of the chlorionic compound depends upon the combination of the two bodies; and its action upon solutions of the alkalies and the earths may be easily explained, when it is considered that chlorine has a greater tendency than iodine to form double compounds with the metals, and that iodine has a greater tendency than chlorine to form triple compounds with oxygen and the metals.

When in the case of the action of the chlorionic compound on fixed alkaline lixivia the chlorine is not in great excess, much iodine is always set free, because as it is easy to perceive from the proportions in which they combine, there is not sufficient oxygen detached from the alkali by chlorine to form the triple compound; and if the estimation of the composition of oxypotassame given in the first section be accurate, supposing that none of the double compound of iodine is formed, a solution

must

must contain five proportions of chlorine to one of iodine, to produce a triple compound without the precipitation of iodine. It is however most probable that some double compound of iodine is always formed, as a solution must be extremely diluted indeed to contain five proportions of chlorine to one of iodine.

When the solution of chlorionic acid is poured into solution of muriate of baryta, water must be decomposed to furnish hydrogen to the muriatic acid, and oxygen to the triple compound, and in this case some double compound of iodine and barium must be formed and remain dissolved in the solution.

From the action of chlorionic acid on metallic solutions, I am inclined to believe that triple compounds of the common metals, oxygen and iodine may be formed by means of it. It occasions a copious precipitate without effervescence in the solution of sulphate of iron, and in the solution of nitro-muriate of lead, and tin, and of nitrate of copper, and from analogy it is probable that these precipitates consist of the metal, oxygen, and iodine.

2. I thought it probable from the rapid action of tin on iodine, that tin-foil would burn in the vapour of iodine, but on introducing it into the violet-coloured gas in a small retort made very hot, though the combination was instantaneous, yet no light was apparent.

I thought it possible that the acid properties of the compound of tin and iodine, which I have described in my last communication to the Society, might depend upon the decomposition of water and upon the formation of hydroionic acid. On this idea I distilled the solution of it in water, hoping if hydroionic acid were formed, that I should obtain some in this process; but the fluid that came over was merely water coloured by a minute quantity of iodine, and the orange-coloured substance which remained when dissolved in water, exhibited the same acid properties as before, and combined with ammonia without affording any oxide.

The compound of iodine and iron when dissolved in water exhibited acid properties, but when the solution was distilled it yielded hydroionic acid and deposited oxide of iron, and the entire solution acted on by ammonia, afforded an olive-coloured precipitate in great abundance.

4. *On the Action of some compound Gases on Iodine.*

1. I heated some iodine in a dry glass globe filled with sulphuretted hydrogen; there was a considerable absorption of gas, no sulphur was deposited, and a reddish-brown fluid was formed, which when thrown into water rendered it strongly acid and deposited

deposited much sulphur; the water passed through a filter exhibited the properties of hydroionic acid.

It is evident from this experiment, that sulphur, iodine, and hydrogen, are capable of forming a triple compound.

2. I sublimed some iodine in dry olefiant gas; a little of a reddish-brown fluid was formed, but the greatest part of the iodine crystallized on the sides of the vessel in which the experiment was made. By repeating the process several times, more of the fluid was formed. It was volatile at a moderate heat, and gave a yellow tint to water, but did not render it acid, there was a very slight absorption of the gas.

3. Iodine sublimed in nitrous gas effected no change in it.

4. When iodine was exposed to carbonic oxide it underwent no change, it was repeatedly sublimed in it in common day-light without undergoing the slightest alteration.

When the violet gas was formed by heating iodine in carbonic oxide, and the vessel exposed for some time to the direct solar rays, a small quantity of a limpid fluid which had an acrid taste formed in the vessel. An accident prevented me from ascertaining if any gas had been absorbed, but it seems probable from this result that, like chlorine, iodine may be combined with carbonic oxide by the agency of light.

5. *On the Mode of detecting Iodine in Combinations, and on certain Properties of its Compound with Sodium.*

1. I have examined many of the marine productions of the Mediterranean, with the view of determining whether they contained iodine. The ashes of the fuci and ulvæ of this sea afford it in much smaller quantities than the *sel de varec*, and in a very few cases only have I been able to obtain evidences of its existence in them.

2. M. Berard was so good as to order a considerable quantity of the species of ulva, which abounds on the coast of Languedoc, to be burnt for me at his laboratory at Montpellier. The ashes consisted for the most part of common salt, but a small quantity of alkaline lixivium which was obtained from them, afforded a red fluid when acted upon by sulphuric acid, and a similar colour I found was produced, when a solution of subcarbonate of soda and common salt, containing a minute quantity of the compound of sodium and iodine, was treated in the same manner by the acid.

3. One of the best tests of the presence of a very minute quantity of iodine in compounds, is their action upon silver. Water when it contains less than $\frac{1}{1000}$ part of its weight of the double or triple alkaline compounds of iodine tarnishes polished silver. The

The effect produced by compounds of iodine, may be distinguished from that produced by sulphurets or sulphuretted hydrogen by this circumstance, that solutions containing sulphurets or sulphuretted hydrogen, by being boiled with a little muriatic acid, no longer tarnish the metal, whereas solutions containing iodine still retain the power.

4. Amongst a number of sea weeds that were obligingly given me for examination, by Professor Viviani, of Genoa, the ashes of the following afforded slight indications of the presence of iodine,

Fucus cartilagineus.	Fucus filamentosus.
——— membranaceus.	Ulva pavonia.
——— rubens.	——— linza.

In the ashes of the corallines and sponges, I could discover no evidences of the presence of the substance.

5. I have examined three specimens of alkali formed by the combustion of vegetables that grow on the sea shore, one from Sicily, one from Spain, and the third from the Roman states, but not one of them afforded any indications of the presence of iodine.

6. I evaporated a considerable quantity of sea water procured at Sestri of Levanto in Liguria, in a part of the bay remote from any source of fresh water; but I could gain no unequivocal evidences of the presence of the compounds of iodine in it. The residual liquor after the common salt had been separated, did not act upon silver nor colour sulphuric acid. The first crystals of salt which fell down when fused upon silver, appeared to me to tarnish it more than the last; from which it appeared probable that they may have contained some triple compound of iodine, yet after being ignited, they did not colour sulphuric acid. When a large quantity of this water was electrized by a Voltaic apparatus, and the products separated at the positive pole collected in a small cup of gold, which was covered with cement, except in the interior and lower part forming the circuit, a yellow solution was obtained, which when it was exposed to the negative pole of a Voltaic apparatus, yielded a black powder fixed in the fire, and not unlike the compound formed by heating gold and iodine together; but the quantity was too minute to admit of analysis, and a dark-coloured substance is likewise obtained by negatively electrifying oxymuriate of gold, and there can be no doubt but that this substance formed a principal part of the solution*.

* Iodine, like chlorine, I find combines both with gold and platinum, when heated with them, or when they are exposed to them in its nascent state.

If

If iodine exists in sea water, which there is every reason to believe must be the case, though in extremely minute quantities, it is probably in triple union with oxygen and sodium, and in this case it must separate with the first crystals of common salt.

Whether the superiority which the curers of fish and meat are in the habit of attributing to bay-salt over rock-salt, is at all connected with the presence of the compounds of iodine, is an inquiry perhaps worth making, and the results of Dr. Henry's elaborate investigation of the composition of different kinds of salts, do not preclude the possibility of the circumstance, though they certainly diminish the probability.

I rubbed pieces of beef that had been killed some days, with the double and triple compounds of sodium. They did not putrify; the one rubbed with the double compound became very tender and soft, and of a red-brown colour; that exposed to the triple compound hardened considerably, and became of a paler colour.

The triple compound, as I have mentioned before, has very little taste, and neither of the compounds seems to have any pernicious quality when received into the stomach. I fed a goldfinch with bread soaked in water, holding in solution the double compound for two days, and he drank water holding in solution the triple compound for three days, without apparently suffering any inconvenience.

Florence, March 23, 1814.

LVIII. *On Gas Light, as far as it regards the Products obtainable from Pit-coal by this new Method of Illumination, and its œconomical Application.* By AMERICO CABRAL DE MELLO.

THE remarkable encouragement which has been afforded for some years past, both by the legislature* and the public, to the scheme of substituting the inflammable gas obtained during the distillation of pit-coal, instead of tallow and oil to illuminate houses, streets, and manufactories; and the whole parish of Shoreditch, Westminster-hall, the avenues to the House of Lords, and the House of Commons, together with several streets in Westminster, being now regularly illuminated with gas light, have induced various enterprising individuals to render their assistance

* An act has been passed by the legislature to incorporate a company by royal charter, under the name of The Gas Light and Coak Company, to apply the gas light illumination to the lighting of the metropolis.

in

in aiding this scheme, in procuring and distributing light, and to apply it to the purposes of domestic œconomy on a large scale, for lighting houses, streets, and factories.

The following remarks on the products of pit-coal, as far as they relate to this new mode of procuring and distributing light, it is therefore presumed, may be interesting to the public. The statement is copied literally from the printed evidence* given by Mr. Accum, on the 10th of May 1810, in the House of Lords, when examined on that subject.

" *Question.*—Can you state in what proportion these results are produced from a chaldron of coals?

" *Answer.*—From reiterated experiments that I have lately made I am enabled to say, that a hundred weight of Newcastle coals produces from two hundred and fifty, to three hundred cubic feet of gas; and with regard to the light that is obtained from the combustion of this quantity of gas I am enabled to state that nineteen cubic feet of the gas, if applied to the purpose of illumination, is equal to a pound of tallow candles; that is to say, if a tallow candle, six to the pound, be set up and lit, and if it be suffered to burn for an hour and weighed after that time, it will be found to have lost 180 grains; therefore 180 grains of tallow are necessarily consumed in the combustion of a candle during the time of one hour.—If I make a gas light of equal intensity to that of the tallow candle, I find that half a cubic foot of gas is requisite for the same period of time, and to give the intensity of light; therefore, from this statement it will become obvious that nineteen cubic feet of gas are equal to one pound of tallow candles, provided they were set up and burnt out one after another, that is to say, nineteen cubic feet of gas are demanded to give a quantity of light equal in duration of time and in illuminating power to one pound of tallow candles, six to the pound.—I have stated already that a hundred weight of coal produces from 250 to 300 cubic feet of gas; therefore from this statement *the value of gas light with regard to intensity and duration of time may be learned when compared to that of the light of candles.*

" A hundred pounds of coal produces from four to five pounds of tar upon an average; this tar is worth from 33*s.* to 36*s.* a barrel at present†.—A chaldron of coals produces sixty pounds of pitch, which is worth about three-pence halfpenny a

* Minutes of Evidence taken before the Lords' Committees, to whom was referred the Bill entituled, "An Act for enabling His Majesty to incorporate by Charter, a Company to be called The Gas Light and Coak Company, for making inflammable Air for Lighting of the Streets of the Metropolis," &c. Ordered to be printed May 22, 1810.

† May 10, 1810.

 pound;

pound;—and a chaldron of coals has produced to me thirty-two pounds of essential oil; this I could sell at sixteen guineas a hundred weight.—With regard to the quantity of asphaltum, from one chaldron of coals I obtained from twenty-eight to thirty-two pounds; this I could sell at 180*s*. the hundred weight. —The quantity of ammoniacal liquor amounts to one hundred and eighty pounds, that is, about eighteen gallons; and fourteen hundred weight of ammoniacal liquor produced half a hundred weight of carbonate of ammoniac, this sells at about sixteen guineas the hundred weight; and fourteen hundred weight of ammoniacal liquor, if it be converted into muriate of ammoniac, will produce a hundred weight of muriate of ammoniac, which is worth fourteen guineas.

"*Question*.—What do you reckon the price of a hundred weight of coals?

"*Answer*.—I reckon a chaldron of coals at retail price 65*s*.; and I take a chaldron of coals to be about twenty-eight hundred weight.

"*Question*.—Describe how pitch and asphaltum are produced?

"*Answer*.—The oil spoken of before is obtained by submitting the tar to a simple distillation; by continuing the same process, an additional quantity of essential oil of an inferior quality is produced, whilst the consistence of the tar becomes diminished, and then approaches to a state of pitch. By a further application of heat, and consequent subtraction of an additional quantity of oil, the pitch becomes converted into an asphaltum, which is to be purified in the usual manner."

Anxious as we are to avoid the charge of encouraging visionary speculation, we cannot help anticipating the extensive introduction of the gas lights into private houses as at no great distance. Mr. Ackerman in the Strand has set the example; the whole of his house, from the kitchen to the drawing-room, his extensive warehouses, shop, printing-office, and manufactory, is now and has for some years been lighted solely with carburetted hydrogen or coal gas.

LIX. *Dr.* Spurzheim's *demonstrative Course of Lectures on Drs.* Gall *and* Spurzheim's *Physiognomonical System.*

[Continued from page 312.]

Lecture 4*. The brain is an aggregation of organs; from birth to the period of declension in age, its figure may be ascertained

* Delivered Friday, October 28.

by

by that of the skull. Gall denominated the organs after the characters of men, and found in mathematicians a peculiar organ which he termed that of mathematics: in mechanicans, in thieves, murderers, each had his characteristic organ; hence a bad nomenclature of organs as being derived from abuse as well as from legitimate exercise of the faculties: difficulty of ascertaining the sphere of the activity of the faculties: organ of cunning, intrigue, and hypocrisy: by constantly reflecting on himself *selfishly*, organ of thieving is developed in man. Gall admits of no division of the faculties: no organ of understanding or volition, but admits a difference in the different faculties of mind, and proceeded in the study of man as naturalists do in the material world. He called mind a class which he divided into two orders or faculties: 1. Intellect; 2. Moral feelings. The latter he subdivided into four genera; viz. propensities, sentiments, knowing and reflecting faculties. Every faculty has a peculiar propensity, but every sentiment has not a propensity. In every faculty its aim, abuses, and the effects of its inactivity, are to be considered.—Few actions are the result of one faculty alone. The laws of induction as well as actual observation, confirm the fact of the correspondence between the faculty and the organ: every faculty is necessary to the perfect being; and where one is exercised more than another the organ becomes conspicuous. The cerebellum not fully developed until puberty and manhood: hence the organ of sexual love or *amativeness* found to reside in it. Dr. Gall was led to this inference, from attending a widow lady of Vienna, who was subject to severe attacks of epilepsy; she uniformly fell backwards, and in supporting her he perceived an unusual enlargement of the back of the neck, and a decided peculiarity in that part; it was also hotter than the rest of the body.

In children and in females the cerebellum much smaller in proportion to the rest of the brain; and this organ is much larger in males than in females, as appeared by comparing the skulls of animals (birds as well as quadrupeds) from the mouse to the elephant. Throughout the whole, the larger this organ the greater the propensity; this propensity is manifested by the thickness or elevation of the back of the neck. Wounds of this part of the neck found to produce impotency in many instances: bleeding frequently behind the ears does the same. Sawing off the horns of stags produces temporary sterility; well known to breeders of cattle that the thickness of the neck of the stallion and the bull the criterion of the generative faculty being vigorous. The ancients also made this observation. Dr. Spurzheim, among other cases, mentioned that of a French soldier who was wounded in the back of the neck in Egypt, at the early age of seventeen: the beard was never developed, and impotency has been the consequence. Idiots and Cretins sometimes with and sometimes without this propensity; and when they indulge in it, it is from the absence of reason to govern their passions, and not always in consequence of an enlargement of cerebellum or neck. This propensity not always accompanied by power of execution, as instanced in the case of eunuchs. Dr. S. concluded by remarking, that although this organ was considerably smaller in females than in males, he would show in a subsequent lecture that they possessed an amiable superiority over man, not only in this respect, but also that the organ by which the love of offspring (or philoprogenitiveness) was characterised, was conspicuously larger. Dr. S. concluded by stating, that if a single fact could be produced contrary to what he had advanced in this lecture, it would overturn the whole system which he was striving to inculcate.

Lect. 5. The professor began with observing, that the distance between the ears gave the horizontal dimensions of the cerebellum, and that the skulls of males and females are much more different in Germany than in England, and still more so than in France; in the latter, the heads of males and females are very nearly similar. The second propensity is that designated by the *organ* of *philoprogenitiveness*, or love of offspring, which English moralists call *storge;* it is an elevated point at the centre of the occiput, and very conspicuous in the female skull, and even apparent in that of little girls. The function of this organ was discovered by observing monkies, which are very fond of their offspring. In some species the females are more careful of their young, in others less, and some totally neglect them, as birds lay their eggs in the sand, and leave them to be hatched by the solar heat, and cuckoos lay them in the nests of other birds. Dr. S. exhibited the corresponding character by the presence or absence of this organ in a great variety of skulls. The female skull and neck are generally longer and narrower in all animals; but at this central extremity of the occiput, the female cerebrum is generally better developed and more conspicuous; in the fox it appears more than in the dog, as the latter is less affectionate to its young. The contrast between the skull of a cuckoo and that of a dove was very striking, as exhibiting nearly the extremes of affection and indifference to offspring. There is a great difference between human beings respecting young; some women are delighted with all children, others cannot be troubled with their noise and bustle; some men also love children, others are annoyed by them. This cannot be explained by habit, and must be referred to a peculiar and

and innate propensity. A few women, indeed, have been found almost destitute of this organ, and wherever it is wanting, it is a certain indication of a predisposition to infanticide, should external circumstances encourage the propensity. In twenty-fives cases observed by Dr. S. in France, where this organ was defective, infanticide occurred. Some nations have this propensity stronger than others; in negroes it is particularly conspicuous. It is, however, peculiarly characteristic of females, and were men to be nurses infanticide would be much more common. Boys prefer whips, dogs, &c. girls babies, dresses, &c.

III. The *organ of inhabitiveness*, Gall confounded with that of self-love, and alleged that physical propensities in animals might become moral ones in men. This is opposed by Professor S. Certain animals prefer elevated situations, as the chamois, eagle, lark, &c. delight in places far remote from the sphere of their wants in procuring food; the lark might sing on the ground, and eat its food; the eagle cannot get its prey in the air, and the chamois roams where every thing is sterile. No external want, however, can ever produce, although it may excite, a faculty. Migration is not occasioned by want of food, as birds generally depart before any scarcity even appears. There are two varieties of rats, one inhabits cellars, the other garrets; their skulls indicate their peculiar localities, as the garreteer has this organ an elevated ridge, on the back of the skull, which the cellarer wants. Different faculties have different organs; and it is very improbable that any physical propensity is changed in man from what it is in animals, or that anything physical even in brutes can ever become moral in man. The faculties never change; even Platonic love in man terminates in physical love. There is, then, a peculiar propensity for certain situations, which is indicated by this organ. All space was destined by the Creator to be inhabited with some beings. This organ is more conspicuous in aquatic animals; but the inquiry relates to comparative anatomy.

IV. *Organ of adhesiveness or attachment.* Some animals live in society, others not; some live in society and are not married, others are married, as canary birds, &c. Daws live in society, jays and magpies isolated. These habits are not owing to the activity of any faculty, but to a peculiar sentiment, which is indicated by this organ of adhesiveness. Dr. S. gives it this name, because its import is more extensive than that of attachment or friendship; the latter is only a modification of this faculty, which includes patriotism, national and local attachment, &c. Nostalgia is an abuse of this propensity, a caricature of patriotism.

 V. *Organ*

V. *Organ of combativeness.* Some children are quarrelsome, others peaceful; even delicate women sometimes fight with great obstinacy. Rabbits fight and beat hares which are generally larger animals, and little dogs often chace large ones. Gall admits of negative qualities, which Professor S. opposes; a positive sentiment or feeling cannot be the result of the want or absence of another; fear is not the want of courage, but a real sentiment. This organ of combativeness or courage is situated in the posterior angle of the parietal bone, nearly parallel with the ear; (it is generally great in proportion to the space between the ears measured behind, and in thick necks and broad heads very distinctly marked).

VI. *Organ of destructiveness;* it is a horizontal ridge in the skull passing immediately above the ear. Carnivorous animals have more brain behind than the herbivorous, instanced in the tyger, polecat, weasel, &c. which have great brain backwards, and hares, rabbits, &c. which have very little. Claws, teeth, &c. are merely the instruments which carnivorous animals use, but they are not the cause of their destructive habits; it is the internal propensity to devour. Man devours every thing; he is omnivorous. The "tyger preys not on the tyger brood;" but men in all conditions and degrees of life have this propensity, this desire to kill. Instances of an apothecary who turned executioner merely to gratify his love of destroying animal life, of merchants who became butchers, and others who paid butchers for permission to kill cattle. Some robbers always murder as well as rob; others only rob; some soldiers in the field put all to death, men, women, and children indiscriminately; others spare all they can. Idiots and madmen often evince this destructive propensity, and have occasional fits of destructiveness. It it not owing to the food, as man eat both flesh and vegetables, whereas tygers, &c. eat flesh only; animals know where to kill others quickly, as they invariably attack them on the neck. Hence it is unquestionably an internal propensity and peculiar to man, who manifests it in the pleasure which some individuals derive (both children and grown persons) from torturing animals, breaking things, as tables, chairs, glasses, &c. and is very properly denominated *destructiveness.* Dr. S. exhibited busts of Mitchell, Hollings and Sharpe the murderers, of Bellingham the assassin, and of the Frenchwoman M. Ampere, who killed her mother and two sisters, in all of whom this organ was very conspicuous: a demonstrative proof of the truth of this system; for, considering the infinite diversity of skulls, it would be impossible to account for this similarity on any other supposition than that of congeniality of character.

Lect.

Lect. 6. After enforcing the necessity of studying the primitive faculties of the mind, Dr. S. proceeded to the VIIth propensity:

Organ of constructiveness, or of mechanic arts. The faculty of construction is not derived from the form of the hands, feet, or any external instruments; monkies have four hands and do not build, other animals have only two, and yet build; the same structure of feet is found in birds that build nests, and those which do not build: hares and rabbits have similar feet, yet the latter burrow, and the former do not. Hands, indeed, are mere instruments; the faculty or disposition is internal: it is a primitive faculty. One man has this constructive faculty more developed, more active than an other, as one excells more or less in the arts. By this faculty birds build nests, men houses, ships, and construct every kind of instrument from the spade and plough to the chronometer, draw figures, design, grave, hew stone, cut cloth, make dresses, &c. and all the arts of civil life; no other faculty is so active and so useful to society. If this faculty be united to that of mathematics or music, it produces a mathematic or musical instrument maker; to that of form and colour, a sculptor, draughtsman, and painter; instances of this organ being developed in a female, a dress-maker; in the reputed skull of Raphael at Rome, and in a boy discovered by Dr. S. in the National school, Baldwin's-gardens. Rabbits have it and not hares; rats are without it, and do not build: *hamsters* (a kind of German rat, *mus cricetus*, Linn.) have it very conspicuous, as they build ingenious houses in the sand in the north of Europe. Such comparisons of similarities are not to be considered as extravagant or irrational, as the organs of these little animals are as perfect for their purpose as man's are for his. The organ of constructiveness is a small elevation situated above the temple, generally parallel with the cheek and jaw-bones.

VIII. *Organ of covetiveness:* there are thieves among all classes of society; even priests have this propensity; Saurin, pastor of Geneva, was remarkable for it; physicians often steal things from the houses of their patients and afterwards send them back; a dying man put out his hand to steal the snuff-box of his confessor; and idiots often have this propensity in the extreme, the diseased state excites it; men covet and steal every thing, even that which is more troublesome than useful to them. Jurisconsults deny the existence of this propensity as a faculty in nature, alleging that all our ideas of property are things of convention, and that the law which defines an art of theft is artificial; consequently there can be no innate faculty in man for what is wholly artificial and the result of social regulations. To this Dr. S. answers, although actual property is

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the result of society and of conventional laws, yet the feeling of property, the love of possession, is innate, it is connate with selfishness. Society creates no faculty; animals and men whether social or unsocial equally evince this feeling of property or possession, this propensity to self-appropriation. It is apparent throughout all nature; birds do not allow others to come into their haunts; hares, game, &c. have a certain country; we never see two robins on one tree; there is never more than a certain number of nightingales in one wood or forest. The necessity of food may have some effect in this case, but the disposition to prevent or flee intrusion is no less unequivocal; a dog defends his bone with more obstinacy in his master's house than abroad; men also having this feeling of *meum* resist with more courage. Hence this covetive propensity is natural, it existed before any social conventions; they came afterwards; its abuse rendered them and laws necessary to check its excesses. This, like all other propensities, if duly exercised, is laudable; it is its abuse only which is criminal; as drunkenness is not a faculty but an abuse of thirst, so theft, plagiarism, fraud, usury, self-appropriation, &c. are abuses of this natural propensity, the organ of which is situated immediately behind that of constructiveness.

IX. *Organ of secretiveness.* This is found in liars, hypocrites, dramatists, intriguers, &c. and runs a lateral direction behind the organs of constructiveness and covetiveness (exhibiting an almost continuous ridge from the temples to the occiput, if the preceding organs be much developed). All expressions are conformable to the primitive faculty; he who conceals his designs is cunning, and therefore has this organ. Keeping a secret belongs to this faculty, and is meritorious (there is perhaps no man of integrity who cannot keep a secret). The above nine faculties are denominated PROPENSITIES in man, in animals *instinct*, because innate or internal impulses.

The next division of the mind is into that of SENTIMENTS, of which four are common to man and four to animals. Sentiments have something more than mere inclination, are more generally active in the individual; some of them are common to men and animals, others only common to men, and others to animals.

The Xth configuration is the *organ of self-love* or pride, which is situated on the central summit of the head; in madmen it is very commonly developed and conspicuous. Profile of a beggar who was too proud to learn any business by which he might earn a subsistence.

XI. *Organ of approbation* or vanity. This sentiment is indicated by two oblong elevations or organs on each side of the

organ

organ of pride. Vanity has two organs, pride only one; the impulses of the former are double, the latter single; the one considers himself with relation to mankind, the other regards himself alone. Insanity arises much oftener from pride than vanity. A vain man looks around and asks himself will such or such a thing please? shall I be applauded or censured? a proud man satisfies himself and cares for no more. Women who are often vain have this organ of approbation strongly developed; it appears in all characters; a coachman is pleased with being told that he manages his horses well; a general, his army. Emulation is founded in this sentiment; it is called a man's weakness, but is in reality his strength.

XII. *Organ of cautiousness.* This sentiment leads to doubting, scepticism, irresolution, anxiety, fear; excessive cautiousness accompanied with bodily debility, disposes to melancholy, and thence to suicide. One man advances without any consideration of difficulties, while another says on every occasion, "take care," and has cautiousness. This organ appears in the breadth approaching squareness of the posterior part of the head; in women it is generally very distinctly marked. This sentiment explains how a man may have courage, and yet be at the same time fearful.

XIII. *Organ of benevolence.* Gall discovered this organ by taking a cast of gentleman's servant, who was remarkable for meekness of character; he observed a peculiarly high elevation on the centre of the forehead, just at the commencement of the hair; extending his observations, he found the same feature in all persons distinguished for benevolence and meekness of character. The skull of a carib is flat and without benevolence. Seneca remarkable for this central ridge and for his benevolence. Animals as well as men have this sentiment; skulls of the chamois and roe, the former less elevated in front and with less good nature. There is a great difference in this respect between animals of the same species; instances in the skulls of two monkies; horses having a vertical ridge between the eyes are always meek and good natured; without it, they kick; dogs with this feature are mild; without it, and being wide between the ears they bite. Thus, observed the lecturer with much natural eloquence and felicity of expression, we have seen that "a good heart is actually situated in the head." Benevolence is passive in animals and active in men; in him it is often called pity, compassion, clemency, hospitality, christian charity, &c. Is cruelty a want of benevolence? No; it has been already proved that no negative produces a positive faculty; but it checks and modulates cruelty.

Lect.

Lect. 7. *Of the faculties proper to Man.*—No philosophical system hitherto proposed has adequately indicated the line of demarcation between men and brutes: the latter have all the primitive faculties mentioned by philosophers,—perception, memory, judgement, and imagination. Some have denied them the latter; but animals dream, and therefore imagine. Sentiments proper to man:—the first is religion, which prevails every where in all states of human existence. It has been vulgarly observed that men with bald heads are religious; but women have rarely bald heads, and yet are more religious than men. Artists have delineated the chiefs of almost all religious sects with elevated heads; the saints are all so represented. Here the lecturer exhibited seven profile impressions of Christ's head, many of them he considered as that of coxcombs, having the organ of approbation highly developed. He then shewed a small figure of the Saviour, having a high head with the hair falling down on each side. The head of Christ (he observed, for the information of artists, who often exhibit a savage tyrant for a representation of the divine author of our religion,) should have all the faculties proper to man greatly elevated, and those proper to animals depressed. Lavater had this configuration very conspicuous. Religion is too vague a term for this sentiment, which does not include benevolence and the moral virtues, many men being pious without benevolence, justice, or moral honesty; they will lie, cheat, and deceive; theosophy is no better a term, as we know nothing of God's knowledge. Veneration is the best appellation, and I denominate it the *organ of veneration*, as it venerates and respects, and is especially directed towards the Great Creator of all things. But, although men are endowed with this sentiment, yet revelation is not superfluous; religion existed among all men long prior to revelation, and many nations are without revelation to the present day; yet the sentiment of veneration even to the works of their own hands, to cats, dogs, and other animals, exists among the most savage people. Man must be prepared for receiving revelation; it is not given to animals, which are incapable of receiving such gifts. This faculty is to be exercised, and it proves that religion is both natural and necessary to man. He who is destitute of this organ can never conceive how men venerate the Creator of whom they know so little. You may reason, describe, and remonstrate with such a person about religion, but he can never either feel or comprehend your meaning. If he not only rejects the practice of veneration, but cavils and rails at all religion, he only condemns himself, he merely proves himself to be destitute of this primary sentiment. His conduct is like that of a blind man who should conclude that there is no such thing as light

light in existence; as the one can never perfectly understand the nature of vision, so neither can the other religion or veneration. No sentiment indeed can be taught. As men cannot be taught the sentiment of hunger without feeling it, so neither can the feeling of veneration be learned. The organ of veneration is situated on the front-top of the head.

XV. *Organ of Hope:* This belongs to religion. There is in man a sentiment of faith: the faculties are given for the use of man in this life, and the indulgence of hope becomes faith. Gall supposed that hope arises from every active faculty, but Dr. S. considers it as having something more than mere desire; many persons evince, by their lives and actions, that they are all hope, and neglect every thing in this world in hopes of the great enjoyments of the next; they are enthusiasts and very credulous. The organ of hope and faith is situated longitudinally on each side of the organ of veneration. Some persons have faith and not benevolence, others benevolence and not faith: some are pious in saying their prayers and yet have no faith, justice, &c. Examples: Mitchell, the murderer, had no veneration or faith, and only a little benevolence; but the animal propensities predominated in the great mass of head behind. Hollings, another murderer, had faith, some veneration and benevolence, he repented and was pious; Mitchell died callous.

XVI. *Organ of ideality* or of poetry: it is situated above that of covetiveness, and is that expanded projection rising over the temples, as in the heads of Dryden, Milton, Shakespeare, &c. contrasted with the head of Locke, in which it is wanting; Homer and Horace contrasted with Demosthenes who also wanted this organ. Some persons are pleased with poetry although they do not write it; others are pleased and write it, and some make verses and not poetry. But, the question is, what is the primitive faculty of poets? Versification is not poetry, neither is rhyme, because poetry existed long before rhyme; there are versifiers who are no poets, and prose writers whose diction is entirely poetical. Poets have a peculiar manner of action; always exalted, elevated, they consider man as he should be and never as he really is; they contemplate the whole moral world in an exalted and purified state; it is by this continued exaltation they become, as it were, inspired. Artists may have this faculty with great advantage to their art. He who writes in a calm reasoning way is certainly no poet. Poets who contemplate all objects in an ideal view, have probably this faculty, which is denominated ideality. It is often very injurious to men, as all their other feelings are exalted in like manner, by this incessant exaltation of the imagination, to the neglect of reasoning, judgement, and experienced observation.

XVII.

XVII. *Organ of Righteousness:* Another sentiment which Gall has not noticed: it is the sentiment of right and wrong, This organ is situated behind that of faith. Gall denies the existence of any positive conscience, and alleges that if a woman kills her child she only repents; if a usurer omits an opportunity of taking advantage of his neighbour, he also regrets it; and therefore there must be a good and a bad conscience or none at all. He would thus have as many consciences as organs; but Dr. S. is of a different opinion, he reasons more correctly. Gall here seems to commit the error which he censures in the philosophers, and confounds will with inclination, regret with repentance or contrition. No inferior faculty has any conscience; and, however repugnant the fact may be to our feelings, there are persons who have really no conscience. Examples: a murderer in Holland threw people into the canals merely to have the pleasure of seeing them drown, and quite unconscious of its being any crime: many are pleased with criminal actions and wonder how others do not enjoy the same delight in them: criminals often go the gallows satisfied with the propriety of their own conduct: the Dutch drowner was astonished that he should be thought guilty of any crime, he had injured no one, he said, nor ever robbed any one. Bigots are of this description; they are pleased with their own conceits, and cannot conceive why all others are not of their mind. Hence, therefore, we must admit that there are persons without any conscience, which belongs to the sentiment of righteousness. In Sparta stealing was permitted, provided it was done cunningly; justice is therefore not a determinate faculty resulting from social laws: yet all men have a feeling of justice in proportion to their faculties. Conscience has been divided into absolute, individual, and positive: absolute conscience is the faculty of justice combined with the higher faculties; individual conscience is not universal, and positive conscience is the same as absolute. Christ is the only lawgiver who founds all laws on the sentiment of absolute justice.

The XVIIIth configuration is the last sentiment and *organ of determinateness*, or firmness. This sentiment fixes all the others. He who has this organ resolves and persists in his resolution; he is a decided man, and may be depended on in moments of necessity. Instances of benevolent and of pious persons who have firmness and who want it; the former are tenacious and the latter yielding. Observe persons who argue and who will not be convinced, and you may discover this organ of determinateness near that of pride. When this organ is disproportionately developed, as often happens, it indicates stubbornness. The professor exhibited a cast of the murderer Sharpe's head, which perfectly

perfectly corresponded with his character, having the organ of destructiveness, and all the animal faculties large behind and the human ones very small before.

Lect. 8. Of the intellectual faculties. These are divided into two genera, the *knowing* or *perceiving* and *reflecting faculties*. The knowing or perceiving faculties make men acquainted with the external world, with the qualities of things; they begin with the five senses. Almost every opinion on this subject, which has been taught by philosophers, is erroneous. It has unfortunately been received as an unquestionable truth from Aristotle to the present day, that there is nothing in the mind but what has been received through the medium of the senses. Yet all propensities are internal, the external senses are only the means by which the internal mind acts on the body. There is no proportion between voluntary motion and the five senses; the muscles may be diseased, yet the senses remain perfect, as they do not feel, they only convey the external impressions to the mind, they act mediately; their object is two-fold, the external world is discovered by them only; and every sense is brought in communication with the brain. Voluntary motion is necessary to all action; the nerves of motion are in communication with all the senses and the mind. All the senses are double; there are ten organs to the five senses, yet consciousness is single. This generally received explanation is not satisfactory to Dr. S., he considers consciousness to be single only in the healthy state; in disease it is often double; one thinks himself occasionally a king; another hears a voice on his one side and not on the other; these are instances of double consciousness, for the supposed voice is internal and not external; it is his internal sensation. But, if consciousness be single, what is the effect? Touch, say philosophers, rectifies all the senses after birth; vision, smell, &c. they pretend, are rectified by the touch; but one sense cannot rectify another. Every sense is subject to its own laws. Some animals see at birth and avoid external objects before touch has rectified their vision; partridges, quails, &c. run from the nest immediately after breaking their shells. Men never believe that they see internally; they are always disposed to transfer their internal sensations to external things. Madmen hear angels sing, but never suspect that the sensation is in themselves. No sense can give power to another, but one may be corrected by another to a certain degree. A stick looked at with one end in water appears crooked; by the touch we discover it is straight, but this is a contradiction, and not a rectification of the one sense by the other. Yet the senses do rectify themselves. There is is no point, no seat of the soul; the pineal gland, meninges, &c. are all visionary notions. To see with two eyes, and hear with two ears better than with one. Sometimes the senses are passive; but whenever the mind is active only one sense is engaged; shadows always fall on one eye, we see with two eyes but look with only one; we hear with both ears, but listen with one. Animals as well as men prove this by the inclination of their heads when listening. The greater number of persons use the right eye, which is generally stronger, and often larger than the left. Hands are stronger by exercise; and most children use the right without any instruction. Disease is more frequent in the left than right side; hump-backs are on the left side. The same reason must exist for the action of the passive as the active senses. The functions of the five senses are mediate or immediate; the greater number is mediate and result from the exercise of the internal senses. The exercise of an external sense indicates the activity of internal power. No sense or faculty has two kinds of functions. It is the internal faculties which exercise the external senses; for seeing only refers to colours, hearing to sounds. The five senses are insufficient to explain the source of all our knowledge; they only receive impressions but do not produce perceptions. Examples; some persons have great brilliancy, have in some degree universal knowledge, and yet are not profound thinkers. The French are distinguished for this brilliancy. Some animals are naturally tameable, others are not, and if an individual be domesticated its offspring is wild; hence we learn that one species has not proceeded from another. Dogs are debarred the houses in Egypt as unclean, and yet they are tame. The brain of the tyger inclines downwards behind, that of the polecat is nearly horizontal, that of the otter is more elevated behind, and it is tamed; the fox has the brain still more elevated; the dogs from the greyhound to the tarrier and pug, gradually rise; monkies still more so, and the orang outang has a forehead approaching that of some middling men; in like manner the brain of caribs, some negroes, and idiots, is flat. Lord Bacon had the highest and greatest forehead ever seen; the Jupiter of the ancients only equal to it. The primitive faculty however is not yet known. A ridge over the nose and eyebrows at the base of the forehead indicates brilliant men or persons of knowledge, men of fact. This deviation is found in children who look at every thing till nine or ten years of age, when it disappears in consequence of the devolopment of the other organs. This faculty, the first of the knowing or perceiving ones; and XIXth configuration, gives a knowledge of the external world; it is the *organ of individuality*, as it remembers

members facts and objects, learns and knows things, but forgets dates, same as children speak in substantives and infinitive verbs.

XX. *Organ of form;* some animals are attentive to learn, as some people easily recollect faces; they have wide eyes, depressed downwards outwards, and look somewhat as if squinting; animals have the same faculty; bees distinguish those of their own hive; dogs and elephants recollect and know their friends and enemies; this is the faculty or organ of form, which is essential to artists.

It is different from XXI, the *organ of size*, as the one concerns the figure or form, the other the dimensions or size, which are different conceptions of the mind.

XXII. *Organ of weight.* It is not feeling, not muscle; he who examines the weight or density of any thing has an internal power, and he may weigh any thing in case of physical injury or disease without the feeling sense.

XXIII. *Organ of colours;* the eyes are not sufficient for painters. Animals want the sense of colour, and some men are devoid of the sense of harmony of colours; hence an internal sense. Instances of persons attempting to become painters, and failing from inability to discriminate colours, yet having perfect vision. This organ is manifested by an elevated circle over the eyebrows, which gives the countenance a gay jovial aspect. Women excel in colouring, as their taste in dress evinces, and have this organ generally very conspicuous.

[To be continued.]

LX. *Notices respecting New Books.*

Dictionnaire Chinois, François et Latin, &c.—Chinese, French and Latin Dictionary. By M. De Guignes, *late French Resident in China, at present attached to the Ministry for Foreign Relations, Corresponding Member of the First and Third Class of the Institute.* Paris: Printed at the Government Press in 1813, but not published until July 1814; one large folio volume. London: imported by Deboffe.

A few copies only of this valuable and most laborious work have reached this country. As the small number of the impresssion, and necessary high price, of such a splendid book will render it inaccessible even to a great majority of the learned on this side of the Channel, we take the earliest opportunity of giving some account of it to our readers. We confess indeed that we are unacquainted with the Chinese language; a deficiency for which we claim the indulgence of our readers, without fear of being accused of an unpardonable disqualification; for we mean

mean to confine our present article to a mere announcement of the work. The task of examining it as critics must devolve upon philologists more skilled than we are in the languages of the East.

The work is ushered into the world with a succinct detail of the origin and progress of the enterprise, and a sketch of the objects which its learned and indefatigable author sought to attain by its completion. This is followed by a preface, distinguished for profound erudition and historical research, in which the history of the Chinese language is traced with the pen of a scholar of no common classical attainments. It is our intention to present the readers of the Philosophical Magazine with a faithful translation of this interesting article in a future number: in the mean time we bring under their notice the curious and interesting account with which M. de Guignes has furnished us of the nature of the enterprise.

"In 1801," he informs us, "when I arrived from the capital of the Chinese empire, and, after seventeen years absence, revisited my native country, the Government zealous of the honour of publishing a Chinese Dictionary, invited a foreigner from London on purpose, who, after remaining in Paris four years, took leave without even commencing it. In 1808, a foreigner was again proposed to M. Cretet; but this minister, truly attached to his native country, would not employ him: he was of opinion that a Frenchman alone ought to bring to light a work, for which the state had already provided the characters at the public expense. I was so fortunate as to receive the order for printing it, and accordingly obtained from the Royal Library the MS. Chinese and Latin Dictionary of Father Basil *, brought from the Propaganda to Paris, which was given me as a model; and I began the composition of the new Dictionary, which I was obliged to finish according to the orders of the Minister, in three years.

"Chinese writing is composed of six elementary traces or strokes, which added to 208 primitive characters form two hundred and fourteen keys, under which all the characters are classed. The Dictionaries published by the Chinese themselves are composed according to this system, *i. e.* all the characters are placed according to the order of the keys, commencing with the key of one single trace, and finishing with that of seventeen traces, which is the last. Father Basil had also arranged his

* M. Langzealles, whose and kind dispositions are well known, intrusted to my care, besides, a Latin and Chinese Dictionary, and a Portuguese and Latin Dictionary, drawn up by the Missionaries in addition to the Chinese and Latin Dictionary of M. Fourmont.

Chinese

Chinese and Latin Dictionary in this manner; but subsequently changed his plan, and ranged all the words according to the distribution of the Chinese sounds, and according to the order of the letters of our alphabet.

"The object which I proposed to myself in publishing the present Dictionary being to facilitate to those who study the language the means of finding the characters, I have preferred adopting the method of the Chinese themselves, being firmly persuaded that they must have pursued the best method, and that which was most conformable to the genius of their language. My own experience, besides, has been my guide, and I have not been able to meet with any other; for there was no person except myself, at Paris, acquainted with the Chinese language. I determined, therefore, to compose the Dictionary according to the order of the keys, and not according to the order of the tones or sounds; for the Dictionaries by keys are simpler. Besides, it would have been almost impossible to print the Dictionary on the plan of tones: this work would have required considerable depth of research, since at each character we must have been obliged to pass from one key to another, frequently very remote, and to employ a great deal of time afterwards to discover this same character among the great number of those which compose every key; a process so extremely tedious, that it would have retarded the printing of the work many years.

"In the Dictionary by keys, we have only two operations to perform: in order to find any given character, we must refer to the table of keys and to the table of traces.

"In the Dictionaries upon the system of the order of tones, there are, on the contrary, three operations: we must in the first place consult the table of the two hundred and fourteen keys to discover the right key; secondly, the table of all the characters ranged by keys, in order to have the pronunciation; and thirdly by the help of this very pronunciation, to look in the body of the Dictionary for the explanation which we want. To this long and difficult labour a serious inconvenience is annexed; *i. e.* in the Dictionaries by tones, if by an involuntary lapse of memory a character has been omitted in the table of the characters arranged by the order of keys, it is no longer possible to find it in the body of the Dictionary, although this character be there explained. Father Basil has sometimes made this omission in his Dictionary, and we may easily see how troublesome this is for those who study Chinese.

"In a word, in the dictionaries by keys, it is sufficient to know the configuration of a character, in order to have the pronunciation and signification: in the dictionaries by tones, on

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the contrary, we must not only be acquainted with the pronunciation and the tone of a character, but even its signification, before finding the form of it. For instance, if we wish to look for the word *tien*: it is necessary to know that *tien* is of the first tone aspirated, and that it signifies *heaven*, in order to discover, in the great quantity of words which are all pronounced *tien*, the character of *tien* which answers for *heaven*.

"It results, therefore, from what has been said, that the dictionaries by keys are more easy for beginners, who know the figure of a character without knowing its signification; whereas the dictionaries by tones are only useful to persons who know the pronunciation, the tone, and signification of a character, *i. e.* who know the Chinese, like the Missionaries, who, when they composed, wrote first the pronunciation of the words, and afterwards put down, by the help of the tone and signification, which are already known to them, the characters of the form of which they are ignorant.

"To this advantage of rendering our researches more simple, and consequently more easy, the dictionaries by keys add that of showing more clearly what the tonic dictionaries do not; namely, the formation of the characters. Nevertheless, that I might not make an incomplete dictionary, I determined to give also the pronunciation of the characters arranged by the order of tones. I therefore ranged all the words in alphabetical order, and formed a separate dictionary, in which every Chinese word is accompanied by the principal explanatory word*; by the help of which, as well as of the number, we may have recourse to the character in the body of the Dictionary. By pursuing this method I have avoided repeating the characters twice, as we are obliged to do in all the tonic dictionaries; a repetition which is useless in my Dictionary, since, if a person knows the pronunciation and the signification of a character, he will easily find this same character in the table of keys, by means of the number of reference†.

"The dictionaries composed by the Missionaries are all constructed on the same model, and copied from a first work‡. The Dictionary of Father Basil drawn up in 1726, *i. e.* long after the

* I have placed under the primitive words the numbers of the words which have some resemblance to them, so that we see the duplicates at the first glance.

† The Chinese have also tonic dictionaries: but these kinds of works are very rare; the explanations are almost always less detailed. There is besides at the head a table of all the characters arranged by keys, with their reference to the Dictionary of tones; which clearly proves that the arrangement of the characters by keys is indispensable.

‡ The Chinese write from top to bottom, and from right to left; but this not agreeing with our way of printing, I have placed, for the sake of facility,

the arrival of the first Missionaries in China, is itself a copy: a proof of which is, that my Dictionary, and those of the Royal Library, are entirely similar to it, with the exception of several errors which are imputable to the copyist. Nevertheless, as these errors are sometimes serious, I have collated the Dictionary of Father Basil with those of the Missionaries, every time that the former gave a different or faulty translation, and in doubtful cases I have restored the translation from the Chinese text*. I have not thought it necessary to give various readings under these circumstances; because, all the dictionaries composed by the Missionaries not being originals, but translations of texts, it was sufficient to consult these same texts to have the true meaning.

"For example, under the character *më* (10,404) we find in all the dictionaries, *quoddam animal urso simile, habens aures elephantis:* I have restored the word *nares*, which is in the Chinese text. In the same way, under the character *ly* (9918), the Missionaries have said, *res sat venusta mox peritura;* it must be *res sat vetusta*, since the text employs the words *yǒng kieòu*, a thing which has been in use a long time, and is consequently old.

"Here I ought to do justice to M. Fourmont, by declaring that in his Dictionary he has adhered to the texts of the two passages which I have quoted. This is a clear proof that M. Fourmont was acquainted with the Chinese language; and yet some persons who have not the slightest knowledge of it have taxed this truly learned man with being ignorant of it: so true it is, that men of the greatest talents are always depressed by those who have the least!

"The number of characters, including the duplicates, amounts in the table of Father Basil's Dictionary to 9959†. I have made them amount to nearly 14000‡. All the characters which I have added are from the Chinese Dictionary entitled *Tching-tse-tong*, and are distinguished by an asterisk.

cility, the characters from left to right. This arrangement does not in the least affect the signification of the character which is isolated: besides, it is conformable to the order adopted by Father Basil and other Missionaries in their dictionaries.

* The Chinese dictionaries *Tching-tse-tong*, *Tchouen-tse-oey* and *Tse-oey*.

† If there are in Father Basil's tonic dictionary any characters explained beyond this number, they cannot be found; for they are not represented in his table of characters arranged by keys.

‡ I comprehend in this number the duplicates: these characters are sometimes employed instead of the primitive character, and have like it the same pronunciation and the same signification; but, as they differ from it frequently by the key, it was important to add them, in order to enable the translator to find them.

 "In

"In composing my Dictionary, I have taken care to report under the principal word, and to put in their true place. the explanations, which in the dictionaries of Father Basil and other Missionaries are sometimes confounded with particular ways of speaking or pronouncing; so that those who translate think that these explanations belong to the phrase which precedes; whereas they have no connection with it whatever. For instance, under the character *fang* (3826) we find in the Dictionary of Father Basil, *fang-tcheou, binæ naves simul connexæ; locus, ars, modus, regula,* &c. Now these last four words do not belong to the phrase *fang-tcheou,* but to the character *fang*. It must not be concluded, however, from what I have said, that the labours of the Missionaries are defective; the manner in which they have translated, on the contrary, proves that they have consulted the texts, and taken the Chinese as their models: but the latter, after having given the explanations, both of the isolated character and of the character connected with another, have taken care, when they wished to indicate some new acceptations, to trace a circle, in order to show that the explanations which follow ought to be referred to the principal character, and not to the preceding phrase. It is this indication which the Missionaries have not given in a manner sufficiently striking; which was necessary to remove every chance of error.

"I have placed at the end of the Dictionary a table of the numeric characters, which is essential to be known, if we wish to speak correctly: I have afterwards put down the characters which the Chinese join with the word *ta*; and lastly, the characters which serve for counting. I have passed over the characters, the acceptations of which are completely opposite, because this addition, besides greatly swelling the volume, would be useless, since each of these characters is arranged in its place in the body of the Dictionary.

"The extremely limited time of three years, which was originally prescribed to me for the completion of the Dictionary, having compelled me to make the composition and printing go on at the same time, I have been obliged to throw into a supplement all the double characters which occurred when they could no longer be put into the body of the Dictionary, the figures or tracings of which these same characters are composed being fewer in quantity than those of the principal character which was printed at the moment of their occurring.

"The table of proper names arranged according to the order of the keys terminates the Dictionary. Those who wish to study the Chinese language ought to read this table with attention; for the proper names of the Chinese having nothing which distinguishes

stinguishes them from other words, it is important to have a perfect knowledge of them, in order to avoid blunders in translating.

"I have verified all the characters and all the sounds: the defective characters are few in number; the erroneous tones, at least those which struck me on running over the great quantity of phrases which exist in the Dictionary, are more multiplied; and I must request the reader to consult the *errata**.

"As to the characters, I employed those which I found in the possession of the Government †; they are correct. Some of them, perhaps, might have been better executed: but at the period when these characters were cut in wood, *i.e.* between 1715 and 1742, this kind of engraving had not reached the perfection which it can now boast of: besides, these characters in wood have not always remained in the same place, and they must necessarily have received some damage in the various removals which they have undergone.

"All the characters in the Dictionary are made in imitation of the characters which the Chinese use in their printed dictionaries, and not according to those which are to be found in the MS. dictionaries of the Missionaries. The former are perpendicular and uncommonly clear, whereas the latter are frequently inclined and sometimes incorrect. Now, as in the Chinese dictionaries, a stroke more or less changes the signification of the characters entirely, it may easily be conceived that we could not admit into a work like a dictionary, such as were not perfectly correct.

"I have conformed myself, with respect to the pronunciation of the Chinese words, to that which is given by the *Tching-tse-tong*. As to the orthography, I have preferred that of the Spaniards, because in the language of that nation all the letters are pronounced. I have nevertheless simplified it, constantly keeping in view that which was adopted by Father de Mailla, and which is besides the most recent. I have suppressed the letters *e*, *o*, *u*, employed by the Missionaries, because these letters are usually pronounced like *i*, *o*, and *u*, in the French. In some provinces of China, they pronounce *o* like *ou*: but this is not general; and I have not thought myself called upon in a classical work to express certain particular inflexions. Besides, the suppression of those three letters surmounted by a point rendered the printing much easier: an important object, since it had al-

* The Errata contains the rectifications of some errors of cyphers in the references; these errors I could not avoid, for these references were printed long before the character to which they belonged.

† These characters were engraved under the inspection of M. Fourmont, from the Chinese Dictionaries *Tching-tse-tong* and *Tse-oey*.

 ready

ready become extremely complicated by the necessary employment of the five tones, either simple or aspirated, placed over each word to indicate the various pronunciations.

"I have replaced the letters *iu* and *u* by the French letters *u* and *ou*, and the words *su* and *zsu* by the words *sse* and *tse*, conforming myself to the pronunciation which I learnt during my residence in China, a pronunciation similar in other respects to that of the Latin and Portuguese Dictionary which the Royal Library afforded me.

"Thus freed by these slight changes from every thing that can embarrass it, the pronunciation which I have made use of approaches the true pronunciation most closely; and this is all that is necessary for persons who wish merely to translate. As to such as wish to learn to speak the Chinese language, they can only expect to acquire it with the assistance of a Chinese master; otherwise they will never succeed in pronouncing, as I can, the words, *ngò* (I), *ngay* (to love), *ngan* (repose), nor even other words less difficult, such as *hien* (city), and *hào* (good).

"I have placed over each word the tone which is proper to it. This tone is simple, or aspirated: it belongs to the entire word, and may be placed indifferently over any one of the letters of which it is composed. The words with a simple tone are pronounced without any effort: this is not the case when we wish to articulate those which have the aspirated tone, in which the first letter is pronounced as if it was double: thus *piao* to strike is pronounced *ppiao*.

"Such are the facts relative to the publication of a work, the printing of which was ordered at the end of 1809, and terminated in 1813, under the auspices of the Minister of the Interior, *i. e.* about a century after the first attempt at its execution. It only remains for me to solicit the indulgence of my readers; and I flatter myself with obtaining it, if they are pleased to recollect that the Chinese Dictionary which was to have been published long since by Messrs. Fourmont and de Guignes, both known throughout all Europe for their erudition and their writings, has now been brought out by a man who has no pretensions to such titles, and whose only honour consists in being attached to a distinguished ministerial department of state, several of the members of which are eminent for talents and extent of knowledge."

We have thus endeavoured to discharge the humble, but we trust not unprofitable, duty which we undertook, namely, that of giving an account of a great novelty in the literary world. Viewing it as a specimen of typography, it exhibits no want of due care and attention; and we have no doubt it will be found as accurate a guide to the Chinese language as can be expected from

from any European press. M. de Guignes has merited the thanks of the learned of all nations by the pains he has taken; and we freely forgive him for the sneers in which he occasionally indulges in his preface and annotations at the expense of the English philologists to whom he occasionally alludes. He too boasts of his contributions to the glory of France: happy would it have been for Europe, if, like M. de Guignes, his countrymen had limited their thirst for glory to the domains of literature and science! The French editor of a Chinese Dictionary is more likely to be a benefactor to mankind, and surely must feel more inward satisfaction, than all the Marshalls of the empire! We congratulate M. de Guignes therefore, on this additional triumph of France over her neighbours. It may not be permitted to us to be envious of her literary fame, but we may fairly be emulous of her example.

Mr. Bakewell is preparing for the press a second edition of his Introduction to Geology, which will be considerably enlarged, particularly by information acquired during the author's recent examination of different parts of England, and the eastern coast of Ireland.

This edition will contain a notice of the most important geological discoveries and observations which have been made on the Continent of Europe, and in various parts of the globe, since the publication of the former, and is intended to comprise a view of the progress and present state of the science.

A new edition of Dr. Lettsom's Naturalist's and Traveller's Companion will be published next month.

This work, whose great utility has been so long acknowledged, will be considerably improved in the new impression; all the subjects formerly treated of will be carefully revised, and adapted to the present state of our knowledge of natural science; and several highly interesting additions will be made, which it is hoped will render the work a most serviceable and agreeable *Vade Mecum* to all who are fond of natural history, and to travellers particularly in every part of the globe.

It is with much pleasure that we announce the completion of "English Botany," containing coloured figures of all the British plants, in number amounting to 2592. This extensive publication has been the labour of about twenty-three years, and may be now be considered a perfect national Flora, a work which has been attempted in several countries, and which has not proceeded nearly to completion in any other, although carried on under the highest patronage, while an humble Englishman assisted by the superior talents of Sir J. E. Smith, M.D. &c. has been so fortunate as to conduct it through a memorable period of calamitous warfare with which it commenced, and with which it has happily terminated.

The Philosophical Transactions, part ii. for 1814, has just made its appearance, and the following are its contents:

15. On a new principle of constructing His Majesty's ships of war. By Robert Seppings, Esq. one of the Surveyors of His Majesty's Navy. Communicated by the Right Hon. Sir Joseph Banks, Bart. K.B. P.R.S.—16. Remarks on the employment of Oblique Riders, and on other alterations in the construction of Ships. Being the substance of a Report presented to the Board of Admiralty, with additional demonstrations and illustrations. By Thomas Young, M.D. For. Sec. R.S. — 17. Some further Observations on Atmospherical Refraction. By Stephen Groombridge, Esq. F.R.S.—18. Propositions containing some Properties of Tangents to Circles; and of Trapeziums inscribed in Circles, and non-inscribed. Together with Propositions on the Elliptic Representations of Circles, upon a plane surface, by Perspective. By Richard Hey, LL.D.; late Fellow of Sidney Sussex and Magdalen Colleges, in the University of Cambridge. Communicated by the Rev. Edward Balme, M.A. F.R.S.—19. On new Properties of Light exhibited in the optical Phænomena of Mother of Pearl, and other Bodies to which the superficial structure of that Substance can be communicated. By David Brewster, LL.D. F.R.S. Edin. and F.S.A. Edin. In a Letter addressed to the Right Hon. Sir Joseph Banks, Bart. K.B. P.R.S.—20. An improved method of dividing Astronomical Circles and other Instruments. By Captain Henry Kater. Communicated by Thomas Young, M.D. For. Sec. R.S.—21. Results of some recent Experiments on the Properties impressed upon Light by the Action of Glass raised to different Temperatures, and cooled under different Circumstances. By David Brewster, LL.D. F.R.S. Edin. and F.A.S.E. in a Letter to the Right Hon. Sir Joseph Banks, Bart. K.B. P.R.S. &c. —22. Consideration of various Points of Analysis. By John F. W. Herschel, Esq. F.R.S.—23. Observations on the Functions of the Brain. By Sir Everard Home, Bart. F.R.S—24. Further Experiments and Observations on Iodine. By Sir H. Davy, LL.D. F.R.S. V.P.R.I.—25. Observations respecting the natural Production of Saltpetre on the walls of subterraneous and other Buildings. By John Kidd, M.D. Professor of Chemistry at Oxford. Communicated by William Hyde Wollaston, M.D.

M.D. Sec. R.S.—26. On the Nature of the Salts termed triple Prussiates, and on Acids formed by the union of certain Bodies with the Elements of the Prussic Acid. By Robert Porrett, jun. Esq. Communicated by William Hyde Wollaston, M.D. Sec. R.S.—27. Some Experiments on the Combustion of the Diamond and other carbonaceous Substances. By Sir Humphry Davy, LL.D. F.R.S. V.P.R.I.—28. Some Account of the fossil Remains of an Animal more nearly allied to Fishes than any of the other Classes of Animals. By Sir Everard Home, Bart. F.R.S.—29. On an easier Mode of procuring Potassium than that which is now adopted. By Smithson Tennant, Esq. F.R.S.—30. On the Influence of the Nerves upon the Action of the Arteries. By Sir Everard Home, Bart. F.R.S.—31. On the Means of producing a double Distillation by the same Heat. By Smithson Tennant, Esq. F.R.S.—32. An Account of some Experiments on Arterial Heat. By John Davy, LL.D. F.R.S.

LXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Nov. 10. This evening the Society assembled after the long vacation; the Right Hon. President in the chair. A letter from Dr. Brewster to the President was read, detailing his further observations on the glass drops, called Rupert's. According to Dr. B's experiments heated glass assumes a crystalline form at a certain temperature, and the specific gravity of unannealed glass is different from that of annealed.

Nov. 17. A paper by Dr. Roget was communicated by Dr. Wollaston, and read to the Society, describing a newly invented instrument on the principle of the sliding rule, for facilitating arithmetical calculations. It embraces all the advantages without the defects of the sliding rule and Gunter's scale; but as the paper was accompanied by drawings, a verbal description would not be intelligible. The idea was perhaps derived from Dr. Wollaston's scale of chemical equivalents, and only more extensively applied.

Nov. 24. Part of a Letter addressed to the President by Dr. Brewster was read, containing a view of the author's numerous experiments on the depolarization of light. Almost every substance, animal, vegetable, and mineral, has been operated on by Dr. B., and all except muriate of soda and spinelle ruby, depolarized light in every direction.

The Society then adjourned till the anniversary on the 30th of November.

LXII. *Intelligence and Miscellaneous Articles.*

DIVING MACHINE.

The *Gazette de France* gives some curious particulars of experiments made on the 22d of October, with a new diving machine, by Mr. Melville, the inventor. He descended twice in the Seine, near the Pont Royal, to the depth from ten to twenty feet, and passed 56 minutes at the bottom. He took with him two swans, two ducks, and some bread and wine. He let loose the aquatic animals while under the water; went from the Pont Neuf to the Swimming School, and came out dressed as usual, without being in the least wet. The machine does not resemble any thing of the kind hitherto employed; it is neither a barrel nor a bell, but has rather the form of an egg. It is not bulky, since it contains only five cubic feet of air; this air is prepared in such a manner that pressure can do it no harm, but it is kept pure and fresh. Mr. Melville says that he has taken with him different animals, as cats, rabbits, dogs, &c. but the latter cannot bear this kind of air longer than five minutes, as they go mad in it; but he declares that he could stay half a day under water without the slightest inconvenience. He has the use of all his limbs, and can do what he pleases, saw wood, bore gimlet holes, and pick up the smallest objects. Though his pulse rises from 120 to 160, he asserts that he feels from it an agreeable sensation, a kind of electrical effect. He promises several other interesting inventions; for instance, that he shall, this winter, make a little carriage, in which two persons may next summer, take the most pleasant excursions at the bottom of the Seine, in the Ponds of Versailles, or in any river.

The Pope, since his return to Rome, has directed much of his attention to the improving and keeping in repair the public buildings, ancient and modern, of that splendid depository of almost all that is valuable in antiquities and the fine arts. The works going on at the Coliseum, and the formation of the Museum of Pius Clementinus, have been particularly the object of his care. He has not been able to prevent the transfer to the King of Naples, of the celebrated collection of Cardinal Borgia, at Velletri, and which the former purchased for 20,000 ducats; but he has put an end to the negociation for the sale of the valuable collection of medals and engraved stones which the same Cardinal left to the Propaganda.

EARTHQUAKE AT LYONS.

On the 6th inst. about six o'clock in the morning, two violent shocks of an earthquake were felt at Lyons, in the direction from

from west to east, preceded by a loud clap of thunder unaccompanied by lightning. Both before and after the report the rain descended in torrents; the weather had been the night preceding very tempestuous. The nature of this phænomenon, whose effects were felt in all quarters of the city and in the surrounding country, has not yet been satisfactorily ascertained. At Croix-Rousse parts of a wall were thrown down, and a reservoir, full of water, became dry; in many places the windows were broken to shivers, furniture thrown down, small craft dashed to pieces. The farmers, on their way to Lyons, were thrown backwards by the violence of the shock.

MEDICAL ELECTRICITY.

On this subject we extract the following article from Gilbert's Annalen der Physik, Neue Folge, 1814, part 5.

"In the battle of ———, an officer in the French service was knocked down by a cannon ball which passed very closely over the crown of his head. His soldiers who had a great regard for him, immediately carried him off the field to a place of safety. The cannon ball had, by the rapidity with which it flew, given such a shock to the head that the tongue was as if shrivelled up, and was reduced into such a small compass that it could scarcely be perceived. It was impossible for him to articulate any sound, and he had completely lost the power of speech. Medical officers of various countries attended him, and did all they could for him: eight months were passed in this melancholy way, and at length all hopes were given up of recovery.

"The use of electricity was then recommended. He was conveyed to the medical institution in the *Palais de la Cour* at Brussels, and the superintendant, Dr. Zimmerman, undertook his treatment. During the three first days he was electrified daily for three quarters of an hour at a time, and also received what is termed the electrical bath, without the slightest appearance of improvement. On the fourth day he was twice electrified, the first time in the morning fasting and the last in the afternoon, when a strong perspiration came on. In this way the process was continued for eight days; and at every application the sweat fell from him like drops of rain. Immediately the tongue began to appear and to move, and the patient seemed also to enjoy some use of it.

"In proportion as this member recovered its natural size and was freed from the restraint under which it had suffered, the patient recovered the use of his limbs also, and in a few days was able to articulate and move about. A perseverance in the treatment ultimately completed the cure."

MINE-

MINERALOGY.

We have received several letters respecting the proposed subscription for collections of petrifactions issued by the Minerological Society of Hanau, and shall lose no time in writing to our correspondents in Germany to ascertain more particulars and how they can be transmitted to this country.

DR. SPURZHEIM'S LECTURES ON CRANIOLOGY.

We have this month continued with much pleasure our sketch of the lectures, now in course of delivery by this accomplished and popular candidate for philosophical fame. Dr. Spurzheim, although a foreigner, possesses a thorough knowledge of the English language and literature, and his demonstrations are, therefore, luminous and eloquent beyond expectation. His audience consists of the most eminent professional and scientific characters of the metropolis, and not a few elegant and enlightened females occasionally grace the benches of his lecture room *.

RUSSELL INSTITUTION.

Early in January, Mr. Singer will commence a Course of Lectures on Electricity and Electro-Chemistry, at the Russell Institution.

These Lectures will include the most recent inquiries, and are to be illustrated by Mr. Singer's powerful apparatus which has been considerably improved and extended for the occasion.

LIST OF PATENTS FOR NEW INVENTIONS.

B. L. Mertian, of London.—For a method of extracting jelly or gelatinous matter from substances capable of affording the same. Communicated to him by a foreigner residing abroad.—July 12.

J. Dawson, of Dublin.—For certain means of communicating motion to bodies either wholly or in part surrounded by water or air, by the reaction of suitable apparatus upon the water or air.—July 16.

J. Smith, of London.—A spring hinge for doors and gates.—July 16.

G. Dunnage, of Hammersmith.—Method of rowing or propelling vessels.—July 26.

H. W. Vandercleft, of High Holborn.—Method of purifying and refining whale and seal oil.—July 26.

* Dr. Spurzheim's next course will commence in February next, at No. 11, Rathbone Place.—Terms Three Guineas.

A. Hill, of Plymouth iron works, Glamorgan.—Improvements in melting and working of iron. July 26.

W. Jonson, of Hall Farm, Essex.—Improved process of making salt. July 26.

W. Doncaster, of Charles Street, Cavendish Square.—Improvements in navigating vessels; a hydrostator or mill; accelerating the motion of carriages; and a dining table. July 26.

T. Sykes of Sheffield.—Improvements on fire arms. Aug. 4.

J. Collier, of Upper Thornhaugh Street.—A machine for combing wool, hemp, flax, cotton, &c. Aug. 4.

J. Thomson, of Yarmouth, Norfolk.—Making ships governable. Aug. 4.

E. C. Howard, Mary-le-bone.—Separating insoluble substances from fluids. Aug. 4.

T. Michell, of Upper Thames Street.—A machine for raising water with less power than hitherto, for the impelling of machinery. Aug. 4.

T. S. Pauly, of Little Charlotte Street.—Improvements in fire arms. Aug. 4.

G. Courtald, of Braintree.—A spindle for the manufacture of silk thread. Aug. 4.

S. Erard, of Great Marlborough Street.—Improvements in musical instruments. Aug. 4.

M. Larkin, of Blackwall.—Improvements in ship's windlasses. Aug. 16.

H. W. Vandercleft, of High Holborn.—A walking-staff to contain a pistol, powder, ball, telescope, pen, ink, paper, pencil, knife, and drawing utensils. Aug. 17.

R. Salmon, of Woburn.—Machines for making hay. Aug. 22.

J. and G. Dickenson, of Nash Mills.—Improvements in machinery for making paper. Aug. 24.

J. Penny and Joseph Kendall, of the county of Lancaster.—Making pill and other small boxes. Sept. 8.

W. Lister, of Paddington.—Improvements in a machine for separating corn or seeds from straw and chaff. Sept. 21.

J. and P. Taylor, of Manchester.—Improvements in a loom for weaving. Sept. 21.

W. E. Sheffield, of Somers Town.—Improvements in manufacturing copper and other metallic substances. Sept. 21.

J. Dobbs, of Birmingham.—Improvements in manufacturing machines for cutting and gathering in grain. Sept. 21.

A. F. Didot, of Holborn.—Improvement in making printing types. Oct. 3.

A. Shaw of Leicester.—Apparatus for the better cutting of window, plate, and sheet glass. Oct. 3.

W. Samp-

W. Sampson, of Acorn Street.—Improvements for raising water. Oct. 3.

R. Philips, of Newbury, Berks.—Improvements in a plough. Oct. 3.

J. Longhurst, of London.—An æolian organ, or barrel organ with a self acting swell. Nov. 1.

J. Walters, of London.—Improvements in the construction and fastening of frame timbers or binds of ships, whether building or under repair. Nov. 10.

W. Howard, of Old Brentford.—Improved apparatus or gear for working ships pumps, also applicable to churning, &c. Nov. 10.

L. Didot, of Paddington.—Improvements in the means for illuminating apartments or places by the combustion of tallow or other inflammable materials. Nov. 10.

W. Benicke, of Deptford.—An improved method of manufacturing verdegris. Nov. 12.

E. Massey, of Coventry.—Chronometers and pocket watches. Nov. 17.

H. Hall, of Golden Square.—Improved method of preparing and spinning hemp, flax, &c. communicated to him by a foreigner residing abroad. Nov. 17.

R. Barlow, surgeon.—A machine or instrument called the hydrostatic self-blowing machine. Nov. 22.

Meteorological Observations made at Clapton in Hackney, from October 20, *to November* 10, 1814.

Oct. 20.—Much *cirrus;* gentle showers; fine golden sunset. Star-light night.

Oct. 21.—Much cloud; damp warm day.

Oct. 22.—Misty morning; fair warm day with *cirri* and *cumuli;* a dry night, wind and rain, with falling Barometer. SW.

Oct. 23.—Fair, though clouds of dense appearance, and windy, clear cool night. Westerly wind.

Oct. 24.—Fair, with *cumulus, cumulostratus,* &c. Cloudiness came on in the evening, followed by a rainy night. SW*.

Oct. 25.—Small rain and mist during day; clear night, but

* The manner in which the raven croaked yesterday indicated that rain would soon fall. This bird has quite a different voice in rainy from what it has before fine weather, and it becomes thereby a good presage of the atmospheric changes; a circumstance noticed of old by Theophrastus, and inserted by Aratus in his Poem of Prognosticks.

Oct.

the light of the moon and stars was rather dim. A small fire balloon launched at eight o'clock, went for about two hundred feet almost perpendicularly, then took first a slow course to S. and then to E. when it ascended very high. Thermometer 11 P.M. 34°.

Oct. 26.—Cloudy day, with small rain; fine moon-light night, with light flimsy confused *cirrus*. Cool air again to night. Therm. 11 o'clock 38°.

Oct. 27.—Fair.

Oct. 28.—Fair morning; damp day. Wind westerly.

Oct. 29.—Clear sky early, but misty below, then small rain. By night confused masses of cloud moving before the moon rapidly, showed that though calm below, the upper current was pretty strong. Wind below soon followed. At 11 P.M. Thermometer 49°. Barometer 29·80. W.

Oct. 30.—Fair and warm; much *cirrus*, and at night flimsy cirrostrative clouds in the lobated form; a *corona lunaris* at time. Thermometer 11 P.M. 44°. S.W.

Oct. 31.—Clouded chiefly, and warm; gales at night.

Nov. 1.—Clouded. Easterly wind.

Nov. 2.—Clouded, with a little small rain. E.

Nov. 3.—Cloudy morning; the day cleared, and the night was cold and clear. Wind easterly.

Nov. 4.—Some gentle showers; wind E; rain at night, which was dark.

Nov. 5.—Small rain, with unpleasant cold air all day. Easterly wind.

Nov. 6.—Fair day; windy at night.

Nov. 7.—Clouds and sun; cool clearish night: lightning by night.

Nov. 8.—Fair morning; a smart thunder shower with hail about 4 P.M.; wind became N.W. About six my brother saw a large meteor moving in an undulatory course and ending by apparently going into a cloud.

Nov. 10.—Electric air; much and various *cirrus*, &c.; hazy below. Wind westerly.

Clapton,
Nov. 10, 1814. THOMAS FORSTER.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For November 1814.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Oct.	27	45	51	46	29·86	24	Cloudy
	28	42	54	44	·89	33	Fair
	29	44	49	43	·68	0	Rain
	30	43	48	44	·88	15	Cloudy
	31	44	52	50	·92	18	Cloudy
Nov.	1	47	53	46	·97	0	Rain
	2	46	43	40	30·00	0	Rain
	3	41	47	36	·01	31	Fair
	4	34	46	42	29·90	22	Cloudy
	5	42	42	38	·78	0	Rain
	6	34	46	38	·89	18	Fair
	7	35	46	46	·72	24	Fair
	8	37	47	38	·32	20	Fair, thunder in the evening.
	9	36	46	36	·59	24	Fair
	10	32	44	32	·90	30	Fair
	11	32	46	45	30·12	27	Fair
	12	50	52	46	29·88	29	Fair
	13	46	46	40	·98	20	Fair
	14	46	52	47	·99	0	Small rain.
	15	49	52	43	·72	10	Cloudy
	16	45	50	41	·87	24	Stormy.
	17	42	51	46	·91	22	Fair
	18	46	52	47	·62	23	Fair
	19	46	53	40	·53	20	Fair
	20	35	40	37	57	0	Small rain.
	21	37	39	30	·62	15	Fair
	22	26	35	30	·70	18	Fair
	23	27	40	36	·72	0	Foggy
	24	29	38	40	·80	0	Foggy
	25	47	50	49	·50	0	Small rain.

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

LXIII. *Reflections on the Inadequacy of the principal Hypotheses to account for the Phænomena of Electricity.*

[Continued from page 350.]

About the year 1733, M. Du Faye, having electrified and repelled a piece of gold-leaf by means of excited glass, attempted to repel it still further with excited copal; but contrary to his expectation an attraction took place. Surprised at this discovery he persevered in his investigation, and observed that the gold-leaf when repelled by glass was attracted as well by sealing-wax, resin, or sulphur.

From these experiments he drew the following conclusion: "that there are two distinct kinds of electricity very different from one another," one of which is found in glass, rock crystal, &c. the other in amber, copal, silk, and many other substances. He supposed that each fluid repelled itself, while it attracted the other; and affirmed that "from this principle one may easily deduce the explanation of a great number of phænomena." Du Faye, however, after some time relinquished his opinion of two electric fluids, and subscribed to that of Dr. Franklin.

In 1757 Mr. Eeles commenced a series of communications, which he affirms were transmitted to the Royal Society. In these papers the author revived Du Faye's discarded opinion of two electric fluids, but modified and extended it so considerably that it might be almost called a new hypothesis. He applied it to the explanation of the principal phænomena, thus bringing his doctrines to a degree of perfection which Du Faye had not attempted. From some unknown causes it happened that Mr. Eeles's papers were never published in the Transactions of the Royal Society.

In 1759 an account of some experiments by Mr. Symmer was read at the Royal Society, from which the author conceived that the existence of two fluids was rendered probable. He supposed that these fluids by contrasting and counteracting each other produced the phænomena of electricity; that they are essentially different from each other; that they both exist combined in matter, and when in quantities sufficient to balance each other that they do not manifest properties; but when one or other exceeds that, the accumulated power exerts its properties.

In 1767 appeared Priestley's History of Electricity, which professed to give an account of the principal hypotheses that had ever been promulgated. Under the consideration of two electric fluids, the Doctor adverts to the hypothesis of Du

Faye as "new modelled by Mr. Symmer," and proposes "to do it more justice," as he expresses himself, "than has been done to it even by Mr. Symmer himself. Doctor Priestley then proceeds with his improvement on Symmer, and it must be confessed that the statement is precisely the same as that which Eeles affirms to have originated with him so many years before.

In 1771 Eeles published his "Philosophical Essays;" in the preface of which he makes some extraordinary charges against Dr. Priestley He begins by noticing the improvement on Du Faye's hypothesis made by Symmer, in the following words: "At first I really thought that this gentleman might have taken hints from my letters*; but on more mature consideration, I think his own experiments might have led him to this doctrine." The justice of this acquittal is confirmed by the fact that Symmer did not pursue the application of his opinions to the explanation of phænomena in the extended manner which had been done by Eeles, and which might have been done so easily had Symmer acted dishonourably. Eeles then commences his charge against Priestley in the following words: "In the year 1767 Mr. Joseph Priestley published his History of Electricity, in which he attempts to mend Mr. Symmer's theory by a supposed theory of his own. But he so regularly supposed the theory which I gave in the following tracts (long before Mr. Symmer or he thought of it) and used my terms and expressions, that I cannot forbear thinking that he had better have brought my experiments to prove it (since he produced none of his own) and put my name to it, than to have left it a supposititious theory of his own. Whether in this he is or is not a plain plagiary, I must submit to the readers of both." A little afterwards he asks, "When any man has read these letters which were addressed to the Royal Society of which Mr. Priestley is a fellow, must he not think that Mr. Priestley has copied his theory from them?"—"Had the Royal Society thought fit to publish these letters, it would have saved Mr. Priestley the trouble of so much supposing; but I must take the liberty of supposing that Mr. Priestley thought I was dead, and that these letters would never come to light." Mr. Eeles concludes his Essays with the following paragraph: "Mr. Priestley has artfully shut me out of his History of Electricity; though all these papers were addressed to the Royal Society, of which he is a member; and it appears from the first of them, that my attempt to show that the electric powers were the cause of thunder was approved by that Society, and is the only attempt of that kind which stands recorded in the Philosophical Transactions."

* Sent to the Royal Society, but never published.

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In so delicate a question as this I shall venture upon no decision. To suppress the efforts of another's genius, to withhold the tribute that was due to his merit, to plunder him of his well earned honours, and appropriate them to one's self, were an offence disgraceful to human nature, debasing to science, and such as it would be painful to lay to the charge of so distinguished a philosopher. When a train of ideas, with which a person has been once familiar, is forgotten, he may fall into the same arrangement of thought, should a few of his former ideas be casually associated, and without a consciousness of ever having been similarly occupied. The letters of Eeles might thus have been read, and (as they were not published) forgotten by Priestley. The outline of Symmer might have revived Eeles's opinions in his mind, without suggesting the real source from whence they were derived. On the other hand, it is scarcely possible to suppose that Eeles would declare to the world that these doctrines were his, that they were explicitly stated by him in letters in the possession of the Royal Society, if the allegation were unfounded; and especially as Dr. Priestley, who was so particularly interested, had it in his power to ascertain, whether letters containing such doctrines had been really received, and to contradict the statement if untrue. From this view of the subject it will probably appear, that I have done no more than justice in attributing this theory to Mr. Eeles.

Hypothesis of Eeles.

1. There are *two* different kinds of fluid called electrical, which naturally coexist in all bodies, and which while in this state exert no sensible properties.

2. The active state of the fluids is that of separation. While separate, each power repels the similar kind, but attracts that which is dissimilar. If the dissimilar powers, while obeying their attractive forces, can meet in equal proportions, they condense each other into the form of flame. The powers after this condensation occupy an almost insensible compass, and are said to be in a quiescent state (as in 1).

3. The substance of electrics is pervious to each power.

4. Bodies are said to be electrified when the powers are separated in them. Thus an excited electric, having received from the rubber as much of one kind as it gave of the other, is electrified in one state, while the rubber is in a contrary state. Such an excited electric will produce on another part of itself, or on a different body, effects analogous to those of the rubber.

1. Concerning the first principle, Dr. Priestley conceives that the supposition of two fluids "is far from being disagreeable to the analogy of nature, which abounds with affinities, and in

which we see innumerable cases of substances formed, as it were to unite with and counteract one another. Here likewise, agreeable to the theory of two electric fluids, while those substances are in union, we see nothing of their separate and peculiar powers, though they be ever so remarkable. What, for instance, do we see of the striking properties of the acid and alkali while they are united in a neutral salt?"

The analogy, however, of acid and alkali with positive and negative electricity is not just. The positive and negative states are in all respects the same with respect to themselves and to all other matter, although different with regard to each other. But no two bodies can be more dissimilar in their properties than acids and alkalies: they are in fact completely opposed to each other. It therefore ill accords with our conception of the simplicity of Nature's operations to suppose *two* fluids of precisely the same effects in creation, and only different when opposed to each other. There is not even one evidence adduced in favour of the supposition; and, as shall be seen, it has no claims to admission on account of its applicability to the explanation of phænomena.

2. The first part of the second principle, namely, that each power while separate repels a similar power, but attracts that which is dissimilar, carries with it an appearance of probability, when the action of similarly and differently electrified bodies on each other is considered, and if the existence of two fluids be admitted. This principle is however encumbered with a variety of difficulties. Thus, if a pith-ball suspended by silk be electrified, and brought near an unelectrified ball suspended in the same manner, the latter is attracted, and after contact is repelled. Mr. Eeles's hypothesis explains this by the supposition that the unelectrified ball contained its two different fluids in combination, or in the state in which they exist in all bodies; that the electrified ball had only one of its powers, the other having been abstracted (in a manner that shall be shown hereafter) during its electrization: that when the latter ball was approached, its power attracted the contrary power in the unelectrified body, and repelled the similar power, and hence the attraction and repulsion. Here the order of the phænomena exists only in the order of the words; for although the word "attraction" *precede* the word "repulsion," yet the real attraction and repulsion of the opposite and similar powers of the balls being *coincident*, they should counteract each other, and the balls should remain at rest; which is contrary to fact. There seems but one way of avoiding this inference, which is so unfavourable to the hypothesis, namely, by supposing attraction to be a power superior to repulsion. But this supposition is rendered

rendered inadmissible by other inferences that may be drawn. Thus, in charging a phial, the quantity of natural electricity to be changed, is, as Mr. Eeles supposes, equal on both sides. The resistance to the change on each side is the attraction of dissimilar powers: the dissimilar powers combined in the natural state being on each side equal in quantity and in force of combination, the resistance on both sides must be the same. The quantities on one side attracted, and on the other repelled, are equal. Now the resistances and quantities attracted and repelled being equal, it follows that the separate forces of attraction and repulsion are equal. This will become more evident when the theory of charging the Leyden phial comes to be examined.

The remaining part of this principle maintains, that when the contrary powers meet in equal proportions, they condense each other in the form of flame, and no longer display sensible properties. Previously to the application of this to the Leyden phial, it will be necessary to consider the quantity of each fluid on the surfaces of the glass. The phial in its natural state contains an equal quantity of each power on its surfaces, and each surface contains equal quantities of both powers. During the process of charging, the action of the electric machine is to withdraw from the inside one of the natural quantities of either fluid, and to communicate an equal quantity of a power contrary to what was abstracted, but similar to what remained of the natural portion after the abstraction. Hence, when a phial is charged in the usual manner before the negative power, half of the whole sensible charge must be thrown in from the conductor, before the negative power can be all drawn out.

From the principle under consideration, it appears that contrary powers in equal proportions destroy each others properties. Every positive spark thrown from the conductor into the phial must combine with an equal quantity of negative electricity derived from the natural combination, and thus the properties of the spark thrown in should be destroyed or suspended. Yet experience proves, that if any quantity of positive electricity, how small soever, be thrown into a phial, the surface receiving it will evince unquestionable indications of being positively electrified.

3. Concerning the third principle Mr. Eeles observes, that Franklin supposes glass to be impervious to the electric power: he adds, "this may be confuted by so easy experiments that I am surprised that Mr. Franklin could have missed them. Take a piece of bog down, suspend it by silk, then take a pane of clean sash-glass, and warm it, and let the down hang by the side of it; then bring an excited electric to the other side of the glass, and the down shall fly off perfectly electrified.—This shows that

the powers pass entire through the glass." But this experiment does not prove its object, inasmuch as it is equally explained by the hypothesis of Franklin: and of the latter mode of explanation Mr. Eeles does not seem to have been aware. If positive electricity be thrown on one surface of the glass, Franklin supposes that it repels a similar quantity from the opposite side; this portion issuing from the glass will enter surrounding bodies, and cause them to be repelled. But it is difficult to understand what advantage can arise to the hypothesis in question, by the supposition of permeability: on the contrary, it seems to offer an insuperable objection to the opinions conceived in the following principle.

4. The chief phænomenon to be accounted for by this part of the hypothesis is the charging of the Leyden phial. This Mr. Eeles states to be effected in the following manner: The electricity thrown into the jar "will repel the power of the same kind from the opposite side of the glass, and attract the different power from the non-electrics; (*i.e.* the connection with the ground.)" The explanation is more circumstantial in the following passage: "When a spark of the vitreous power is taken into the bottle, an equal quantity of the resinous power goes from the inside of the bottle to the conductor; by which means the bottle is electrified with the vitreous power: but when you apply a non-electric to the outside of the bottle, part of the vitreous power goes to the non-electric (*i.e.* the communication with the ground) in exchange for an equal quantity of the resinous power drawn to the outside of the bottle, which resinous power is held there by the attraction of the vitreous power on the inside of the bottle."

If then the contrary powers are situate on each surface of the charged phial, and if they strongly attract each other through its substance, why do they not pass through it and combine, since Mr. Eeles supposes that glass is permeable to each kind of electricity?

But passing this objection, it will be found that his manner of explaining the charging of the phial is encumbered with much improbability and difficulty. Mr. Eeles states that "the electric powers are never rendered visible except in their passing from one body to another, in opposition to each other, when they condense each other into the form of flame." In charging it is therefore manifest, that as long as sparks are seen between the conductor and the ball of the phial, it is an infallible indication that the two powers are passing in opposition to each other; the negative power is passing from the inside towards the conductor, and the positive from the conductor into the phial. These two powers necessarily meet and "condense each other into the form of flame:" they return to the natural state, "and therefore do

not

not exert any sensible action." If sparks be still thrown in from the conductor, the phial will soon be charged, and at length will receive no more. According to the hypothesis the sparks indicate that the opposite powers are destroying each other, and their cessation shows the complete abstraction of the negative power from the fluid natural to the phial. The phial ought now to contain no more than the positive part of the natural quantity, the negative having been destroyed: so that, when fully charged, it contains but half the quantity that it does when not charged. This, which seems to be a fairly deduced consequence from some of the principles of the hypothesis, is not only improbable as to fact, but even irreconcileable to Mr. Eeles's opinion, for he conceives that the charged phial contains twice the quantity of positive electricity when charged.

Applying the same arguments to the outside of the phial, the question occurs, How can positive electricity received on the inside from the conductor be able to effect a continual repulsion of positive from the outside of the phial, when, after being saturated by the negative power derived from the inside, it must have lost the power of repulsion? The question becomes stil less answerable, when it is considered that the positive power on the outer surface was firmly maintained in its place by the attraction of the negative electricity with which it naturally coexisted.

But even if the positive power could be repelled from the outside, it must be condensed and its properties destroyed by the negative electricity attracted by the opposite power on the inside. Therefore but half the natural quantity can remain on the outside when the phial is fully charged, and nothing additional could be acquired.

The principal objection and the last now remains to be stated. Mr. Eeles supposes that positive and negative electrics exist combined, constituting the natural state of all bodies. He has endowed both fluids with the powers of attraction and repulsion: and he has given data from whence may be inferred an equality of force in the powers of attraction and repulsion. Yet in his explanation of the Leyden phial, he supposes that when a quantity of one power is thrown on one surface, it repels an equal quantity of the same power from the opposite surface, a contrary power supplying its place. If the two electrics have so great an attraction, why did the power thrown in on one side overcome the attraction of the similar to the contrary power naturally coexisting on the other side? Why should not the natural combination remain unaltered, when it was maintained by a force equal to that which endeavoured to effect a separation of its component electricities? No more should an additional quantity of the positive fluid separate the positive from the negative fluid of another portion, than a salt should be decomposed by a simple addition of its own acid or base. In fine, it is not philosophical to suppose that either fluid can displace another portion of itself, without having any more power to do so, than the displaced fluid had to resist such a change.

In defence of this principle it might be urged, that the superior power of the sparks thrown into the phial from the conductor depends on their superior intensity. But beside a great many other arguments that might be used, it must be observed that the intensity of the spark which enters the inside, and that of the spark which leaves the outside, are supposed to be equal.

Conclusion.

I have concluded my statement of the principal objections which I considered as applying to the doctrines of electricity at present received. Upon either of the two preceding hypotheses are founded all the explanations that have since been given of electrical or Galvanic phænomena: and upon these also depend the calculations that have been made by the celebrated philosophers Cavendish and Æpinus. The task which I endeavoured to accomplish was, to investigate whether or not the fundamental principles of either of these systems would in a reasonable degree be consistent with themselves and with phænomena; conceiving, if they did not, that calculation was in vain, and that all applications of them to modern discoveries ended but in deception. Hypotheses are perhaps on the whole of no use: experimental researches seem to be rather encumbered than assisted by them: and it is absurd that they should be retained when they are contradicted by the facts which they were intended to explain. It were more conducive to improvement to relinquish such suppositions and deceptive knowledge, and to banish without respect to authority every thing that deviates from the standard of reason and experiment.

LXIV. *Memoir on various Combinations of Gold.* *By M.* Obercampe*.

Although gold has long been a favourable object of chemical research, yet its properties are perhaps as little known as those of any other metal. Bergman and Lewis were the first who threw some light on its history. Afterwards Proust, and latterly Vauquelin, furnished new observations on this subject, but they still left much to be desired. In fact, it was still doubtful whether gold was susceptible of being combined with sulphur;

* *Annales de Chimie*, No. 239.

and

and chemists were ignorant whether it forms several oxides, of the manner of preparing them, of the quantity of oxygen that enters into them, and of the various circumstances that accompany the precipitations of this metal. To solve these questions is the principal object of this memoir, which may be serviceable, as preparations of gold appear to be coming into use in medicine.

Dissolution of Gold.

The best solvents of gold are nitro-muriatic acid and oxy-muriatic acid. M. Proust has observed that muriatic acid will, in time, also dissolve it, if heat be employed, especially if it be previously well divided, as by precipitation by sulphate of iron at a minimum. Nitric acid has but little action on this metal: nitro-muriatic acid is generally employed: but I prefer the oxy-muriatic acid, because it gives a more pure solution, and with less redundance of acid. I have remarked that when the solution has been exposed to heat, to render it less acid, that its golden yellow colour has passed to a deep tint of a brownish red, and that it retains this colour, though proportionably weakened, when diluted with water. The nitro-muriatic solution, when deprived of the excess of acid by evaporating to dryness, presents the same result; which proves that the orange yellow colour of solutions of gold is owing to excess of acid. By saturating them with potash or soda they become red, as when exposed to heat.

Precipitation of Gold by Hydrogen.

If a stream of hydrogen gas be made to pass for some time through a diluted solution of muriate of gold, it assumes a fine purple-red colour like that of wine, without forming any precipitate. I left some of this for a long time in a glass vessel hermetically closed. No sensible change took place in the colour, but an extremely light precipitate was formed at the bottom of the vessel, which at first appeared purple, but when put into water exhibited that blue tinge peculiar to gold held in suspension in a metallic state. The liquid exposed to heat became again yellow; metallic traces formed on the surface, and the capsule took a violet colour, having all the metallic brilliancy. The action of muriatic acid did not affect this appearance; but nitro-muriatic acid destroyed it speedily, and had then all the characters of a solution of gold.

Precipitation of Gold by sulphuretted Hydrogen.

A current of sulphuretted hydrogen gas passed through a solution of gold immediately renders it turbid, and a black precipitate is quickly formed, which is perfectly homogeneous, and exhibits

exhibits no metallic particles. Some consider this as a simple mixture of sulphur and gold; but I think it should be held as a true combination; for it has a black colour, never produced by a simple mixture of sulphur and gold; and when agitated in water the deposit is homogeneous, which would not be the case with a mere mixture, from which the gold, being the heaviest, would precipitate first. The combination, however, is but slight, and the sulphur is easily separated by heat. 2·963 grammes of this sulphuret, exposed to a red heat, left 2·386 gr. of gold; whence it follows that 100 parts of gold combine with 24·39 of sulphur; or that 100 parts of the sulphuret contain gold 80·39, and sulphur 19·61 = 100. It is possible that this sulphuret, though dried with care, may yet contain a minute portion of water, which will lessen the proportion of sulphur. Bucholz states the proportions in this sulphuret, at gold 82 and sulphur 18.

The alkaline hydro-sulphurets produce, in muriate of gold, a black precipitate perfectly similar to that from sulphuretted hydrogen; but when they are more or less sulphuretted, the precipitate contains more or less sulphur in excess, and the colour is more or less deep.

Hydro-sulphuret of potash dissolves sulphuret of gold entirely, and gives it a reddish-yellow colour. By the aid of heat it can dissolve more. Acids poured into this solution cause the sulphuret to reappear, by saturating the potash and disengaging the sulphuretted hydrogen.

Sulphuret of gold treated with potash loses its colour and becomes yellow. A portion is dissolved, yielding a solution less deeply coloured than that of which I have been speaking, and which with the acids exhibits the same phænomenon. The yellow residue does not dissolve in muriatic acid, but takes a brown colour: with nitro-muriatic acid a solution is effected.

Gold is the only metal which does not dissolve in the hydro-sulphurets; but by the addition of sulphur and the aid of heat its solution is effected, though with more difficulty than that of sulphuret of gold. This seems to prove that alkaline sulphurets dissolve gold only in proportion as it is in a state of sulphuret, or as they themselves are very much sulphuretted.

Precipitation of Gold by phosphoretted Hydrogen.

The action of this gas on muriate of gold is very variable: gold or phosphoret may be obtained at will. When a current of this gas is passed through a dilute solution of gold, the first portions of phosphoretted hydrogen give to the solution a brown colour, which soon changes to a deep purple. If the operation be stopt at this point, a yellow-brown precipitate forms, which is the gold in a metallic state, and the yellow liquid is a solution of of gold, mixed with phosphoric acid. A second portion of phosphoretted hydrogen, not sufficient to precipitate all the metal, would again give a similar result.

But if the precipitate be not suffered to form, and the gas be still passed through the liquid, the colour becomes blackish, and a black precipitate is formed which does not appear to contain gold in a metallic state. This precipitate with nitro-muriatic acid yields a solution of gold and of phosphoric acid; exposed to heat it inflames, and leaves a residue of metallic gold, retaining, probably, a little phosphorus. When this precipitate has all subsided the liquid is left colourless, and contains only muriatic and phosphoric acids. This precipitate should be considered as a true phosphuret, since it has a colour never obtained by mixing gold and phosphorus; when agitated in water, is always homogeneous; when heated in water, at a temperature which would melt phosphorus, none of this substance is separated in globules; and lastly, when exposed for days to the atmosphere, gives no white fumes and does not appear to change.

To assure myself by other means of what took place in this case, I poured a very small quantity of muriate of gold into water charged with phosphoretted hydrogen: the water assumed a blackish colour, like that of the liquid from which I obtained phosphorus. Into muriate of gold, on the other hand, I poured phosphoretted hydrogen water, leaving an excess of muriate: the liquid took a fine purple colour, like that from which gold was precipitated in a metallic state.

Phosphuret of gold, like phosphorus, has the property of precipitating the gold of its solutions in a metallic state.

It results from these experiments, that while any gold remains in solution, the phosphoretted hydrogen will precipitate it only in a metallic state; but that, in the course of the operation, this gold acting in a state of suspension, upon the phosphoretted hydrogen, passes to the state of phosphoret, and gives a precipitate entirely homogeneous.

Precipitation of Gold by the Alkalies.

The phænomena in precipitating solutions of gold by the alkalies are so different, according to the circumstances under which they are effected, that it is not surprising that chemists should differ in their opinions on the oxides of this metal.

Proust says that potash precipitates from the solution of gold a yellow powder, which is violet when operated on in a large quantity of water, but black when washed: excess of alkali, saturation, the heat of ebullition, are none of them sufficient, he says, to render him master of this preparation: this precipitate washed,

washed, and dried by the gentlest heat, is nothing more than a mixture of oxide and metallic gold.

Thomson, quoting Bergman, says that potash forms in the solution of gold a precipitate of a yellowish-brown colour, approaching sometimes to black, which is insipid, and insoluble in water.

Vauquelin announces that neither alkalies nor carbonate of potash form precipitates in a solution of gold, cold, but that by the aid of heat they precipitate reddish flakes, which he considers as pure oxide of gold, and to which he attributes the property of being soluble in water, and of having a very sensible metallic styptic taste.

I have also obtained, by alkalies, precipitates of very variable colours, according to the different circumstances which I shall describe.

1. Having poured muriate of gold into a solution of potash, leaving the latter in excess, an inconsiderable precipitate, of a blackish-gray colour, was produced. The liquid exposed to a moderate heat deposited a similar precipitate, but still retained a quantity of gold in solution.

2. With less excess of potash a yellow precipitate was formed, which I separated from the liquid, which was turbid and blackish. The liquid, when left to itself, deposited a blackish powder similar to that of No. 1.

3. Another experiment produced an orange-yellow precipitate. The liquid, perfectly clear, was of a fine yellow colour, bordering on red: it was alkaline, but less so than the two preceding. Exposed to a moderate heat, it gave a yellow precipitate of a shade a little more red than the other.

4. I poured a concentrated solution of potash into muriate of gold, taking care to stop at the point of saturation, and even to leave a slight excess of the muriate. A yellow precipitate was formed, which I separated. By adding to the liquid a great excess of alkali, I procured a brown-black precipitate.

Lastly: I have, at will, produced either yellow or black precipitates with the same solution. When excess of alkali is avoided, or a very slight excess is used, the precipitate is yellow, inclining more or less to orange: when the potash is in great excess, it is black. In both cases the clear liquid when submitted to heat forms yellow or black precipitates according to circumstances.

The variable colour of these precipitates shows that they are not identical: let us inquire into the cause of these differences.

Presuming from analogy that the differences may be owing to acid retained in combination, as in most other metallic preparations,

parations, I began by washing them, until the water by the test of nitrate of silver ceased to indicate any trace of muriatic acid. I then treated them with pure potash, and with the aid of heat I remarked, that the yellow precipitate gradually assumed a deeper tint, and became at last a brown black, while precipitates of this colour underwent no change. By saturating the potash with nitric acid in excess, and pouring nitrate of silver into it, a very sensible precipitate of muriate of silver was formed in the alkalies with which the yellow precipitates had been treated; but that with which the black precipitates had been treated gave no indication of muriatic acid.

From these experiments it is evident, that the precipitates by potash owe their difference of colour, to the greater or less quantity of acid retained in combination. The yellow precipitates retaining a certain quantity of acid ought, therefore, to be considered as true salts with excess of acid, which excess probably gives them the solubility and styptic taste observed by Vauquelin. The black precipitates, which may be considered as a true oxide of gold, appear to have neither of these properties.

Barytes added in excess to a solution of gold by oxy-muriatic acid, produces a pale-yellow precipitate, variable in intensity, according to the quantity of alkali: it still shows the presence of muriatic acid. When heated the precipitate swells, becomes gray; and lastly, brown black, like that procured by potash: it is now free from acid. The liquor still contains gold in solution.

Soda, strontian and lime produce analogous results.

The precipitates, whether from solutions in nitro-muriatic or in oxy-muriatic acid, are always of the same nature when brought to the black colour by alkalies. Therefore, in these various circumstances the brown black oxide obtained is identical.

Of the Oxide of Gold.

Having analysed this oxide by heat, and measured with care the oxygen gas evolved, it resulted from one experiment that 100 parts of gold take 9·82 of oxygen: a second experiment gave 10·21; and a third 10·60. The mean of the three is 10·01; so that 100 parts of the oxide are composed of gold 90·9, and oxygen 9·10.

These results agree with Bergman's, who states that 100 parts of gold take 10 of oxygen; but they differ from others. Proust's results were variable: in one operation 100 of gold took 8·57 of oxygen; in a second, the same proportion of gold took 31 of oxygen; and Thomson announces, in his Appendix, that 100 parts of gold always combine with 8 of oxygen, or with some multiple of of this number. These differences may have proceeded from differences in the degree of purity of the precipitate.

Triple Salts of Gold.

The precipitation by alkalies affords different results according to the excess of acid in the solution. Even when we employ a solution as little acid as possible, alkalies form precipitates in it cold; but even assisted by heat the precipitation is not complete. If the solution be very acid, the precipitate cannot be obtained cold, nor even hot, but in small quantity. This is easily explained. All alkalies possess the property of forming, with the oxide, very soluble triple salts. As a proof of this:—if we take a solution slightly acid, from which, consequently, we may by alkalies precipitate oxide, cold, and add to it a sufficient quantity of muriate of potash, or of soda, or of barytes, or strontian, or lime, the precipitates will no longer be obtainable. Even ammonia, which precipitates gold in the fulminating powder, does not form it in a solution to which a sufficient quantity has been added of muriate of ammonia.

These very soluble triple salts, though not deliquescent, crystallize with difficulty; nor does the form of the alkaline salts seem sensibly altered by the muriate of gold combined with them.

Though any of the alkalies will precipitate the oxide of gold, with barytes the precipitate is most abundant.

Precipitation of Gold by Muriate of Tin at the Minimum.

Precipitates by muriate of tin differ much according to circumstances. When very concentrated solutions of muriate of gold and muriate of tin at the minimum are mixed, in whatever proportion, the precipitate is gold in a metallic state; but when the proportion of tin is excessive the precipitate is blackish: and on the contrary, when these solutions are mixed, diluted with water, the precipitates are purple, but inclining more to violet when the quantity of muriate of tin is more considerable—a circumstance which also causes in the precipitate a greater proportion of oxide of tin. These different shades are rendered still more sensible by applying them on porcelain: the precipitates formed when muriate of gold predominates, give a shade more or less rosy; those from mixtures in which muriate of tin predominates give a colour bordering on violet.

These precipitates are of a very variable nature. M. Proust, by treating that which he tried with very weak nitro-muriatic acid, concluded that it was composed of oxide of tin at the minimum 76, and gold in a metallic state 24. By employing the

the same method of analysis, I found in the very violet precipitate obtained by using an excess of muriate of tin, oxide of tin 60·18, and gold 39·82; and in precipitate of a fine purple, made with excess of muriate of gold, I found oxide of tin 20·58, and gold 79·42.

From these experiments it results that precipitates by muriate of tin are very variable, since, when the solutions are concentrated, the gold is always thrown down in the metallic state; and that, when the solutions are diluted, the difference in their proportions occasions precipitates equally variable in their composition and properties. In what state is the gold found in them? It is difficult to answer this question; but every thing tends to show, that if it is not entirely in a metallic state, as Proust thinks, there can remain in it only a minute proportion of oxygen.

LXV. *Observations respecting the natural Production of Saltpetre on the Walls of subterraneous and other Buildings. By* JOHN KIDD, *M.D. Professor of Chemistry at Oxford. Communicated by* WILLIAM HYDE WOLLASTON, *M.D. Sec. R.S.**

ALTHOUGH the following observations afford no positive evidence of the source of that saline efflorescence which is so frequently seen on the walls of subterraneous and other buildings, and which, as consisting principally if not entirely of common nitre, long since gave rise to the name† by which that salt is most commonly known; yet as tending to throw some light on a very obscure part of natural history, they will not, perhaps, be unacceptable to this honourable and learned Society.

There can be no doubt that the production of saltpetre or nitre, in the situations above alluded to, had been observed long before there existed any general inducement to collect it from those sources; but after the invention and subsequent extensive employment of gunpowder, it became an object not only to search out every natural source of the principal ingredient of that important compound, but also to investigate the circumstances of its production; for the purpose either of accelerating the natural process, or of imitating it by artificial means.

The usual and almost exclusive occurrence of saltpetre on walls constructed of limestone, and in situations exposed to animal and vegetable effluvia, in all probability led to the empirical practice of heaping together the mortar and refuse of old build-

* From the Philosophical Transactions for 1814, part ii.

† Saltpetre (Sal Petræ.)

ings

ings with putrescent animal and vegetable matter; from a mixture of which kind, after exposure for a sufficient length of time to the action of the air, a quantity of nitre may usually be obtained by lixiviation: but it would be a question of mere curiosity, on this occasion at least, to investigate the origin of the practice. The intention of the present paper is to state the result of a series of observations made during the last year, on the connexion that exists between the natural production of nitre and the state of the atmosphere. In detailing these observations, it will be convenient to give previously a description of the laboratory of the Ashmole Museum, in which building they were principally made: nor shall I be afraid of being thought too minute in this description, or in any other part of the following detail, by those at least who know the precision that is requisite in every induction, which like the present rests on phænomena of an obscure and equivocal nature.

The Ashmole Museum, which was built by Sir C. Wren in the reign of Charles the Second, is an insulated building, constructed entirely of calcareous freestone, and consisting of three stories. The lowermost of these stories was originally designed for, and has constantly been used as, a chemical laboratory.

The pavement of the laboratory, on its eastern. northern, and western sides, is about nine feet below the level of the street in which the Museum stands; on its southern side it is on the same level with an area, about ten feet in breadth, which in part occupies the site of the ditch of the old town, and insulates a quadrangular projecting part of the whole building of the Museum. The laboratory itself is a single room sixty feet in length from east to west, and twenty-five in breadth; having an arched stone ceiling, the centre of which is seventeen feet above the level of the pavement. The walls of this room, which are nearly three feet in thickness, are constructed of squared calcareous freestone, which I have reason to believe was dug from a quarry near Burford, and is technically called Windrush stone, from the river of the same name. There are four windows in the upper part of the north side of the laboratory, formed in the curve of the arched ceiling; the dimensions of each of which are five feet by four and a half.

There is no window either on the eastern or western side of the laboratory.

On the south side there are two windows, one at each extremity, looking into the area above described; and these windows are placed at the usual distance from the ground, that is, about three feet: and all that part of the south side intermediate to these two windows separates the laboratory from the quadrangular

quadrangular projecting part of the whole building of the Museum already mentioned.

The saline efflorescence takes place most copiously on the north wall, and it occurs on various parts of it from nearly the level of the pavement to within three or four feet of the centre of the arched ceiling. It takes place also, though not so abundantly, on the east and west walls; and also at the eastern and western extremity of the south wall; but it is worth noticing, that I have never seen it on that part of the south wall which is common to the laboratory and the attached projecting building of the Museum. It is true that there are chimneys in this wall connected with fires that are lighted daily; but this circumstance does not seem sufficient to account for the absence of the nitre, because its formation takes place in another part of the laboratory equally near a chimney, and in which, from being inclosed, the temperature of the air is always considerably higher.

It is also worth noticing, that between the highest and lowest points of its appearance in every part of the building there are intercepted spaces entirely and always free from the least deposition.

I may here mention that the occasional formation of nitre is observable in many other buildings and parts of Oxford, besides the laboratory of the Ashmole Museum; as on the wall, called Long Wall, which bounds the park of Magdalene College to the west—on the exterior surface of the south wall of the Theatre—on the exterior surface of the three walls of the quadrangular projecting part of the Ashmole Museum—very abundantly on the inclined base of the windows of the Examination School, looking to the north—and also very abundantly on the west side of the wall, which separates the square of the Schools from the arched way leading from thence to the Theatre and Convocation House.

It has been observed repeatedly, that the presence of lime is necessary to the natural production of saltpetre; and in all the foregoing instances the stone on which the saline efflorescence takes place is the common limestone of Oxfordshire. I have only once observed its formation on the surface of a brick wall: but in that instance the substance of those bricks on which the nitre appeared had crumbled away to some depth; and if this destruction of their texture be owing to the presence of an unusual proportion of lime in the clay of which they are made, (a supposition not improbable, since many parts of the stratum of clay from which bricks in this neighbourhood are made do contain an unusual proportion of lime), the reason of the exception in the case of this brick wall will correspond with the truth of the general observation above stated.

The following circumstance is particularly deserving of notice. A part of the north wall of the laboratory, on which saltpetre usually effloresced, having been covered with wainscot some months since, and the wainscot having been painted with common white paint, I was surprised after a time in observing an efflorescence on particular parts of the paint, similar to what might have been expected on the wall itself. Where this efflorescence had taken place the paint was loosened from the wainscot, and might be readily peeled off in small flakes. The saline particles of which this efflorescence consisted I at first supposed to be nitrate of lead; but upon examination in various ways no trace of lead could be found in them, and they exhibited the principal characters of common nitre: they deflagrated, for instance, with charcoal, leaving a deliquescent alkaline residuum. Many weeks have elapsed since that saline efflorescence was brushed off, but I have not yet observed any renewal of it.

Though the production of saltpetre had been pointed out to me in the laboratory of the Ashmole Museum as long since as the year 1802, I was prevented by many circumstances from observing with any degree of regularity or precision the phænomena of this natural process previously to the commencement of last year; about which time having carefully brushed away the whole of the saline efflorescence from a part of the north wall situated below the level of the street, and very imperfectly exposed to the light, I was surprised by the fact of its quick reappearance. It was brushed away at the end of January; but within three days it had again effloresced in sufficient quantity to present that appearance of hoar frost, or down, or mould, which is very characteristic of the manner in which naturally formed saltpetre is often accumulated; and which a person, utterly incapable of judging of the real nature of the substance, described at the time by the term "damp."

I was still more surprised, however, in observing after a few days, that the quantity of the saltpetre was apparently very much diminished; and that at the end of eight or ten days there was scarcely any appearance of it remaining: though there was no reason to suppose a particle had either accidentally fallen to the ground or been intentionally removed.

The same part of the wall was again brushed perfectly clean, and I continued to make daily observations in expectation of a renewal of the process; but no new collection was perceptible, excepting in the form of a very few detached and minute capillary crystals, till the 16th of March; on which day, having been absent during the whole of the 14th and 15th, I found a more abundant accumulation than in the preceding instances.

It

It was an obvious supposition that the reproduction of the nitre was connected with some change in the state of the atmosphere: and it is to be remarked, that its first-mentioned appearance took place in frosty weather, and that its reappearance about the 16th of March had been preceded by a frost of a few days; whereas during the intervals in which it disappeared and was not again produced, the weather had been mild. The wall was again brushed quite clean on the 16th of March; between which day and the 4th of April a considerable quantity had again collected on the same part; the process having been more rapid during the last four or five days, which were cold.

On April the 12th the front of this part of the wall was renewed by scraping, for the purpose of observing what would be the effect of a fresh surface, and before the 25th of April nitre had formed on many parts of this new surface; but having first increased and then continued stationary for some days, it began to diminish about the beginning of May, the state of the air having been very moist during the two or three preceding days.

About the middle of May 1813 I selected several distinct parts on which the spontaneous formation of nitre usually takes place, some within and some without the building of the Museum, and began to make daily observations on the phænomena of that process, which I noted down at the time: but as a register of this kind would be unnecessarily tedious in its recital, I shall beg leave to give the following general results of those observations; requesting it may be kept in mind, that I do not presume to lay a greater stress on them than is proportional to the short period of time and the circumscribed space in which they were made.

It appears then from the observations I have hitherto been able to make, that the spontaneous formation of saltpetre is, generally speaking, much more extensive and rapid in winter than in summer, whether it take place on the interior or exterior of a building: thus during the period of the cold weather in January 1814, it became visible not only on parts of the walls where I had never before observed it, but even on the pavement of the laboratory. The part of the pavement on which it appeared is adjacent to the north wall; but as the greater part of the pavement is covered by a flooring of wood, it is impossible to say whether or not it took place on other parts also. The stone forming the pavement is the same kind of limestone as that of which the walls are built.

Again, whereas in summer its reproduction is most rapid and extensive in proportion to the degree of light present, the reverse of this (though not universally) takes place in winter. Wherever the saline efflorescence in question occurs, the sur-

 face

face of the stone becomes permanently discoloured, as if from the effect of damp; but this discoloration is merely superficial. If these discoloured parts be whitewashed, the process still goes on; and the whitewash is gradually detached in flakes: but it is difficult to ascertain whether the nitre is formed on the whitewash, or on the wall which it covers; though probably the latter.

When the spontaneous formation of nitre takes place slowly and in a sheltered situation, it is at first visible in the form of minute prismatic crystals, which usually project from the surface of the wall nearly at right angles; but sometimes they are scattered in different directions, lying upon its surface so lightly as scarcely to appear in contact with it. In general, however, the saline efflorescence makes its appearance in extremely minute capillary crystals, either accumulated in groups, which resemble recently fallen flakes of snow, or investing the wall like a fine down.

During the severely cold weather of January 1814, it appeared in some places in the form of minute dense grains closely aggregated; while in others it still continued to wear the appearance of down or wool: and the local circumstances most obviously connected with this difference in the manner of crystallization, were the presence of a greater degree of light, where the granular deposition took place, and a less degree of shelter from the influence of cold air.

In some instances the production of the saltpetre is accompanied with a disintegration of the substance of the stone on which it is formed: but this circumstance is only observable on stones of a loose texture.

The shortest interval I have observed between the time of its having been brushed away and its reappearance, is four hours: but it was then in full efflorescence, and would probably have been visible much earlier. The observation was made on November the 17th, about midnight: there had been snow in the midlde of the day, and the night was frosty.

The spontaneous formation of nitre takes place indifferently on the surface of the stones composing a wall, and of the mortar by which those stones are cemented; and near the close of the late frost, I observed it for the first time on the surface of a partition consisting entirely of laths plastered over with the mortar or stucco commonly used for that purpose.

It accumulates in greater quantity on some parts of a given surface than on others; and this difference in the degree of its accumulation, is probably connected with some slight difference in the texture or composition of the stone on which it is formed: for I have repeatedly observed that after a careful removal of it, its

its reproduction takes place to the same proportional extent in the same parts: and this difference in the degree of the efflorescence often takes place not by a gradual transition, but as abruptly as if the line of separation had been marked by a graver; so that the part of the surface on one side of the line shall be almost totally devoid of any efflorescence, while on the other side it shall resemble the accumulation of hoar-frost on the small branches of a tree.

The saltpetre formed in summer scarcely appears to contain a particle of any calcareous salt: that formed in winter contains most evident traces of such a salt, though probably even in winter the amount of this is not much above one part in two hundred of the whole mass.

A frosty, clear, and dry state of the atmosphere is particularly favourable to the natural production of nitre: but there seems to be a limit to its formation, on the same spot, even under the most favourable circumstances; the quantity of the nitre not continuing to increase after it has proceeded to a certain extent.

In a moist state of the atmosphere the formation either does not take place at all, or goes on slowly: and if that state of the atmosphere which is unfavourable to the production of nitre continue a sufficient length of time, the nitre already formed gradually disappears. At the commencement of these observations I attributed the occasional disappearance of the nitre to its mechanical removal from the wall, and supposed that it must have been by accident brushed off: but repeated observations convinced me this was not the case, its disappearance proceeding as gradually as its previous appearance; besides which, had it fallen from the wall, I should most probably have found it on the pavement beneath, which never happened to me. It afterwards appeared probable, although I have never met with any condensed moisture on the surfaces submitted to the foregoing observations, that the aqueous vapour precipitated from the atmosphere in the state of it above alluded to, might dissolve minute particles of the nitre, and be absorbed with them into the substance of the wall; but on this supposition nitre ought to be found in lixiviating a portion of the stone taken near the surface. I have however made the experiment without detecting any nitre in the stone so taken. But, in opposition to the idea of the absorption of the nitre into the substance of the stone, I found the efflorescence disappear in more places than one during the severe frost of the present year (1814), at a time when, from the temperature of the stone, if not of the air also, the absorption here supposed could not have taken place; since, that temperature being below the freezing point, the aqueous particles

 would

would by congelation have been rendered incapable of dissolving the nitre.

It is deserving of notice, that such a spontaneous disappearance of the nitre, as has been just mentioned, took place antecedently to and during the late heavy fall of snow on January 18 and 19.

Wishing to ascertain whether the free presence of atmospherical air be necessary in the natural process under consideration, I selected a part of the wall on which the formation of saltpetre usually takes place to a considerable extent; and insulated about a square foot of its surface which had been previously brushed quite clean. It was insulated by means of a plate of glass, the edges of the frame of which were covered with a cement, so as to exclude any access of air between the glass and the wall. As the depth of the frame was not more than the third of an inch, the inclosed space contained but an inconsiderable quantity of atmospherical air. On the following day, October 29, I observed that a formation of nitre had taken place on the part of the wall within the glass, but that it was not so extensively diffused, nor so abundantly deposited, as had been usual before the application of the glass; and the crystals, which were distinctly prismatic, were much more accurately defined, and larger, than I had ever before observed them; in consequence, probably, of the tranquil state of the medium in which they were formed. On the day following, October 30, the quantity was increased; but it remained stationary from that time to November 12, when it began to diminish; and on November 16 there was no longer any appearance of it: nor did it reappear in the interval between November 16 and 29. In the mean time the efflorescence had not only taken place on the adjacent parts of the wall in the usual manner, but had been more than once brushed off and again deposited, while no increase had taken place in that originally formed within the glass.

On November 29, the exterior surface of the glass was, for a purpose to be mentioned presently, covered over with whitewash; which was not removed till January 8, 1814. No observation could of course be made on the appearance of the surface within the glass during the interval that the glass itself was covered with whitewash; but no nitre was visible on the removal of the whitewash on January 8. It had, however, reappeared before the morning of January 10, partly in separate capillary prismatic crystals which were much larger than on the former occasion, and reached from the surface of the wall to the inner surface of the glass; and partly in small flocculent tufts; the whole quantity of the nitre formed being much greater than, and the manner of its formation being very different from, that formation

which

which took place within the glass on October 29 and 30. Still the whole quantity was not nearly equal to that which was usually formed on the same surface when exposed to the free action of the atmosphere. The increased quantity however formed in January, compared with that formed in October, corresponds with the observation that the natural production of nitre is much influenced by temperature: but it is worth mentioning, that while the nitre formed within the glass in January occurred either in distinctly prismatic crystals or in flocculent tufts, that which was formed on the adjacent parts of the wall was of the granular character above described in page 420.

Having by the foregoing experiment satisfied myself with respect to the necessity of the free presence of atmospherical air for the full effect in the spontaneous production of nitre, I wished to ascertain the nature of the connexion between the nitre formed and the stone on which it is formed; and particularly whether carbonate of lime simply would be sufficient for its production. I therefore made a nearly liquid paste with distilled water and prepared chalk *, with which I covered the exterior surface of the glass that had been attached to the wall for the purpose already mentioned: but though nitre was repeatedly formed in the neighbourhood of the glass, not a particle appeared on the whitewash during the space of nearly six weeks, from November 29 to January 8.

The foregoing statement contains the general results of all the observations I have hitherto been able to make on the present subject. I subjoin an imperfect analysis of the stone of which the laboratory of the Ashmole Museum is built, and of the saltpetre periodically formed on it; concluding with a few remarks on the local differences connected with a more or less extensive formation of that substance, and on its probable or rather possible source.

The stone of which the laboratory is constructed belongs to one of those alternating series of clay, and sand, and calcareous freestone, of which the varieties of Bath stone form nearly the lowermost strata; and the Headington stone, of which the greater part of Oxford is built, the uppermost. It is principally made up of the *debris* of small sea shells, but frequently contains organic vegetable remains, and always some proportion of sand and ochry clay.

Six portions of this stone taken from different parts of the labo-

* As chalk sometimes contains magnesia, and almost always a small proportion of silex and alumine, I intend on a future occasion to repeat the experiment, using very finely pulverized crystallized calcareous spar, instead of chalk.

 ratory,

ratory, each weighing at least two hundred grains, were separately submitted to the action of a sufficient quantity of diluted muriatic acid. The undissolved residuum, which amounted in no instance to as much as four parts in a hundred, consisted of a yellowish brown ochry clay, mixed with a few particles of white sand, and small laminar fragments of a dirty-white colour, very much resembling portions of the siliceous incrustation of the Geyser spring. Small flocculent shreds slowly subsided in the muriatic solution, derived probably from the membranous part of the shelly matter of the limestone; for this insoluble residuum, when thrown into red-hot nitre, slightly deflagrated, and gave out an odour resembling burnt bones: and as this residuum had been collected without the use of a filter, the inflammable matter could not well have been derived from any other source than the stone itself.

The muriatic solution contained a small proportion of iron, and gave a very slight trace of magnesia. So that the composition of the stone may be thus stated:

Carbonate of lime	96
Oxide of iron, sand, ochry clay, and animal membrane	4
	100

In offering the following imperfect analysis of the saltpetre produced in the situations above alluded to, it is necessary to state, that the quantities on which I have hitherto had it in my power to operate have been very small.

Some preliminary experiments made on a solution of saltpetre which had been formed during the summer, gave the following results.

The solution contained no disengaged acid or alkali, and upon the addition of oxalate of ammonia afforded the slightest possible evidence of the presence of lime. It contained minute portions of sulphuric and muriatic acid. A portion of it being evaporated left a mass of crystals which, in their form, as in all their other characters, exhibited the properties of nitrate of potash.

As it is usually stated by writers who speak of the subject, that nitrate of lime is a principal constituent part of saltpetre formed by a natural process, I was much surprised by the very slight trace of lime evident in the present instance; though I had felt assured previously that but little would be found in the natural saltpetre which I have had an opportunity of examining; having never observed in it any tendency towards deliquescence. Having had frequent occasion to suppose that carbonate of lime is much more readily soluble in water than is commonly believed, and having never been able to detach the saline efflores-

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cence in question from the walls of the laboratory, &c. without admixture of particles of the limestone, or of the whitewash, amounting to at least seven or eight parts in a hundred, it struck me that these particles might be the source of the lime rendered evident by the addition of the oxalate of ammonia in the solution of the saltpetre. I therefore pulverised small portions of calcareous spar, of the common limestone of this country, and of whitewash; and having agitated accurately distilled water, at the common temperature, with each of these portions, I then filtered the water, and tested it with oxalate of ammonia. In each instance there was fully as copious a precipitate as when the oxalate of ammonia had been added to an equal quantity of the solution of saltpetre.

Similar preliminary experiments having been made on some saltpetre detached from the same part with that already submitted to examination, but formed during the winter instead of the summer, the same results were obtained, with this single difference, that the precipitate obtained by the addition of oxalate of ammonia was much more copious: and I found this to be the case from whatever part of the laboratory or elsewhere the saltpetre had been detached, provided it had been formed during the winter. I could not extend the experiment on the saltpetre formed during the summer, having only reserved a portion from one spot.

Judging from such experiments as I have made, it appears that the saltpetre formed in the situations described in this paper consists of full 99 parts in 100 of nitrate of potash; with a very minute proportion of some calcareous salt, which is either not at all present in in the saltpetre formed during summer, or is present in smaller quantity than in that formed during winter—though even in the latter instance it scarcely amounts to one part in 200.

The proportion of sulphuric and of muriatic acid, and the bases with which these acids are combined, I have not attempted to ascertain, on account of the minuteness of the proportion in which they evidently exist.

In considering the relative situations of the different parts of the laboratory, and the other buildings also, in which saltpetre is naturally formed, it is evident that the efflorescence takes place only where the exterior of the wall on which it is formed is either exposed to the direct influence of the weather, or is in contact with the adjacent ground; not taking place at all in those instances in which the wall is neither exposed to the weather on either side, nor is in contact with the adjacent ground: from which it might be argued, that the effect depends upon the action

action of the external air, or of the moisture of the ground transmitted through the substance of the wall: but the result of that experiment in which, the action of the air on the interior surface of the wall having been prevented by the intervention of a glass, the formation of nitre took place only to a very slight extent, is in a great measure at variance with such a supposition.

Again: though it is evident that the natural production of the saltpetre is closely connected with changes in the state of the atmosphere, those changes relating not only to its temperature but also to the degree of its moisture and barometrical pressure; yet a much longer series of observations, and these not partial like the present, but carried on in various parts of the world, is requisite, before even this part of the problem can be accurately solved. There still would remain a part of much more difficult solution, namely, the source of the metallic base of the alkali of the nitre.

With respect to this difficult question, if we compare the elements present in the composition of the saltpetre formed, with the elements of the substances present during its formation, it seems a reasonable conclusion that the potassium, or the metallic base of the potash of the saltpetre, is either a simple principle of some of the elements present, or that it results from the union of two or more of those principles, or of two or more of the elements themselves*.

The gross compounds present during the formation of saltpetre, in the instances mentioned in this paper, are atmospherical air and the limestone on which the saltpetre effloresces; and all the component parts of these may be ultimately resolved into the following elements—oxygen, nitrogen, hydrogen, and carbon, together with iron and the metallic bases of some of the earths.

The component parts of nitrate of potash, of which the saltpetre under consideration almost entirely consists, are nitric acid, water, and potash; which may be resolved into the following elements—oxygen, nitrogen, hydrogen, and potassium: and all these elements are experimentally known to be present in the situations where saltpetre is formed, with the exception of potassium.

In the supposition then that the saltpetre is a product and not an educt of the above process, since no potash can be de-

* The terms "element" and "principle" are here used in the same relation to each other as the early physical philosophers used the terms *στοιχεῖα* *ἀρχαί*. They supposed that the former, though incapable of decomposition by common means, were not necessarily to be considered as absolutely simple substances: those absolutely simple substances they expressed by the term *ἀρχαί*.

tected

tected either in the air or limestone, the potassium must either be contained in a free state in the atmosphere or in the limestone, which from its remarkable attraction for oxygen is inadmissible: or it must be a component principle of some one of the elements present; or lastly, it must be itself a compound of two or more of the principles of those elements, or of two or more of the elements themselves.

But I am aware that the saltpetre may be considered as an educt of the process, and that it may possibly exist already formed in the atmosphere in a state of minute division: yet, when we consider the comparatively fixed nature of that salt, and that no experiments have yet detected its presence in atmospherical air, or in the moisture precipitated from atmospherical air; and lastly, when we reflect on the probability that the metals, of which potassium is one, are compound bodies, the former supposition seems upon the whole preferable to the latter.

February, 1814.

LXVI. *Stratification of Glamorganshire**.

Mountain Lime.

THIS rock forms the whole of the peninsula of Gower, westward of a line drawn S.S.E. through Penclawold, Gellyhir, Littlehills, west side of Clgne Moor to Blackpill; appears again near Pgle Inn, Cenfig, Skerr, Cornely, Nottage, Newton Down, Cevn Cribbor, rises through the lias at Ewenny, forms Ewenny and Ogmore Downs, Golden Mile, Saint Mary Hill, Coed y Mwstor near Coychurch, Newforest, Llanblethian, Peulline, Trecastle, Lanchacar, Stalling Down near Cowbridge, Saint Lythians Down, Courtgrala, Bolston, covered by alluvia at St. George's and elsewhere in that valley. Very hard variety at Saint Fagaas. Hills near Lantrissant E. and W. Lower Garth, Castell Coch, Lisvane towards Caerphilly, Ruddrey, whence at Machen it runs into Monmouthshire. This last named range bounds the great coal basin which will be presently described. I have observed no fossil madrepores, but abundance of entrochi; galena is found in various places. A shallow bed of coarse sandstone rests on the mountain lime at St. Hilary and Lantrythid; beds of white calcareous slate have been cut through in the lead mines there. Manganese has been worked on Newton Down. Cherty blocks may be observed there and on St. Mary Hill.—

* For the localities mentioned in this sketch, see the excellent Map of Glamorganshire, published from the Survey of G. Yates, by Cary, 181, Strand, London.

A grey

A grey variety streaked with red and white is worked at the Mumbles by Mr. Wallis of Swansea, sawn into slabs and polished. Near Pyle a reddish-grey variety occurs with green and yellow veins: it is very hard, and contains no fossils. We may observe it near Coity. Mytili, chamæ, pelices, pectenites, gryphites, &c. are found in some parts of the mountain lime. I have hæmatitic iron ore from Lanharan. The mountain lime rises on the north side of the county near the Ddinas rock, Brecknockshire. Here I observed the laminated sulphate of baryte, containing small crystals of sulphuret of iron partially converted into green vitriol. Here two valuable veins of fire clay may be seen.

Red Ground.

Beds of marl, red, blue and white, sometimes with narrow beds of siliceous freestone, occur at Sully, Lavernock, Cogan, Porthkerry, where they pass into blue lias limestone. At Pennaith Point, a bold headland, the stratification exactly resembles Aust Cliff, Glocestershire, red rock marl with beds of common and striated gypsum. Barry Island consists of red ground, lias, and mountain limes irregularly disposed. The tract along the coast near Cardiff lies low, consisting of vast beds of pebbles and gravel protected by a sea wall.

Blue Lias.

An acute angle one mile N.N.W. of Bridgend is the most northern direction where we can trace the lias in Glamorganshire, there it joins the coal-basin at Penyva. It is seen everywhere at Laleston, Tithegston, Merthyrmawr, where there are very extensive caverns through which part of the Ogmore river passes. It forms the cliffs a little west of Dunraven Castle to Marcross, Monknath, Boverton, St. Athan, Aberthaw, where it appears in the form of banks of shingles, Porthkerry thence to Fonmor, Penmark, Wenvoe, Lancarvan, Lantrythid, Llancadle, Flemingston, Saint Mary Church, Landough, Llanmaes, Llantwit, Saint Donats, Lisworney, Colwinston.

At Wick Cliffs E. of Dunraven the lias is 200 feet thick, dipping a little S. About a mile west of Dunraven, the cliffs assume a different character. A calcareous breccia with nodules of chert, jasper, &c. (similar to that described by Mr. Gilby, Phil. Mag. vol. xliv. page 246) underlies the lias for about half a mile. The mountain lime rises from under the Sutton stone a little to the N.W. of the caves.

At Tresilian E. of St. Donats there is also a very extensive cave. Numbers of fossil gryphites occur in thin seams of clay between the lias beds, fossil wood in several states, silicified, or burning with the smell of Cannel coal, of which it has the fracture. A coralline

coralline (species unknown to me) is often met with. Near Dunraven large fossil nautili, and at Llanblethian delicate casts of cornua ammonis in calcareous spar. Shells of several other genera occur less frequently. The breccia above mentioned occurs, I believe, close to the blue lias at the mouth of Cadorton river.

Small oysters, nearly in a recent state, occur in the seams of clay between the layers.

Sutton Stone.

The Sutton or Silton stone which occurs near the Dunraven caves is of a greyish-white colour, and contains cockles, muscles and scollops converted into silex. The rock makes but indifferent lime, as it contains much siliceous matter. It bears the weather well, as the copings of Swansea Castle, Neath Abbey, &c. evince. The Sutton stone overlies the masses of rock which rise round and in front of the caves. These masses are calcareous, but mixed with much argil of a reddish brown or black colour; and it is curious, that this species of rock occurs no where else in Glamorganshire.

Coal Basin.

This important formation with its valuable beds of different coals, and argillaceous iron ore, has been well described by Mr. Martin. (Phil. Trans. 1806.) It comprises all the tract lying N. of a line drawn eastward from Taybach, along the hills behind Bridgend and Lantrissent to the extremity of the county. The iron ore is most abundant round Merthyr and at the upper part of Heath Valley. The picturesque scenes in the vales of Tawey, Neath, Dgmore, Rondda and Taaf, lie within its range. The mountain lime rises to the north of the basin at Castell Morlais near Merthyr. A valuable bed of siliceous freestone is met with at Coity; it lies between the coal basin and mountain lime; it is also worked near Pyle. At Sutton 1½ mile W. of Dunraven a valuable bed of calcareous freestone much used for ornamental masonry rests upon the lias near its junction with the mountain lime.

Dec. 3, 1814.

LXVII. *Some Experiments on the Combustion of the Diamond and other carbonaceous Substances.* *By Sir* Humphry Davy, *LL.D. F.R.S. V.P.R.I.**

Since it has been shown by various accurate experiments, that the diamond and common carbonaceous substances consume nearly the same quantity of oxygen in combustion, and produce

* From the Philosophical Transactions for 1814, part ii.

a gas

a gas having the same obvious qualities, a number of conjectures have been formed to explain the remarkable differences in the sensible qualities of these bodies, by supposing some minute difference in their chemical composition: these conjectures have been often discussed, it will not be necessary therefore to dwell upon them. MM. Biot and Arago, from the high refractive power of the diamond, have supposed that it may contain hydrogen. I ventured to suggest in my third Bakerian Lecture, from the circumstance of its non-conducting power, and from the action of potassium upon it, that a minute portion of oxygen may exist in it; and in my Account of some new Experiments on the fluoric Compounds*, I hazarded the idea that it might be the carbonaceous principle combined with some new light and subtile element of the class of supporters of combustion. M. Guyton de Morveau, who conceived he had proved by experiments made fourteen years ago, that common carbonaceous substances were oxides of diamonds, from his last researches, conducted in the same manner as those of Messrs. Allen and Pepys, seems still inclined to adopt this opinion, though in admitting a much smaller quantity of oxygen than he originally supposed in charcoal; and he considers the diamond as pure carbonaceous matter, containing, possibly, some atoms of water of crystallization.

I have long had a desire of making some new experiments on the combustion of the diamond and other carbonaceous substances; and this desire was increased by the new facts ascertained with respect to iodine, which by uniting to hydrogen affords an acid so analogous to muriatic acid, that it was for some time confounded with that substance. My object in these experiments was, to examine minutely whether any peculiar matter was separated from the diamond during its combustion, and to determine whether the gas, formed in this process, was precisely the same in its minute chemical nature, as that formed in the combustion of common charcoal. I have lately been able to accomplish my wishes; I shall now have the honour of communicating my results to the Royal Society.

During a stay that I made at Florence in the end of March and beginning of April, I made several experiments on the combustion of the diamond, and of plumbago, by means of the great lens in the Cabinet of Natural History; the same instrument as that employed in the first trials on the action of the solar heat on the diamond, instituted by Cosmo III. Grand Duke of Tuscany; and I have since made a series of researches on the combustion of different kinds of charcoal at Rome, in the laboratory

* Phil. Trans. for 1814, part i. p. 72.—Phil. Mag. p. 93.

of

of the Academia Lyncei. In the first series I was honoured by the assistance of the Count Bardi, the Director, and Signior Gazzari, the Professor of Chemistry at the Florentine Museum: and in the last by that of Sig. Morrichini and Barlocci, Professors of the College Sapienza at Rome.

In the very first trials on the combustion of the diamond, I ascertained a circumstance that I believe has not been noticed before; namely, that the diamond, when strongly ignited by the lens in a thin capsule of platinum perforated with many orifices, so as to admit a free circulation of air, continues to burn in oxygen gas after being withdrawn from the focus. The light it affords is steady, and of so brilliant a red, as to be visible in the brightest sunshine; and the heat produced is so great, that in one experiment, in which three fragments of diamonds weighing 1·84 grain only were burnt, a fine wire of platinum used for attaching them to the tray was fused, and that some time after the diamonds were removed out of the focus.

The knowledge of this circumstance enabled me to adopt a very simple apparatus and mode of operation in my researches, and to complete, in a few minutes, experiments which have been supposed to require the presence of a bright sunshine for many hours.

My apparatus consisted of clear glass globes of the capacity of from fourteen to forty cubical inches, having single apertures to which stop-cocks were attached: a small hollow cylinder of platinum, which I use in experiments with the blow-pipe, was attached to one end of the stop-cock, and was mounted with a little perforated capsule of platinum for containing the diamond. When the experiment was to be made, the globe containing the capsule and the substance to be burned was exhausted by an excellent air-pump, and pure oxygen gas, made from hyperoxymuriate of potassa, admitted. The globe before and after the experiment was brought to the same temperature as the water over which the oxygen gas had remained. And as during the short time required for the combustion there was no sensible change either in the thermometer or barometer, no corrections for pressure or temperature were rendered necessary; the change of volume in the gas after the combustion, was estimated by means of a fine tube connected with a stop-cock, adapted by a proper screw to the stop-cock of the globe, and the absorption was judged of by the quantity of mercury that entered the tube, which afforded a measure so exact that no alteration, however minute, could be overlooked. As the elastic force of the vapour of water is the same at the same temperature, it was evident, that if any water formed in these experiments, it would be deposited as dew or mist in the globe; and I am convinced by direct trials,

that

that a quantity of moisture not capable of being weighed by a balance sensible to the $\frac{1}{100}$ of a grain, is rendered evident by deposition on a polished glass surface*.

The diamonds were always heated to redness before they were introduced into the capsule.

During the combustion of the diamond, the glass globe was kept cool by the application of water to that part of it immediately above the capsule, and where the heat was greatest.

In the first experiment, three diamonds, weighing together 1·63 grain, were entirely consumed, in a quantity of oxygen gas, more than three times as much as was necessary to convert them into carbonic acid. In this case, after the combustion had once commenced, it continued without a fresh application of the lens till there remained only a very thin piece of the largest diamond in contact with the capsule, and this by being brought into the focus rapidly disappeared. On restoring the globe to its original temperature, there was a very evident deposition of moisture; but on arranging the apparatus, so as to ascertain the change of volume of the gas, there entered only twenty-one grains of mercury. In this experiment, the cylinder of platinum had been fastened into the stop-cock by means of a small perforated cork: it seemed probable, when the small diminution of gas was considered, that the appearance of moisture might be owing to the production of vapour from this cork during the combustion, and the second experiment demonstrated that this was the case.

In this second experiment 1·84 grain of small diamonds were employed, and a glass globe of the capacity of 14·9 cubical inches. Soon after the capsule was placed in the focus in bright sunshine, the diamonds burnt with great brilliancy, and continued to burn till they had considerably diminished in bulk; but their splendour of combustion gradually became less, and before they had apparently lost half of their volume the process ceased. By placing them a second time in the focus, after agitating the globe so as to change their places, the combustion was again produced; but the light was much less vivid than before, and the combustion continued for a much shorter time. They were exposed to the concentrated rays a third and a fourth time; but after the fourth time they seemed incapable of burning, and though kept for some minutes in the focus, appeared to under-

* A piece of paper weighing a grain was introduced into a tube of about the capacity of four cubical inches, the exterior of which was gently heated by a candle; immediately a slight dew was perceptible in the interior of the upper part of the tube; the paper taken out and weighed immediately in the balance above referred to, had not suffered any appreciable diminution.

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go no further diminution: two fragments remained, which, as it was afterwards found, weighed ·52 of a grain; the barometer during the experiment was at 29·9 inches, the thermometer at 56° Fahrenheit. When the original temperature of the globe was restored, there was not the slightest appearance of vapour or humidity; the interior was as clear as before the experiment, and there was no solid matter of any kind separated in the tray. The fragments of diamond which remained were not black, but had lost their lustre like glass that has been acted on by fluoric acid, nor at any period of the process was any carbonaceous appearance perceived upon them. When the communication was made by the stop-cock between the interior of the globe and a surface of mercury, a minute quantity entered equal to 1·5 grain only.

A portion of the gas in the globe was transferred into a tube in the mercurial apparatus, and the oxygen it contained absorbed by the combustion of phosphorus; 3·5 parts of gas heated in this way left a residuum of 2·5 parts. A portion of the gas was agitated with lime-water, when seven parts out of ten were absorbed. I exposed the gas which remained after the combustion of phosphorus to several tests; it had not only the obvious characters of carbonic acid, but exhibited exactly the same chemical phænomena. Potassium strongly heated in it in a small glass tube over mercury, burnt with a dull red light, and formed an alkaline product of the same intense black colour as that produced by its combustion in the carbonic acid procured by the dissolution of marble: distilled water absorbed rather less than its own volume of the gas, and became subacid, sparkled by agitation, gained the taste and smell of a solution of carbonic acid in water, precipitated in the same manner lime-water, and when in excess redissolved the precipitate. To ascertain if this precipitate was exactly the same in composition as pure carbonate of lime, I made a sufficient quantity of it by pouring lime-water into the recipient containing the results of the first experiment; and after collecting and drying it at the temperature of 212° Fahrenheit, I introduced a quantity of it contained in some foil of platinum through mercury into a glass tube filled with mercury, and I heated in the same manner an equal quantity of finely powdered Carrara marble, and admitted to them equal quantities of solution of muriatic acid. In this trial, there was rather more elastic fluid disengaged from the Carrara marble than from the carbonate of lime from the diamond; but on examining the foil of platinum after the experiment, I found that a little of the carbonate had not been acted upon: I tried two similar experiments, substituting bibulous paper instead of the metallic foils for infolding the carbonates; the

the results were such as to show that both substances afforded the same quantities of elastic fluid.

I heated some of the carbonate from the diamond in a tube which contained potassium, and passed the potassium through it in vapour: there was ignition, and a substance of a dense black colour was formed; this substance was acted on by dilute muriatic acid, when it left a fine black powder which burnt like lamp black, and when thrown into fused nitre scintillated and disappeared in the same manner as powdered charcoal.

The gas that remained in the second experiment, after the absorption of the carbonic acid gas, vividly supported combustion, and diminished with nitrous gas; but as the degree of purity of the oxygen gas with which the globe was filled had not been determined before the experiment, it was impossible to ascertain with precision, that no elastic matter had been emitted during the process. To determine this point, I made a third experiment. A thin diamond weighing ·93 of a grain was introduced into the platinum capsule, which was placed in a globe filled with water and inverted in water; some oxygen gas, the last portion from the decomposition of hyper-oxymuriate of potassa, was thrown into the globe, so as to displace the water below the level of the capsule. The focus of the great lens was thrown upon the capsule, which with the diamond was instantly rendered dry by it, and the diamond soon entered into combustion and burnt as usual. After the process was finished, the carbonic acid was absorbed by lime-water, and the remaining gas, which equalled about one-third of the quantity of oxygen originally used, was compared analytically in several experiments with a portion of the same oxygen as that introduced into the globe, two measures of nitrous gas being added to a measure of each of the gases; the diminution was less by from $\frac{1}{100}$ to $\frac{2}{100}$ parts in the cases in which the gas that had been exposed to the action of the diamond was used; but this minute difference is what might have been expected, and which indeed could not fail to exist, when it is considered that, during the absorption of carbonic acid gas by water and lime water, a small quantity of common air is always expelled from the water.

In this last experiment a small fragment of diamond remained unconsumed, which was similar in appearance to that mentioned in the second experiment, and its colour, which was originally yellow, was rendered rather darker. In no one of the three experiments was there any distinct appearance of carbonization, when the process was stopped in consequence of the impurity of the gas; though the diamonds were of various colours and different lustres.

A piece of plumbago from Borrowdale in Cumberland, weigh-

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ing two grains, was exposed in the focus in the same manner as the diamond in the first and second experiments, having been previously heated red; the quantity of oxygen gas employed was 8·5 cubical inches: more than half the plumbago remained unconsumed, and during the combustion some brown ashes were produced. The phænomena in this experiment were very different from those observed in the experiments on the diamond, the gas became clouded during the process, and there was a considerable deposition of dew on the interior of the globe. When the original temperature of the globe was restored, and the stop-cock opened, 96·6 grains of mercury entered, and drops of moisture even were observed condensed on the sides of the vessel.

In the second series of experiments, charcoal formed by the action of sulphuric acid on oil of turpentine, and some produced during the formation of sulphuric ether, from which nitric acid had been distilled, and which afterwards had been strongly ignited, and charcoal of oak which had undergone the same process, were used.

Three grains of the charcoal from turpentine were employed, 2·5 of that from alcohol, and five grains of the charcoal of the oak: in all these instances of combustion the gas became clouded during the combustion, and when the original temperature of the globe was restored, moisture was found condensed in the interior; much the largest quantity in the experiment on the charcoal of oak, and the least in that on the charcoal procured from oil of turpentine. The charcoal from oak left a residuum of white ashes, which was principally carbonate of lime; that from oil of turpentine produced no residuum; that from alcohol, which was formed in a common process of the manufacture of ether, left a minute quantity of ashes, probably owing to the impurity of the sulphuric acid employed.

The quantity of mercury which entered the apparatus indicating the change of volume of the gas, was in the experiment on the charcoal of oil of turpentine .... 107·5 grains

In that on the charcoal of alcohol .... 194·5

In that on the charcoal of oak .... 513·3

From the results of these different experiments, it appears evident, that the diamond affords no other substance by its combustion than pure carbonic acid gas; and that the process is merely a solution of diamond in oxygen, without any change in the volume of the gas; for the slight absorption in the second experiment is scarcely more than a compensation for the volume occupied by the diamonds consumed.

It is likewise evident that in the combustion of the different kinds of charcoal, water is produced; and from the diminution

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of the volume of the gas, there is every reason to believe, that the water is formed by the combustion of hydrogen existing in the charcoal; and experiments which I have referred to, or detailed in my third Bakerian Lecture, prove the presence of hydrogen in common charcoal; and as the charcoal from the oil of turpentine left no residuum, no other cause but the presence of hydrogen can be assigned for the diminution occasioned in the volume of the gas during its combustion.

M. Guyton de Morveau* has noticed the production of water during the combustion of plumbago from Keswick; and from these experiments it is most probable that it is formed in the process of combustion, for it is unlikely that water should remain in union with plumbago at a red heat; and in the various experiments that I have made on the ignition of plumbago by Voltaic electricity, I have never perceived the separation of any moisture, or the production of any gas; so that it seems most likely that it contains intimately combined hydrogen. It cannot be supposed that water exists in it in union with oxide of iron, for in this case there would be no obvious cause for the diminution of the volume of the gas; and all analogy is in favour of the conclusion that the iron is in a metallic state.

The general tenor of the results of these experiments is opposed to the opinion, that common carbonaceous substances differ from the diamond by containing oxygen; for in this case they ought to increase and not diminish the volume of oxygen: nor, on the other hand, is it favourable to the supposition that the diamond contains oxygen, for the difference in the quantity of carbonic acid produced in the different experiments, is no more than may be reasonably ascribed to the generation of water, in the combustion of the common carbonaceous substances; and the results of the experiments, to which I have referred in the beginning of this paper on the action of potassium on the diamond, may be easily accounted for from other circumstances†.

That charcoal is more inflammable than the diamond may be explained from the looseness of its texture, and from the hydrogen it contains; but the diamond appears to burn in oxygen with as much facility as plumbago, so that at least one distinction supposed to exist between the diamond and common carbonaceous substances is done away by these researches.

A fact which I formerly noticed, the blackening of diamond

* *Annales de Chimie*, tome lxxxiv. p. 241.

† See Bakerian Lecture for 1808. Potassium decomposes the silica in glass by being heated in contact with it; and in the case in which equal quantities of potassium were long heated in glass tubes, the one in contact with diamonds, the other alone, that in contact with the diamonds must have acted upon a much greater surface of glass.

by

by the long continued action of heated potassium, induced me to suspect in the beginning of these inquiries, that common charcoal might owe its colour, opacity, and conducting power, to the circumstance of its containing minute portions of the metals of the alkalies, or earths, and plumbago to the iron it contains; but when I found that charcoal made from oil of turpentine, which left no residuum in burning, and charcoal precipitated from carburetted hydrogen gas by chlorine, had the same properties, it was necessary to renounce this opinion.

The only chemical difference perceptible between diamond and the purest charcoal, is, that the last contains a minute portion of hydrogen; but can a quantity of an element, less in some cases than $\frac{1}{50000}$ part of the weight of the substance, occasion so great a difference in physical and chemical characters? This is possible, yet it is contrary to analogy; and I am more inclined to adopt the opinion of Mr. Tennant, that the difference depends upon crystallization. Transparent solid bodies are in general non-conductors of electricity, and it is probable that the same corpuscular arrangements which give to matter the power of transmitting and polarizing light, are likewise connected with its relations to electricity; and water, the hydrates of the alkalies, and a number of other bodies which are conductors of electricity when fluid, become non-conductors in their crystallized form.

The power possessed by certain carbonaceous substances of absorbing gases, and separating colouring matters from fluids, is probably mechanical and dependent upon their porous nature; for it belongs in the highest degree to vegetable and animal charcoal, and it does not exist in plumbago, coke, or anthracolite.

The same conclusions respecting the composition of carbonic acid may be drawn from these experiments, as from those of Messrs. Allen and Pepys, and Theodore de Saussure. If the calculations be founded upon the difference of the weights of oxygen and carbonic acid gases, which appears the most exact method, carbonic acid gas will contain, according to the estimate of the mean specific of the gravities of the two gases given by M. Theodore de Saussure*, thirty parts of oxygen, or two definite proportions, to 11·3 parts of carbon, or one definite proportion.

Supposing the diminution of the oxygen produced in the experiments on the common carbonaceous substances entirely occasioned by the formation of water, it is easy to calculate the proportions of hydrogen in them; but in the case of plumbago

* *Annales de Chimie*, tome lxxi. pag. 261. This estimation is the same as that I have given. Elements of Chem. Phil. pag. 305.

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there is probably a diminution of oxygen, from the oxidation of the iron; and it is not certain that the ashes afforded by the charcoal from vegetable substances exist in it in the state of earths and alkalies: and as the quantity of hydrogen varies according to the degree of heat to which charcoal has been exposed, it is almost useless to attempt to assign its proportion in any particular case, especially when the largest portion is so extremely minute.

The nature of the chemical difference between the diamond and other carbonaceous substances may be demonstrated by another process, namely, igniting them in chlorine; when common well burnt charcoal, or plumbago from Cumberland, is intensely ignited in chlorine, white fumes are immediately perceived in consequence of the production of muriatic acid gas by the hydrogen, which acid precipitates the aqueous vapour in the chlorine; but the diamond occasions no such effect. A small diamond, weighing ·45 of a grain, was kept in a state of intense ignition by the great lens of the Florentine Museum for more than half an hour; but the gas suffered no change, and the diamond had undergone no diminution of weight, and was not altered in appearance.

Charcoal, after being intensely ignited in chlorine, is not altered in its conducting power or colour; and this circumstance is in favour of the opinion, that the minute quantity of hydrogen is not the cause of the great difference between the physical properties of the diamond and charcoal.

LXVIII. *New Outlines of Chemical Philosophy.* *By* Ez. Walker, *Esq. of Lynn, Norfolk.*

[Continued from p. 355.]

What has been advanced upon evaporation in my last paper, receives considerable support from a series of experiments carried on by Mr. Six, to investigate the variations of local heat.

On December 19, 1783, this gentleman disposed of three thermometers, of a peculiar construction, in the following manner; one in his garden; one on the top of St. Thomas's hill, at fifteen feet from the ground, which was nearly on a level with a third placed on the top of the high tower of Canterbury cathedral, at about 220 feet from the ground. From observations taken with these instruments he found, that

"The local variations in the day time seemed to be regulated by the general degree of heat only, without being affected by any other particular disposition of the atmosphere, or the clear-ness

ness or cloudiness of the sky, as the nocturnal variations were. In the month of September, when the glasses rose from 60° to 70°, the heat at the lower station constantly exceeded the heat at the upper station; and in some measure proportionably, as the weather was hotter*.

"That the clearness of the sky should contribute to the coolness of the air in the night, is not at all surprising; but that, whenever the sky becomes clear, the cold should seem to arise from the earth, and be found in the greatest degree, as long as it continues clear, in the lowest situation, seems a little extraordinary: this however seemed to be the case, both in the warmer and in the colder weather, during the whole time these observations were taken. About noon, on the 3d of January, the sky becoming clear, the air got cooler; and going into my garden about eight in the evening, I perceived the surface of the ground, which had been wet by the rain in the forenoon, begin to be frozen. Looking immediately at the thermometer, I saw the mercury at $33\frac{1}{2}$°; and observing a piece of wet linen hanging near the glass, not five feet from the ground, I took it into my hand, and found it not in the least frozen; by which it appeared, that the degree of cold which had frozen the surface of the ground, had not then ascended to the glass, nor to the linen, and consequently had not been communicated to the air five or six feet above the earth. The next day I found, as expected, a considerable local variation; the index for the cold of the night in the garden being at 32°, that on the hill being $35\frac{3}{4}$°, and that on the top of the town at $37\frac{3}{4}$°. Probably the weather did not continue clear the whole night: if it had, it is likely the degree of cold would have been found proportionably greater at every station. On the morning of the 4th, there fell a misty rain, which continued only till noon, when the sky became clear again, and continued so till the 7th; during which time the nocturnal heights of the thermometers differed considerably from each other; but on the sky's becoming cloudy, the local variation ceased †."

To confirm in a more particular manner the foregoing observations respecting a remarkable refrigeration, which in clear weather takes place in the night near the earth's surface, Mr. Six continued his journal, with the omission of a few days only, from July 1784 till July 1785.

* "As the heat at the lower station exceeded the heat at the upper ones, when the weather was hot; and equally so, when the sky was cloudy, as well as when it was clear; it appears, that the glass at the lower station was not materially affected by the reflection of the sun's rays from the earth, as at first I apprehended it would be."

† Phil. Trans. abr. vol. xv. p. 609.

"From these observations it appears, that a greater diminution of heat frequently takes place near the earth in the night-time, than at any elevation in the atmosphere within the limits of Mr. Six's inquiry; and that the greatest degrees of cold are at such times always found nearest to the surface of the earth; that this is a constant and regular operation of nature, under certain circumstances and dispositions of the atmosphere, and takes place at all seasons of the year; that this difference never happens in any considerable degree but when the air is still, and the sky perfectly unclouded; but when the moistest vapour, such as dews and fogs, did not, as far as he could perceive, at all impede, but rather increase, the refrigeration. In very severe frosts, when the air frequently deposits a great quantity of frozen vapour, he generally found it greatest; but the excess of heat, which in day-time, in the summer season, was found at the lower station, in the winter diminished almost to nothing *."

The truth of these experiments may be verified by a few familiar observations.

When the moisture in the air begins to be frozen in the night, hoar-frost is seen upon the bare ground before it is formed upon the grass, trees, and the roofs of houses; but in the morning it vanishes from the bare ground some time before it ceases to appear upon the grass and higher objects. Hence it is evident, that under certain circumstances, the air is usually some degrees warmer by day and colder by night, close to the ground, than at a few inches above it.

These variations of local heat are the effects of that universal law of nature which causes the ordinary vicissitudes of temperature, that are daily taking place in our atmosphere.

It has already been advanced in these outlines, that no increase of temperature can be produced unless thermogen and photogen be united to matter †.

In the present instance, the solar influence increases the temperature of the air, and the thermogen that it contains. This element, having its temperature increased, attracts the photogen from the earth: hence a greater degree of heat is generated upon the surface of the ground than at a few feet above it.

But as soon as the sun descends below the horizon, the temperature of the thermogen contained in the air diminishes, and its attraction for photogen gradually decreases till the attraction of the earth for that element commences, in consequence of which the photogen returns to the earth, and a diminution of temperature takes place upon the surface of the bare ground,

* Phil. Trans. abr. vol. xvi. p. 404. † Phil. Mag. vol. xliii. p. 349.

before

before any alteration is perceived upon objects which stand only a few inches above it.

Lynn, Dec. 16, 1814. EZ. WALKER.

[To be continued.]

LXIX. *On the Sublimation of Silica.* *By* J. MACCULLOCH, *M.D. F.L.S. Chemist to the Ordnance, and Lecturer on Chemistry at the Royal Military Academy at Woolwich. V. Pr. Geol. Soc.**

SOME years ago, being in pursuit of another object, a mixture of the oxides of tin and lead was exposed to the heat of an air furnace in an English crucible, to the top of which was luted another of the same sort. This apparatus was allowed to remain in the fire for some hours. No account of the heat was taken, but I have on former occasions produced in the same furnace a heat sufficient to contract one of Mr. Wedgwood's original clay pieces to the 130th and 140th degree of his scale. On removing the crucibles, the insides of both from the bottom of the lowermost to within a third part of the top of the uppermost, were found covered with white brilliant filamentous crystals crossing each other in all directions. I concluded that they consisted of the oxide of tin, or perhaps that of lead, and subjected them to the obvious experiments necessary for ascertaining this circumstance. Failing to confirm this supposition, I then conjectured that they might consist of silex. The quantity I procured scarcely amounted to half a grain, and I therefore divided it into two parts, that I might have the satisfaction of confirming or refuting my own trials by comparison with those of some chemical friend. Mr. Aikin was so good as to undertake the examination of the reserved portion, and from his well known accuracy, the Society will naturally place confidence in our mutual results. On igniting them in successive portions of borax and of pure potash, they were dissolved. The solution was then neutralized, and a few light flakes fell down, which were redissolved in muriatic acid. This solution being evaporated to a transparent jelly, was ignited by the blow-pipe, and became insoluble in acids. I was very desirous of obtaining a second specimen, and repeated the same process many times for that end, but in vain. I cannot pretend to account for this accidental appearance, and only regret that I was unable to ensure it at will. There can be no doubt that they were crystals of silica, however

* From the Geological Transactions, vol. ii.

difficult

difficult we may find it to form them at pleasure, and the rarity of the occurrence only serves to prove that there are properties and relations of this substance with which we are as yet unacquainted. An agreeable confirmation of this fact appeared some time after in an observation of Vauquelin, copied in Tilloch's Journal for 1809, with which the members of this Society are doubtless well acquainted. In a geological view it may perhaps be worthy of record, as not only establishing the volatility of silica, but serving to prove that this substance may be crystallized from the state of vapour, as sulphur, some neutral salts, and some metals are known to be. How far this property of vaporization and crystallization from that state may be possessed by the other earths, or by earthy compounds, as it undoubtedly is by all the metals, must be determined by future observations. Possibly we may thus gain a step on which to rest, in the investigation of the difficult subject of mineral veins, and the arrangement of the crystallized substances which occupy their cavities.

The possibility also of explaining by this process the crystallization of the delicate filamentous zeolites which occupy the cavities of amygdaloids, will readily occur to every mineralogist.

LXX. *On Electricity.* *By* Francis Ronalds, *Esq.* *Communicated by the Author.*

To Mr. Tilloch.

Sir,—By a paper of Mr. De Luc, in your Number for October last, I perceive that he has rather misconceived the inferences which I drew from some experiments on the variable action of the electric column, described in your Number 194; having stated my opinion to be, that moisture has very little effect on that action. He seems also to imagine that the results of those experiments were intended to be opposed to his, and refers to some which were made with Dr. Lind, to prove that dryness diminishes the action.

I cannot discover, however, that I have drawn such a conclusion, and had no intention to dispute with a philosopher of so great and universally acknowledged acuteness, the truth of a fact which his own elegant experiments have fully established. Had not the last paragraph of the paper above mentioned escaped Mr. De Luc's attention, I think the mistake could not have happened. I there state as a probable cause of the power of the column supported with three stems of glass, not having been increased by a rise of temperature in its usual degree,

degree, that the disk of paper had possibly been deprived of a *part of the water which appears necessary to its action* by the alkali.

Mr. De Luc's valuable experiments and observations lead to the conclusion, that the presence of water, or of some conducting fluid, in the substances which have been hitherto interposed between the metals, is necessary to the accumulation of electricity. Whether this effect is occasioned by the presence of water only, because a conducting fluid is essential? which I take to be his opinion; Whether water acts merely as a conductor, differing in some unknown electric relation from the metals? which I imagine to be that of Mr. Singer; or, Whether any kind of decomposition is necessary? are questions not yet determined; but I have no doubt the researches of those intelligent gentlemen will contribute very materially to elucidate the subject.

Mr. De Luc also supposes, of course, that I have not been aware of the effect of a moist atmosphere, to increase the power of the column: but I was induced to place moistened cards in the receiver with the column and hygrometer, in order to compare the effect of moisture on both instruments, suspecting, certainly not being fully aware of its influence. I was also induced by the observations of Mr. De Luc in the paper above referred to, to undertake the following experiment, for the purpose of examining, more closely, this influence; and, if possible, of ascertaining what increments of moisture coincided with different electrical intensities.

About half the interior surface of a tall glass receiver was covered with a piece of linen. A column of about 800 groups of zinc, silver, and paper, connected by a silken cord passing through their centres, was suspended in this receiver, by a copper wire attached to the arm of a balance, and the balance was insulated and connected with an electrometer. Thus I could weigh the water which was absorbed by the column when the air of the receiver was rendered moist, by moistening the linen from the upper opening; and the insulation of the balance was so well preserved, that I could produce extreme moisture in the receiver, without sensibly injuring it; which was ascertained by means of an electrometer charged much more highly than the column could at any time charge the other. But the silken cord, no doubt, became more conducting as moisture penetrated it; to which circumstance I attribute some of the results which might otherwise appear anomalous. The divergence of the electrometer was measured by an arc placed under the gold-leaves, and divided into tenths of an inch: an hygrometer and thermometer were also placed in the receiver.

Time.

Time.	Weight.	Elect.	Hygr.	Therm.
H. M.	OZ. GRS.			
Nov. 12. 3 20 P.M.	3 170	8	50½	57
4 30	3 172	11	71	57
6 0	3 176	14	82	54
11 0	3 184	12	80	54
13. 11 30 A.M.	3 190	12	90	51

This experiment seems to prove that water had produced its utmost effect on the intensity of the electricity when the column weighed 3 oz. 176 grs. at the temperature of 54; but I observed that when the column was driest, if the electricity was taken off by touching the cap of the electrometer, it rose again to its former divergence rather slowly; and when it was taken off whilst the column contained most moisture, it rose to the greater part of its former divergence almost as quickly as the hand could be withdrawn. My next object was therefore to compare the increments of moisture with the frequency of the divergence, which I conceive to be a measure of the quantity of electricity arriving at a certain intensity which the column can give off. It was therefore detached from the balance, and suspended before a fire one hour and a half, when the effect on the electrometer was nil; it was then replaced upon the balance, and the following results were obtained.

Time.	Weight.	Elect.	Hygr.	Therm.	Time required to recover diverg.
H. M.	OZ. GRS.				
Dec. 8. 1 30 P.M.	3 49	0	61	55	
2 15	3 55	2	60½	55½	3 minutes
3 15	3 60	10	61	55	3½ ditto
4 15	3 65	10	61	55½	1 ditto
10 30	3 72	10½	66	55½	1 ditto
11 0	3 73	10½	66	55½*	
9. 11 30 A.M.	3 75	9¾	63	54	30 seconds

To avoid the objection which might be made to the silk thread, I constructed a short column whose extremities were pressed by brass plates, which were connected by glass stems covered with sealing-wax in such manner that it did not touch them, and was surrounded by a cylinder containing a moistened piece of linen, so that the column was submitted to the action of an atmosphere of perfect humidity; but the insulation of its supports was perfectly preserved. But as I could not conveniently weigh this column, and as it was necessary to have its disks of larger

* I touched the electrometer ten times during the preceding ten minutes, and found the divergence fully recovered to 10½ at 11h. 1m.

diameter

diameter and more pressed together, the effects were much less striking, although they were fully sufficient to prove that no more electricity escaped by the insulators or the air from the upper extremity when the latter was moistened than when it was drier.

I would draw from these experiments the following conclusions; but I do not wish to insist very confidently on their validity.

1. The quantity of electricity arriving at a certain intensity, which a column of this kind is capable of manifesting, as measured by the vibrations of the pendulum apparatus, or the strikings of a gold-leaf electrometer, in a given time, is great in proportion to its degree of moisture within certain limits.

2. Under certain circumstances this greater quantity, thus manifested in a moist state of the column, does not produce greater intensity than when it is in a drier state.

3. Since electricity does not pass off in one of the above cases by the insulators or the air more copiously than in the other, it must pass by the water, which is the cause of its accumulation; otherwise the intensity would be proportional to the quantity.

4. If the last proposition is correct, the great quantity and low intensity of the electricity produced by Voltaic batteries charged by acids and salts, may be attributed to a similar cause.

I am, sir, your humble servant,

Hammersmith, Dec. 16, 1814. FRANCIS RONALDS.

LXXI. *On Staffa.* *By* J. MACCULLOCH, *M.D. F.L.S. Chemist to the Ordnance, and Lecturer on Chemistry at the Royal Military Academy at Woolwich. V. Pr. Geol. Soc.**

If the "Description and Natural History" of Staffa, by Faujas de St. Fond, or the various other descriptions which have been published of this island by naturalists and by tourists, had exhausted the subject, I should have forborne to have troubled the Society with any remarks on a place which ought now to be well known.

But a visit to this celebrated island having given me an opportunity of remarking a circumstance before unnoticed, and of some importance in its natural history, I think it my duty to lay it before the Society. In so doing, I find it difficult to avoid entering rather minutely into the general description of the island, particularly since a second examination, besides confirming the remarkable fact I at first noticed, has enabled me to investigate its structure more completely. I shall doubtless still leave something to be corrected by those who may come after me. A

* From the Geological Transactions, vol. ii.

multiplicity

multiplicity of objects pressing at once for regard, a visit always necessarily hurried from the impossibility of remaining long on the island, a boisterous sea, and a stormy atmosphere, are hostile to that accuracy of observation which may preclude future corrections.

The circumference of Staffa is estimated at about two miles. It forms a sort of table land of an irregular surface, bounded on all sides by perpendicular cliffs, varying in altitude and broken into numerous recesses and promontories.

It is intersected by one deep cut, scarcely to be called a valley, which divides the higher and more celebrated columnar part from the remainder of the island. At the highest tides this more remarkably columnar part which forms its southwestern side, appears to terminate almost abruptly in the water, but the retiring tide shows a causeway of broken columns forming a sort of beach at its foot. Round the other sides of the island there is also a beach of varying breadth, consisting of detached fragments, and of rocks jutting out into the sea in many irregular directions. This beach, when the weather is perfectly calm, and the swell off the shore, will, under due precautions, afford landing in various places, but it is on the eastern side that the most numerous landing places occur. Various narrow creeks sheltered by the island itself from the predominant western swell, admit of easy access in moderate weather, provided the wind is in any direction from SW. to NW. And for the encouragement of the mineralogist, who may be terrified at the exaggerated reports of this difficulty, I can assure him that I have landed on Staffa when the vessels that navigate this sea have had their sails reefed, and the boatmen of Iona and Ulva have called it impracticable. The love of the marvellous has conferred on Staffa a terrific reputation, which a greater resort has discovered to be somewhat akin to that of Scylla and Charybdis.

It is easy to perceive from the southward, that with this flat disposition of its surface, and notwithstanding its irregularities, Staffa possesses a gentle inclination towards the N.E. although no opportunity is afforded for ascertaining the precise dip. It is not of importance to ascertain it, nor can it amount to more than five or six degrees of variation from the horizontal plane.

The highest of the perpendicular faces which bound it, rise about 60 or 70 feet above the high-water mark, and these are on the south-western side, where the most remarkable columns and where the great caves exist.

The greatest elevation of the island cannot be more than 120 feet above the level of the sea. There are no sunk rocks round it; but the water deepens rapidly from the shore, and admits of large vessels coasting it close at hand, provided they have a leading wind. There

There is a soil of considerable depth on the surface, and it is covered with herbage.

It is almost superfluous to say that the whole island consists of a mass of basalt. I have indeed been told, that a sandstone bed has been seen at low water on the southwestern side, but I had not an opportunity of observing it. This is the part of the island, where, if in any place, it should, from the inclination of the strata, be perceived; and there is no reason to doubt the assertion, as we find most of the trap rocks of the Western Islands lying on beds of sandstone. Is is equally superfluous to describe the basalt, since specimens of it are in every one's possession. It may be sufficient to remark, that its texture is more compact, more crystalline, and less earthy than that of basalt in general, and that it is at the same time less homogeneous, less black, more fragile, and more sonorous. But it would be idle to attempt to apply different terms to the endless varieties of the rocks of this tribe.

This basalt exhibits two modifications: the columnar, so often described, and the amorphous, which is generally more or less amygdaloidal, containing imbedded zeolites of different sorts. I saw no examples of basaltic breccia, or of trap tuff, as it is improperly called.

It is in the amorphous basalt that the zeolites are most abundant. The nodules vary from the size of a pea to that of a hen's egg and upwards, and generally exhibit specimens of radiated mesotype and of analcime. The cubical zeolites (chabasite) are of rare occurrence, and the mesotype is seldom granular, and never, as far as I saw, capillary. The lamellar variety of stilbite is occasionally found filling the intervals of approximate columns. I did not observe any zeolites in the larger and more perfect columns, but in the smaller and more irregular ones they occur, though rarely.

If we were to view the island only from the southwestern side, and at half tide, we should conclude that it has been formed of three distinct deposits, or beds of basalt. Of these the lowermost appears in some places amorphous, but it is not easy to see enough of it to judge whether it actually forms a continuous bed. It is only from the analogy of Canna, and the other basaltic islands of this sea, that we should be tempted to generalize this conclusion.

The next bed is that which is divided into those large columns which form the most conspicuous feature of Staffa, and it varies from 30 to 50 feet in thickness. The upper one appears at a distance to be an uniform mass of amorphous basalt; but on a nearer inspection it is found to consist of small columns, laid and entangled in every possible direction, often horizontal, and generally

generally curved. It is this bed which forms the ponderous cap (as it is called) which crowns the summit of the grand *façade*.

Although the great columnar bed occupies but a small portion of the whole exterior face of the island, the columnar form is perhaps predominant throughout the whole. Yet it would be equally difficult, as useless, to attempt to determine its proportion to the amorphous part where they are irregularly mixed, as they are at the northern and eastern sides. On these sides also the division into distinct beds such as I have described above, is by no means easy to trace, and possibly it does not exist.

To those who have seen the beautifully regular columns of the Giant's Causeway, those of Staffa will appear rude and comparatively shapeless. They no where exhibit that accuracy of design which is so conspicuous in the former, and are rarely seen of any considerable length without some incurvation. But their thickness is much greater, since they often attain a diameter of four feet. They vary perpetually in the number of their angles, the pentagonal and hexagonal being the most common, and those of an inferior number of angles being less common than those of a superior. Their joints are very irregularly placed, and are frequently wanting through a considerable length. When separated, the touching surfaces are either flat, or marked by a slight respective concavity and convexity. In many places, and most conspicuously in the great cave, the angles of the upper joint are considerably and obliquely truncated at the point of contact with the lower one. But I did not perceive any instance where a corresponding projection of the end of the inferior angle rose to cover the truncation, a circumstance of such frequent occurence at the Giant's Causeway. I may add, that the articulated columns are most remarkable in the great cave, and that the straightest columns generally exhibit the most frequent articulations. The curved columns visible at the cave called the Clamshell cave, extend for 40 or 50 feet without a joint.

The disposition of the variously curved columns above this small cave, is perhaps one of the most striking features of the whole island. But it will be time enough to speculate on the formation of a curved basaltic column, when we have something rational to offer on that of a straight one.

A very extraordinary aggregation of columns lies off this cave, forming a conical detached rock, corruptly called Boo sha la. The Gaelic name Buachaille (*Βουκόλος?*) the herdsman, is commonly applied to conspicuous single rocks all over the country. This rock consists of variously inclined columns resting against each other, and meeting till they form a conical body, which appears to repose on a bed of curved and horizontal columns.

It

It is superfluous to attempt a description of the great cave. The language of wonder has already been exhausted on it, and that of simple description must fail in an attempt where hyperbole has done its utmost. I may however remark, that its dimensions appear to have been over-rated, in consequence of the mode of measurement adopted, and that the drawings of it which have been engraved, give it an aspect of geometrical regularity which it is far from possessing. Its superiority in point of effect to the greatest efforts of architecture, might admit of dispute if there were any disputing about feelings. Another cave occurs at a short distance westward, of inferior dimensions, and inaccessible unless when it can be entered in a boat, an event requiring a combination of circumstances of no very common occurrence at Staffa. Large fissures are seen above this cave, with an incipient detachment of considerable masses, threatening a ruin which is perhaps not far distant. Beyond this there is still another cave which appears to pass through the promontory in which it lies, but equally or even more difficult of access, and still involved in uncertainty. Many other caves of less note are to be seen in various parts of the cliff around the island, into which the sea breaks with a noise resembling that of heavy and distant ordnance.

In a letter transmitted last year to the Secretary of this Society, I took notice of a fact of considerable importance in the natural history of this island, which had before escaped the remarks of visitors. This is, the occurrence of a bed of alluvial matter on some parts of its surface, containing fragments of the older rocks. It is most easily seen at that side of the island which faces Iona, and on the summit of the cliffs of a semicircular bay opening in that direction. The bed is here broken at the edge of the cliff, so as to expose its whole thickness for a considerable extent. But the same appearance may also be observed immediately above the ordinary landing place, where the bed has also been broken. The stones which it contains are all rounded, and of various, often considerable dimensions, and they exhibit specimens of granite, gneiss, micaceous schistus, quartz, and red sandstone. Together with these, are some rolled pieces of basalt.

Here then is a circumstance in the mineral history of Staffa, adventitious it is true, but involving difficulties of no small importance. If we cast our eyes on the map, we shall perceive that it is embayed in a large sinuosity formed in the island of Mull, and nearly inclosed on the opposite side by Iona and the Treshanish islands. Beyond the latter, a second line is drawn by Tirey and Coll; while to the north, but at a greater distance, are placed the islands of Muck, Rum, Egg, Canna, and

 Sky.

Sky. The whole island of Mull, with the exception of the Ross, is of a trap formation, containing however some partial tracts of sandstone and other rocks which I need not notice. The islands of Ulva and the Treshanish, with their dependent rocks, are also of trap formation. So are the islands which lie to the north, and which I have enumerated above. Iona however, together with Coll and Tirey, consists principally of gneiss and mica slate traversed by granite veins, rocks which also form the chief parts of the coasts of Lorn, Appin, Morven, and Ardnamurchan.

It is to the former, then, that we must look for the origin of the rolled stones which cover Staffa, if, limiting the great operations of nature by our own narrow views, and the ages which have contributed to change the face of the globe by our own short span, we are led to seek for that solution which may appear the least difficult. Even then, we must admit that Staffa has formed part of one continuous land with the islands of Coll, Tirey, and Mull, since no transportation could have been effected without the existence at some period of a continuous declivity between them.

The language which this circumstance speaks is not obscure, and the nature of these changes allows of little dispute. If we admit this obliteration of so large a portion of solid land, and consider that a deep sea now rolls above the foundations of former mountains, we have no further difficulties to obstruct us in accounting for the numerous and distant accumulations of transported materials which occur over the whole surface of the earth. The same power, whatever it was, that hollowed the great sinuosity of Mull, might well remove the solid matter that once filled the valleys which now separate Mont Blanc from the ridge of Jura.

But if, appalled at the supposed magnitude of those changes, and at the period of time which must have elapsed to complete them, we suppose that the island of Staffa was elevated from the bottom of the sea in its present detached form, and retaining on its summit a portion of the bed of loose matter deposited under the present waters, another order of phænomena crowds on us no less important, and involving circumstances almost equally repugnant to the visible operations of nature.

The appearances are perhaps insufficient to enable us to decide between two difficulties of equal magnitude, nor is it here necessary to enter further on that question. I may also leave it to those who have engaged more deeply in such investigations, to determine whether, in the supposition of the first of these causes, whether the wasting of the land has arisen from the gradual action of natural operations, or the more violent efforts of an occasional

occasional destroying force. It is my humble task to point out a fact, as a contribution to that mass of accumulating information on which a consolidated fabric may at some future time be erected. Yet the idle spectator or enthusiastic lover of Nature, who shall hereafter view this interesting spot, may, when he contemplates these grand revolutions, learn to wonder less at the efforts of that power which has hollowed the cave of Fingal, and submerged in the depths of the ocean those columns which seemed destined for eternity.

LXXII. *An Account of some Experiments on Animal Heat.* By John Davy, *M.D. F.R.S.**

The recent inquiries of Mr. Brodie have rendered questionable the different prevailing hypotheses relative to animal heat, and have shown that fresh investigation is necessary, before we can expect to arrive at any accurate theory.

In the present uncertain state of our knowledge, three circumstances are particularly deserving of attention, viz. the relative capacities of venous and arterial blood for heat, their comparative temperatures, and the temperatures of different parts of the animal body.

On the first of these subjects we possess only the experiments of Dr. Crawford, which I believe have not yet been repeated, notwithstanding they form the basis of his hypothesis.

On the second, little inquiry has been made, and especially of late years, since the improvement of the thermometer.

And on the third, the observations that have been collected are very few in number, and, with the exception of those of Messrs. Hunter and Carlisle, are scarcely, perhaps, deserving of confidence.

Such were the inducements that led me to the consideration of each of these subjects apart, and to endeavour to acquire by experiment some more certain knowledge respecting them. The experiments that I have made will be described in the two following sections, and in the last will be offered the few remarks and conclusions which naturally arise, and are fairly deducible from the results.

1. *On the Capacities of venous and arterial Blood for Heat.*

I must premise, that my object has been to endeavour to ascertain the relative capacities of venous and arterial blood for heat, rather than their exact specific caloric. The latter, from many circum-

* From the Philosophical Transactions for 1814, part ii.

circumstances, is difficult to be accomplished; whilst the former is comparatively easy, and in a theoretical point of view is probably equally useful.

I have employed both the methods commonly used. I shall mention most of the experiments that I have made, without noticing the repetitions of them, and shall begin with those on the times of cooling of equal volumes of venous and arterial blood.

The blood used was from the jugular vein and the carotid artery of a lamb about four months old. It was received in bottles; and to remove the fibrin, which is a great impediment in experiments of this kind, it was immediately stirred with a wooden rod. In respect to colour, the difference between the venous and arterial blood was not so great as in the sheep's; and this in a great variety of instances I have always observed, the venous being of a less dark hue. The specific gravity of the venous blood, without the fibrin, was found to be 1050, and that of the arterial 1047.

A glass bottle equal in capacity to 2518 grains of water, and weighing 1332 grains, was filled respectively with water and venous and arterial blood of the temperature of the room 62, about four hours after the blood had been drawn, during which time each bottle had been closely corked. A delicate thermometer, by means of a perforated cork, was placed in the middle of the liquid. The bottle was then plunged into water of the temperature 140 Fahrenheit; and when the mercury had risen to 120, the bottle was quickly wiped and suspended in the middle of the room, and the progress of cooling was noticed every five minutes, till the thermometer had fallen to 80. The following were the general results obtained:

Water cooled from 120 to 80 in	91 minutes
Arterial blood in	89
Venous blood in	88

Considering therefore the capacity of water for heat to be denoted by 1000, neglecting the effect of the glass bottle producing a difference only of about half a minute, and the same in each instance, and dividing the times of cooling by the specific gravity, the relative capacities of venous and arterial blood without fibrin appear to be as ·921 and ·934.

In the following experiments the same kind of blood and the same quantity was used as in the preceding. The mixtures were made in a very thin glass receiver containing a delicate thermometer. The temperature of the room was 66.

Hot water temperature 121; cold water 61. Mixture of the two 90, after two minutes 89, after three 88, and after eight 87.

Venous

Venous blood 121. Water 62·5. Mixture 89; after three minutes 88·5; after seven 87.

Arterial blood 121. Water 63·5. Mixture 89·5; after three minutes 88·5, and after seven 87.

Now, allowing about one degree of the cooling effect to have been produced by the receiver indicated by the admixture of the hot and cold water, calculating the quantity of blood used from the knowledge of its volume and specific gravity, employing the formula given by Professor Robison, which consists in multiplying respectively the weight of the water and the blood by the change of temperature, and dividing the first product by the second, the quotient or specific caloric for venous blood appears to be as ·812, and for arterial as ·814; results very similar to those I have obtained with the blood of the sheep.

In the remaining experiments, blood with the fibrin present was employed, and with this exception they were perfectly similar to those already described.

The blood used to ascertain its time of cooling was obtained from a sheep; and one day the vein was opened, and on the next the artery. The capacity of the bottle employed exceeded that of the first by one ounce measure of water; but it was equally thin. The air of the room was of temperature 69.

Water cooled from 120 to 80 in	118 minutes
Venous blood in	112
Arterial blood in	113

And hence, as the latter was of specific gravity 1049, its capacity for heat seems to be as ·913; and as the former was of specific gravity 1051, its capacity appears to be ·903.

In the following experiment, equal volumes of fluid blood and of water were used; which was easily accomplished by means of a thin bottle with a large mouth, to which a cork was adapted, with a perforation more than sufficiently large to admit the bulb of a very delicate thermometer, and of course to allow, when the bottle was filled to the brim, the excess to flow out on the introduction of the cork, which was always similarly placed. To retard the process of cooling, the bottle was closely surrounded by a thick layer of what is commonly called cotton-wool. Its capacity was equal to five ounce measures of water, or 2400 grains. It was first filled with cold water, which, when its temperature had been ascertained, was thrown into the receiver before used; it was next filled with hot water of temperature about 110, so that the heat of the glass might be nearly the same as that of the blood: and lastly, when the vein or artery had been opened, the bottle was immediately emptied and filled with blood, the temperature of which was ascertained by the

 thermo-

thermometer in less than a quarter of a minute. The mixture now was instantly made, and by the same thermometer the highest temperature after mixture was discovered.

The four following trials were made on the blood of two lambs, both about five months old. The temperature of the air was 60.

Cold water 57·5. Venous blood 100. Mixture 80; after one minute 78·5.

Water 58. Arterial blood 103. Mixture 80; after one minute 79.

Water 58. Venous blood 101·5. Mixture 79; after one minute 78·25.

Water 58. Arterial blood 106·5. Mixture 81; after one minute 80.

The rate of cooling was not noticed after the first minute had elapsed, as the blood then generally began to coagulate. The specific gravity was only ascertained in the two last trials; that of the venous blood was found to be 1050, and that of the arterial 1049; and hence allowing, as before, one degree of the cooling effect to be produced by the receiver, the capacity of venous blood for heat appears to be ·852, and that of arterial blood ·839.

It is evident that these trials admit of less accuracy than the preceding; and much more confidence, it appears to me, is due to the third series of experiments; so that, if required, I should be inclined to give the numbers thence deduced, as the greatest approximation to the truth.

2. *On the comparative Temperatures of venous and arterial Blood, and of different Parts of the animal Body.*

To endeavour to ascertain the comparative temperature of venous and arterial blood, I have made a considerable number of experiments; some of which on lambs, sheep, and oxen, it will be sufficient for me in this place to describe. In each instance, a long incision was made through the integuments; the jugular vein was laid bare, and the exact seat of the carotid artery found. The vein was then opened, and a small delicate thermometer introduced, and thrust about an inch up the vessel beyond the wounded part; and as the bulb of the instrument was small, the flow of blood was not stopped. When the mercury was stationary, its height was marked. The carotid artery next was divided, and the thermometer was immersed in the current of blood, and left there till it ceased to rise.

The following are the results of five experiments made on lambs, all of which were about three months old. The thermometer in the shade stood at 65.

1. Venous

1. Venous blood	102·5	Arterial blood	104
2. ———	104	———	105
3. ———	104	———	105
4. ———	103·5	———	105
5. ———	104	———	105

The following results were obtained from three experiments on sheep, whose exact age I could not ascertain. The thermometer in the shade was 60.

1. Venous blood	103·5	Arterial blood	104·5
2. ———	102·5	———	104
3. ———	103	———	104

The experiments on oxen were only two in number. The temperature of the air was 64.

1. Venous blood	100	Arterial blood	101·5
2. ———	100	———	101

In both instances the oxen were knocked down before the vessels were opened; and as respiration had ceased in consequence of the injury of the brain and spinal cord, no difference of colour, of course, was perceptible between the blood from the jugular vein and that from the carotid artery.

These results, so different from what might have been expected, from the observations of Messrs. Coleman and Cooper on the temperature of the two sides of the heart, led me to repeat their experiments. The experiments in which I place most confidence were made on lambs about four months old, and to these I shall confine myself at present. In each instance the animal was killed by the division of the great vessels of the neck; an opening was made immediately into the thorax, and a very delicate thermometer was introduced into the ventricles of the heart by means of a small incision. The operation occupied so short a space of time, that in three instances the right auricle had not ceased contracting.

1.	Venous blood	104
	Arterial	105·5
	Rectum	104
	Right ventricle	105·5
	Left	106
2.	Rectum	105
	Right ventricle	105
	Left	106
3.	Rectum	105
	Right ventricle	105·5
	Left	106·5
4.	Rectum	105
	Right ventricle	106
	Left	107.

I cannot well explain the difference which exists between the results of the preceding experiments, and those of Messrs. Coleman and Cooper, which are directly opposite. Can the mode in which the animals they experimented on were put to death, be the cause of the want of agreement? In death by asphyxia, there is generally an accumulation of blood in the right ventricle; and in many instances I have observed, when the right ventricle has been distended with blood, little difference of temperature between the two sides of the heart.

To describe all the experiments that I have made to ascertain the temperature of different parts of the animal body, would extend this paper to an unusual length, and there is the less occasion for a long description, as the comparative results were very similar. It will be sufficient therefore here to notice the observations made on the human body, and on that of a lamb.

That the thermometer might be equally applied to all parts of the surface, its bulb, in form nearly cylindrical, was fixed to a small piece of cork, hollowed and lined with fine wool, and thus half its superficies was applied in each instance. The observations were made on the naked body at seven A. M. immediately after quitting bed. The temperature of the air of the room was 70. The following were the results obtained:

At the central part of the sole of the foot	90
Between the malleolus internus and the insertion of the tendo Achillis, where the artery is felt	93
Over the middle of the tibia	91·5
On the middle of the calf	93
Over the popliteal artery at the bend of the knee	95
Over the femoral artery in the middle of the thigh ...	94
Over the middle of the rectus muscle	91
Over the great vessels in the groin	96·5
About a quarter of an inch below the umbilicus	95
Over the sixth rib on the left side where the heart is felt pulsating	94
Over the same place in the right	93
Under the axilla, the whole surface of the bulb being applied	98

About an hour had now elapsed from the commencement of the experiment. The thermometer again applied to the sole of the foot rose no higher than 85, five degrees less than at first. A disagreeable sensation of cold was experienced, and particularly in those parts not supplied with large vessels, and out of the course of the great arteries. The body remained unpleasantly chilly till breakfast had been taken, and then a slight degree of pyrexia was perceived; the heat of surface being increased, the pulse quickened, and the mouth slightly parched. After breakfast, the thermometer was applied to both hypochondriac regions, and the left was found one degree higher than the right.

To ascertain the temperature of different parts of the surface beneath the integuments, the bulb of a thermometer was introduced through small incisions about half an inch between the skin and subjacent parts of a lamb just dead. The heat of the rectum

rectum was first ascertained, as a means of marking the rate of cooling, and the different parts were then tried in the following order:

Venous blood in the jugular vein........	105·5
Arterial blood from the carotid artery....	107
Rectum	105·5
Over the metatarsal bone	97
Over the tarsal bone..................	90
Over the knee joint	102
About the head of the thigh	103
At the groin	104

Nearly a quarter of an hour had been occupied in making these observations, and the temperature of the rectum was now found to be 105. The three great cavities were next opened in the order enumerated.

Near the lower part of the liver........	106
The substance of the liver	106·5
The substance of the lung	106·5
The left ventricle	107
The right ventricle....................	106
The central substance of the brain	104
Rectum..............................	104·5

Surprised at the temperature of the brain being lower than that of the rectum, I was led to repeat the experiment. It may be proper to notice a few of the results, as it is a curious circumstance which they confirm. The four experiments I shall mention were made on lambs. As soon as the animal was dead, the cranium was perforated, and a delicate thermometer introduced into the central part of the brain.

1. Brain 104	Rectum 104·75
2. Brain 104·75	Rectum 105·5
3. Brain 105·5	Rectum 106·5

4. Posterior part of the brain 105·5; anterior 103. Re tum 106·5.

The temperature of the air at the time was 68. Different parts of the brain were found to vary considerably in temperature; the anterior, as already noticed, being lower than the posterior, and the superficial than the deep-seated parts.

3. *Remarks and Conclusions.*

That there is no material difference between venous and arterial blood in respect to specific caloric, excepting what arises from difference of specific gravity; that the temperature of arterial blood is higher than that of venous; and the temperature of the left side of the heart, than that of the right; and lastly, that the temperature of parts diminishes as the distance of of the parts from the heart increases—are the general results of the preceding experiments.

Admitting the accuracy of these experiments, and I think that they will be found correct when repeated, what are their consequences in a theoretical point of view?

They are evidently in direct opposition to Dr. Crawford's hypothesis; the essence of which is, that the capacity of arterial blood for heat is greater than that of venous, that there is no difference of temperature between the two ventricles of the heart, and in fact that the heat of all parts is nearly the same.

They are more agreeable to, and indeed they even support, the hypothesis of Dr. Black, that animal heat is produced in the lungs, and distributed over the whole system by means of the arterial blood.

Neither are they inconsistent with that hypothesis which considers the production of animal heat as dependent on the energy of the nervous system, and arising from all the vital actions constantly occurring.

Besides the results of the preceding experiments, many arguments may be advanced in opposition to Dr. Crawford's hypothesis.

As we never perceive a difference of capacity in bodies without a difference of form or composition; and as very slight differences of the former result only from great changes of the latter; it might be expected *a priori*, as no difference, excepting that of colour, has been detected between venous and arterial blood, that their specific caloric would be very similar. From analogy also, it might have been expected, that the capacity of arterial blood for heat would be much less than that of water, as water appears to exceed almost every other fluid, and as the capacity appears to diminish as the inflammability of compounds increases. But the strongest arguments against this hypothesis are to be derived from the recent experiments of Mr. Brodie, and those of MM. Delaroche and Berard.

Dr. Black's hypothesis appears to me far more satisfactory than Dr. Crawford's, and capable of explaining a much greater number of phænomena; but there are objections even to this hypothesis, which must be removed before it can with propriety be received.

The last hypothesis, which I mentioned, that which refers animal heat to vital action, has many facts in its support, and especially the results of Mr. Brodie's curious and interesting experiments; and the results of my inquiry, as I have already observed, are not incompatible with it. It may be said, that the viscera of the thorax and abdomen are of highest temperature, because these parts are, as it were, the elaboratories of life; and that

that the heat of the arterial blood, and of the parts best supplied with this fluid, is greatest, because they lie deepest and abound most in the principle of life or vital action. This explanation was suggested to me by my brother Sir H. Davy. There are some facts which I have observed agreeable to it, but not more so than to the hypothesis of Dr. Black. I have found the stomach of the ox, the pyloric compartment, of a higher temperature than the left ventricle itself: thus, when the latter immediately after death was 103, the former full of food was 104·5. I have also found the temperature of young animals, in whom all the vital actions are most energetic, higher than that of animals arrived at maturity. I may mention here, in illustration of this statement, a few observations made on infants, as I am not acquainted with any yet published. In one instance I found the heat under the axilla of a child just born 98·5; after twelve hours 99, and after three days, the same; during the whole of which time it appeared in perfect health. On five other children of the same age, similar observations were made. In two instances of weak infants, the temperature, one hour after birth, was found not to exceed 96, which is two degrees below the standard heat of man in a state of health; but their respiration was still languid, and the next day the heat of the axilla had risen in one to 98·5, and in the other to 99*.

To conclude: As in each hypothesis examined, difficulties are found to exist from facts or the results of experiments of an unbending nature, we must at present either suspend theory altogether and search for *experimenta crucis*, or adopt that hypothesis which is conformable to the greater number of facts. The first measure is certainly most philosophical; but to the latter we are naturally most inclined; and if I were questioned which view is preferable, I should make no hesitation in selecting Dr. Black's, which to me appears both most simple and most satisfactory.

LXXIII. *Notices respecting New Books.*

The Second Volume of the Geological Transactions has made its appearance, and the following are its contents:

1. On certain Products obtained in the Distillation of Wood, with

* The opinion of Haller, I am well aware, is contrary to these results, as is expressed in the following paragraph: "Viri feminis calidiores duriori nempe sunt fabrica, contra pueri aliquanto minus calent quam adulti homines, ut modo natus puer vix calorem conservet, nisi sollicite et copiose vestibus textis."—Elem. Phys. ii. p. 297. As this great physiologist seems to

with some Account of Bituminous Substances, and Remarks on Coal. By J. MacCulloch, M.D. F.L.S. Chemist to the Ordnance, and Lecturer on Chemistry at the R. Military Academy at Woolwich, and Vice-President of the Geological Society.—2. Mineralogical Account of the Isle of Man. By J. F. Berger, M.D. Member of the Geological Society.—3. On the Granite Tors of Cornwall. By J. MacCulloch, M.D. F.L.S. Chemist to the Ordnance, and Lecturer on Chemistry at the Royal Military Academy at Woolwich, and Vice-President of the Geological Society.—4. Notes on the Mineralogy of the Neighbourhood of St. David's, Pembrokeshire. By John Kidd, M.D. Professor of Chemistry in the University of Oxford, and Member of the Geological Society.—5. Account of the Brine Springs at Droitwich. By Leonard Horner, Esq. F.R.S. and Member of the Geological Society.—6. On the Veins of Cornwall. By William Phillips, Esq. Member of the Geological Society.—7. On the Freshwater Formations in the Isle of Wight, with some Observations on the Strata over the Chalk in the South-east Part of England. By Thomas Webster, Esq. Member of the Geological Society.—8. Remarks on the Vitrified Forts of Scotland. By J. MacCulloch, M.D. F.L.S. Chemist to the Ordnance, and Lecturer on Chemistry at the Royal Military Academy at Woolwich, and Vice-President of the Geological Society.—9. On the Sublimation of Silica. By J. MacCulloch, M.D. F.L.S. Chemist to the Ordnance, and Lecturer on Chemistry at the Royal Military Academy at Woolwich, and Vice-President of the Geological Society.—10. Observations on the Specimens of Hippurites from Sicily, presented to the Geological Society by the Hon. Henry Grey Bennet. By James Parkinson, Esq. Member of the Geological Society.—11. An Account of the Coalfield at Bradford, near Manchester. By Robert Bakewell, Esq. communicated to the Society by Dr. Roget, Member of the Geological Society.—12. Some Account of the Island of Teneriffe. By the Hon. Henry Grey Bennet, M.P. F.R.S. and President of the Geological Society.—13. On the Junction of Trap and Sandstone, at Stirling Castle. By J. MacCulloch, M.D. F.L.S. Chemist to the Ordnance, and Lecturer on Chemistry at the Royal Military Academy at Woolwich, and Vice-President of the Geological Society.—14. On the Œconomy of the Mines of Cornwall and Devon. By John Taylor, Esq. Member of the Geological Society.—15. On the Origin of a remarkable Class of Organic Im-

to have drawn his inference merely from the circumstances mentioned, it can have little force, except from the authority of the author; to which may be opposed an equal authority, not less than that of Hippocrates himself: he says, in his fourteenth aphorism, "Quæ crescunt plurimum habent calidi innati: senibus autem paucus calor."

pressions

pressions occurring in Nodules of Flint. By the Rev. William Conybeare, of Christ Church, Oxford, and Member of the Geological Society.—16. A Description of the Oxyd of Tin, the Production of Cornwall; of the Primitive Crystal and its Modifications, including an Attempt to ascertain with Precision the Admeasurement of the Angles, by means of the reflecting Goniometer of Dr. Wollaston: to which is added, a Series of its crystalline Forms and Varieties. By William Phillips, Esq. Member of the Geological Society.—17. On some new Varieties of Fossil Alcyonia. By Thomas Webster, Esq. Member of the Geological Society.—18. Miscellaneous Remarks accompanying a Catalogue of Specimens transmitted to the Geological Society. By J. MacCulloch, M.D. F.L.S. Chemist to the Ordnance, and Lecturer on Chemistry at the Royal Military Academy at Woolwich, and Vice-President of the Geological Society.—19. Remarks on several Parts of Scotland which exhibit Quartz Rock, and on the Nature and Connexion of this Rock in general. By J. MacCulloch, M.D. F.L.S. Chemist to the Ordnance, and Lecturer on Chemistry at the Royal Military Academy at Woolwich, and Vice-President of the Geological Society.—20. Notice relative to the Geology of the Coast of Labrador. By the Rev. Mr. Steinhauer.—21. Memoranda relative to Clovely, North Devon. By the Rev. J. J. Conybeare, Member of the Geological Society. In a Letter addressed to G. B. Greenough, Esq. Vice-President of the Geological Society.—22. On Staffa. By J. MacCulloch, M.D. F.L.S. Chemist to the Ordnance, and Lecturer on Chemistry at the Royal Military Academy at Woolwich, and Vice-President of the Geological Society.—23. On Vegetable Remains preserved in Chalcedony. By J. MacCulloch, MD. F.L.S. Chemist to the Ordnance, and Lecturer on Chemistry at the Royal Military Academy at Woolwich, and Vice-President of the Geological Society. —24. On the Vitreous Tubes found near to Drigg, in Cumberland.

Practical Observations on Telescopes. One volume, 12mo. Bagster, Strand. 1815.

The public is indebted for this valuable little work to the zeal of a very distinguished and philanthropic friend of science, Dr. Kitchener, who, unambitious of fame or celebrity for himself, (for he has not even announced his name on the title-page,) but actuated purely by the love of knowledge, has here put together some excellent practical hints for the use of amateurs of the delightful study of astronomy.

The work may be regarded, in fact, as a grammar of astronomy: it describes in a plain, unaffected and circumstantial manner, the acquisitions requisite to enable an amateur to prosecute

secute the science with pleasure to himself and advantage to others. The author informs us, that for the last fifteen years he has been in possession of every description of reflecting as well as refracting telescopes, and has ascertained their powers experimentally; modestly assuring his readers, that the considerable expense of time and money which his pursuits have cost him, will in his estimation be overpaid, should the perusal of his treatise "afford any satisfactory intelligence, or be useful to the novice in optics and astronomy, by directing him in the choice and assisting him in the use of his instruments."

The author then proceeds to explain his views in publishing his treatise.

"The principal prejudice," he observes, "which has confined the study of the minutiæ of astronomy to the observatories of the state, and of a few opulent individuals, is, that an immense apparatus of unwieldy magnitude, extremely costly to purchase, difficult to procure, and troublesome to use, is indispensably necessary to discern what has been described by various astronomers.

"I hope I shall succeed in my endeavours to extinguish this *vulgar error*, and be able to prove, that neither such enormous instruments, nor monstrous magnifying powers, are either necessarily required or commonly used; and thereby the contemplation of the wonderful and beautiful celestial bodies may become more general, the science simplified and made easy, and the study of it rendered universally attractive, and no longer confined to the happy few whose good fortunes will furnish them with such expensive instruments: and I hope I shall clearly convince the amateurs of astronomy, that all the principal and most interesting phænomena are visible with glasses which are easy to procure, and handy to use; and that the rationale of telescopes has this in common with other sciences, that what is most worth learning is easiest learned; and is, like all other sciences, reduced to *a few clear points*. There are not *many certain truths* in this world."

After paying a just tribute of respect and admiration to the labours of Dr. Herschel, Dr. Kitchener proceeds to describe the various forms and properties of the telescopes now in use, giving the history of their invention, and noticing the progress of astronomy as therewith connected.

The following hints to persons desirous of having a good instrument may be useful to opticians also:

"Whoever desires to have a perfect and fine instrument, must have at least two sets of specula made: this alone will give the optician a fair chance of doing his best; for such is the extreme uncertainty of obtaining a perfect figure, that, if their employers are

are not liberal enough to pay for the extra labour, they ought not to be surprised if the makers are willing to stop when the figure is tolerably good, rather than run the risk of destroying a week's work, by trying to make it a fine one. Get one metal as good as you can, then set to work at another, and when you have made one more perfect, try to mend the first: thus, by alternately working one after the other, you will at last obtain the 'ne plus ultra' of perfection.

"It is of the utmost consequence to the perfection of reflecting telescopes, that the mirrors be truly parallel to each other, and also that the centres of them, together with the centres of the eye-glasses, be all in one direct line; viz. in the axis of the tube. Indeed, unless these particulars are attended to, the instrument will prove defective and faulty, even though the mirrors have the most exquisite figure possible given to them. That truly excellent artist, the late ingenious Mr. James Short, always took the greatest care to adjust and centre the metals of his telescopes. If the mirrors are truly centred and adjusted to their best position, a fixed star, when the telescope is put out of focus, should always appear, in reflecting telescopes, as a truly *round* circle of fire with a black spot exactly in its centre; and when the telescope is adjusted to distinct vision, the star should appear, if the telescope is excellent, and the state of the air favourable, exactly *round*, and totally free from all irradiations, or false rays and glare. Indeed I can assert, from experience, that no object is so proper to determine the excellence of telescopes as the fixed stars, as the least irregularity in the figure of the metals in reflecting telescopes, or of the object-glass in achromatics, is rendered by them exceedingly conspicuous by a false glare, and by their not appearing perfectly *round*."

To purchasers of telescopes the following useful advice is given:

"There is an extraordinary and curious fact, with which few people are acquainted, but is of the first importance every one should be aware of when choosing a telescope, or comparing instruments to ascertain their peculiar powers; that when trying astronomical glasses, we should not be satisfied with less than three evenings' observation: such is the capricious variation of the atmosphere of this country, that some evenings which appear extremely fine, when the stars look most brilliant and dazzling to the naked eye, are quite unfit for observation, and our best telescopes will not perform. Quiet, serene nights, when there is no moon, are the most favourable. When comparing telescopes, we should take very particular care that the eye-tubes be glassed with the same sort of glass, and that they are charged with precisely the same magnifying powers; otherwise the comparison will be in vain: a difference of even five or ten times in the magni-

fying

fying power will sometimes, on some objects, give a different character to the glass: and whatever difference there may be in the size of the instruments, when we wish to become acquainted with their respective advantages, they should each be charged with the same magnifying power, which, if the telescopes are intended for astronomical use, should not be less than 100 times; if for terrestrial purposes, not less than fifty times."

In a note there is an elegant tribute to the merits of the late celebrated instrument-maker Ramsden: scanty as it is, it is the only biography of him, we believe, which is extant: we therefore present it to our philosophical readers with much pleasure.

"The highest praise is due to the merits of the late Mr. Jesse Ramsden, for his ingenuity, liberality, and persevering endeavours to invent and perfect the various instruments used in astronomy, philosophy, and mathematics; to produce which, he devoted all his time, and almost all the profits of his very extensive trade, in carrying on which, his anxiety was not (like the razor-maker, who merely made his goods to sell) to study and contrive how cheap he could make an instrument, and how dear he could sell it; his sole care was to make it as perfect as possible, and he spared neither pains nor expense in forming an instrument, or bringing it to perfection; and the method he pursued, though singular, almost invariably produced ultimate success. Without the least ostentation, pride, or reserve in his manners, he was polite, easy, and familiar to all that had business with him.

"I have been favoured with the following anecdote from such a source, that I can vouch for the authenticity of it.

"It was his custom to retire in the evening to what he considered the most comfortable corner in the house, and take his seat close to the kitchen fire-side, in order to draw some plan for the forming a new instrument, or scheme for the improvement of one already made. There, with his drawing implements on the table before him, a cat sitting on one side, and a certain portion of bread, butter, and a small mug of porter placed on the other side, while four or five apprentices commonly made up the circle, he amused himself with either whistling the favourite air, or sometimes singing the old ballad of,

'If she is not true to me,
'What care I to whom she be?
'What care I, what care I, to whom she be?'

and appeared, in this domestic group, contentedly happy. When he occasionally sent for a workman, to give him necessary directions concerning what he wished to have done, he first showed the recent finished plan, then explained the different parts of it, and generally concluded by saying, with the greatest good humour, 'Now see, man, let us try to find fault with it;' and thus, by by putting two heads together, to scrutinize his own performance, some alteration was probably made for the better. But, whatever expense an instrument had cost in forming, if it did not fully answer the intended design, he would immediately say, after a little examination of the work, 'Bobs, man! this won't do, we must have at it again:' and then the whole of that was put aside, and a new instrument begun. By means of such perseverance, he succeeded in bringing various mathematical, philosophical, and astronomical instruments to perfection. The large theodolite for terrestrial measurements, and the equal altitude instrument for astronomy, will always be monuments of his fertile, penetrating, arduous, superior genius! There cannot be a lover (especially of this more difficult part) of philosophy, in any quarter of the globe, but must admire the abilities and respect the memory of Jesse Ramsden!"

The author concludes his work with some plain, unaffected directions respecting the management of the instrument.

Upon the whole, we think Dr. Kitchener has added a useful pocket volume to our stock of knowledge on the subject of astronomy; and as the science is daily acquiring cultivators in the United Empire, the importance and value of the work will be gradually more duly appretiated.

A work by the late Bernardin St. Pierre, author of "The Studies of Nature," is to issue from the French press in the course of a few weeks. It is entitled *Harmonies de la Nature*, and is directed to an illustration of the wisdom and beneficence of Providence in the works of creation, by exemplifying many coincidences and aptitudes which do not occur to ordinary observers. A translation into English from the proof sheets is in progress, and will be published in this country at the same time as the original.

LXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Wednesday, Nov. 30, being St. Andrew's Day, the Royal Society met, according to annual custom, at their apartments in Somerset House. The Copleyan Gold Medal was presented, with an appropriate speech from the President, to James Ivory, Esq. A.M. for his various Mathematical Communications published in the Philosophical Transactions; after which the Society proceeded to the election of a Council and Officers for the year ensuing; when the following Gentlemen were elected:

The Right Hon. Sir Joseph Banks, Bart. President.
Samuel Lysons, Esq. Treasurer.
William Hyde Wollaston, M.D. } Secretaries.
Taylor Combe, Esq. }

Of the Old Council.	*Of the New Council.*
Right Hon. Sir Jos. Banks, Bart.	William Allen, Esq.
Sir C. Blagden, Knt.	William Blake, Esq.
S. Goodenough, Ld. Bp. of Carlisle.	Rev. Charles Burney.
Taylor Combe, Esq.	Charles William, Earl of Charleville.
Samuel Lysons, Esq.	Davies Giddy, Esq. M.P.
George, Earl of Morton.	Sir Everard Home, Bart.
Thomas Murdoch, Esq.	James Horsburgh, Esq.
J. Pond, Esq. A.R.	Alexander Marcet, M.D.
William Charles Wells, M.D.	Thomas, Earl of Selkirk.
Wm. Hyde Wollaston, M.D.	Henry Warburton, Esq.
Thomas Young, M.D.	

After the election the Members dined together at the Crown and Anchor Tavern in the Strand.

Dec. 8. This night the Society assembled after the Anniversary; when the President's elaborate address on adjudging the Copleyan medal to J. Ivory, Esq. of the Royal Military College, Sandhurst, was read. Sir Joseph took a lucid and comprehensive view of the different mathematical papers laid before the Society by Mr. Ivory, particularly his Investigation of Spheroids. He then proceeded to a history of this abstruse branch of mathematics. Sir Isac Newton's principles began by assuming the earth to be a homogeneous fluid; but the theory did not correspond with actual experiment. Maclaurin was the first who demonstrated that a homogeneous fluid in rotatory motion would always remain globular. He was followed by Simpson. Their labours were adopted and extended by Lagrange in the Berlin Memoirs. The question is now finally established by Mr. Ivory, whose papers in the Transactions of the Royal Society have attracted the attention, and obtained the approbation, of M. Legendre.

The reading of Dr. Brewster's experiments on animal, vegetable, and mineral substances which depolarize light, was continued.

Dec. 15 and 22. Part of a paper by Mr. Travers, describing the structure of the Eye, its different organs, and power of adopting vision to distant and near objects, was read: the remainder was postponed till a future meeting, and the Society adjourned two Thursdays.

LXXV. *In-*

LXXV. *Intelligence and Miscellaneous Articles.*

The celebrated M. Van Mons, of Brussels, has lately published French translations of Sir Humphry Davy's valuable Chemical and Scientific Works. The same indefatigable foreigner has also presented a Memoir to the Royal Academy of Sweden, on three new chemical bodies, known by the appellation of the *Metallo-fluores*, *Iodine*, and the Detonating Oil of Dulong. From this ingenious work, which consists of upwards of 300 octavo pages, it is our intention to present the readers of the Philosophical Magazine with some interesting extracts. In the mean time we have the satisfaction to state, that M. Van Mons is about to resume the publication of his highly useful Journal of Chemistry and the Arts, which has been long suspended in consequence of the unsettled state of the Continent.

M. Van Mons thus expresses himself in his letter to the Editor of this Journal:

"It is said that Gay Lussac has lately read to the Institute a long work upon Iodine, and that he has examined this new oxygenated acid under its various relations. I have heard that he speaks of the *intactile* powder upon hearsay only; and he seems to consider azote and sulphur as combustibles, the one alcalifiable, and the other acidifiable: it is in this light that I have also considered these bodies and all the other bodies, except the metals reduced into hydrogen; for whatever can be fixed of the hydrogen or of the reduced metals, ought either to be oxygen or to contain oxygen, and must have received these bodies as substitutes for caloric.

"I have made some new experiments on the metallo-fluores, and I have found that any given metal takes up the dry fluoric acid from any given oxide, as hydrogen takes up the dry sulphuric acid from all the sulphates; the dry phosphoric acid from all the phosphates; and as oxygen takes up the dry iodic acid from all the iodates, &c. I have spoken at full length of those bodies in the Memoir sent herewith, and also in my notes upon Davy. In addition to the dry fluoric acid being combined with the metals in the two relations, to be salified into oxidulates and oxides, it appeared to me that, in a third instance, which answers to that of solution, the acid is situated in the same way in proportion with the hydrogen, the sulphuric acid is laid bare, and that in its second proportion with the oxygen the muriatic acid is equally so. It also appeared to me that the hydrogen allowed itself to be incorporated with my metallic fluores; for, as dull as they were before, they now assumed a metallic lustre: nevertheless I am not entirely certain of this fact. These will be true

fluors

fluors of reduced metals; from which I think that the sulphur will take up the metals by isolating the fluor, or the combustible or acidifiable from the fluoric acid. All that I can do for oxygenating the dry fluoric acid into fluorine has been without success, and I think I may safely say that this acid is incapable of oxygenation. I have not been informed if Gay Lussac has been able to put beyond combination the hyperoxygenated iodic acid.

"You know without doubt, that, according to the present nomenclature, an iodate is a hyperoxygenated salt; an *i dure*, a dry salt; a hydroiodate, a dry salt and water; a hydrosulphate is a salt composed of an oxide and of sulphurated hydrogen gas, and this gas is hydrosulphuric acid, &c. &c.

"I am about to resume my Journal for 1815, and I shall have much pleasure to transmit it as formerly.

"My Philosophical Dictionary of Chemistry is fast approaching to a conclusion, and you shall receive it shortly.

"I have the honour to be, &c.

Brussels, Oct. 12, 1814. "J. B. Van Mons."

CAPTAIN BAUDIN'S VOYAGE OF DISCOVERY,

Undertaken by Order of the French Government.

The Moniteur of December 24 gives the following notice on this interesting subject:—"We have the satisfaction to announce the publication of the Voyage to the Southern Hemisphere, drawn up by M. Louis Freycinet, Captain in the French Navy.

"In the Moniteur of Jan. 15, 1813, a Report was published, which had been made by the vice-admiral who was director of the charts and plans of the marine, from which the public were enabled to appretiate the merit of this superb hydrographic work. It contains accurate charts of those shores of New Holland and Van Diemen's Land, which at the epoch of the voyage were least known. This discovery made by the French navigators of the south-west coast of New Holland must have been remarked in a particular manner, situated between the islands of St. Francis and Port Western, which no voyagers seem to have approached previous to them.

"The work which we announce has the stronger claims on the curiosity of the public, as Capt. Flinders, who commanded an English expedition, sailed along and explored the same coast at the same time. His Voyage has just been published, accompanied by an atlas containing a great number of charts.

"The English captain was on the 28th January 1802 at the islands of St. Francis, situated at the eastern extremity of de Nuyt's Land, which rear-admiral Dentrecasteaux had visited. Leaving those islands, he sailed along the shores from north-west

west to south-east. Capt. Baudin was off Port Western, at the opposite extremity, on the 29th of March of the same year, and followed the windings of the shore in an opposite direction to that of the English; *i. e.* proceeding from south-east to the north-west. The ships of the two nations passed each other under sail in the eastern extremity of the easternmost of the two great gulfs which are on this shore, as may be seen in the account of the French Voyage published by M. Peron.

"The most friendly communications took place between the two ships, and Capt. Flinders was the first to visit the commander of the French expedition. The priority of discovery of the south-west coast of New Holland, from the islands of St. Francis to the point at which the ships met, belongs therefore, unquestionably, to Capt. Flinders; but the other portion of the coast ought not to be disputed with the French. Nevertheless, a regard to truth obliges us to retrench from this last portion about 50 or 60 leagues to the east of Port Western, which had been some time previously explored by Capt. Grant, commanding the Lady Nelson.

"It must not be concluded that each of the navigators of both nations had not the merit of discovering the half of this dangerons coast, since each inspected the whole of it closely, without communicating to each other the chart of the parts which they had first visited.

"When Capt. Baudin had explored and laid down the south-west coast of New Holland, he proceeded to Port Jackson, where he met Capt. Flinders a second time.

"In an interview which took place in presence of the Governor of New Holland, the chief inhabitants of the place, and some French officers, the English captain agreed with M. Baudin on the limits of the shores upon which each should in future lay claim to a priority of discovery. These were fixed by common consent on a chart exhibited by Capt. Flinders."

After a few observations on the importance of the work, the Moniteur thus vindicates the French officers from the charge which has been brought against them, of appropriating all the discoveries of Capt. Flinders to themselves, and giving them French names:

"The impartial public will decide if this reproach be well founded, or if it was possible to describe shores which were unknown to them, without distinguishing by names the most remarkable parts of those shores; and if, when they knew nothing of the names imposed by Capt. Flinders, they could do any thing else than resort to the privilege of every voyager, of naming the capes and islands which had not been pointed out in any known work."

COMMUNICATION ESTABLISHED BETWEEN THE BLIND AND DUMB.

Dr. Guillié, Director of the Royal Blind Institution at Paris, has been enabled, by an infallible method of his own invention, to establish an immediate and perfect mode of intercourse between the blind and the deaf and dumb. These two species of unfortunate individuals, betwixt whom Nature appeared to have placed insuperable barriers, may henceforward, thanks to the author of this invaluable discovery, draw near to perfectly understand each other.

The first trial of this ingenious practice was made before a numerous public meeting held at Paris the 26th of August last. A sentence was there dictated to one of the deaf and dumb, named Massaca, a pupil of the Abbé Sicard, and by him communicated to one of the blind, who immediately repeated it in a loud voice. He, in his turn, communicated to Massaca the sentence dictated by the meeting, who instantly wrote it down on a tablet.

We are assured that Dr. Guillié intends coming to London in the winter, for the purpose of making an experiment of his practice before the Royal Society.

ANATOMY AND PHYSIOLOGY OF THE BRAIN.

Dr. Spurzheim has at length finished his second course of lectures on the Anatomy and Physiology of the Brain, after having received the testimony of a numerous and respectable medical and amateur class of hearers to the preeminence of his mode of dissecting the brain, and treating of its physiology. He is now gone to Bath, by the particular invitation of some medical gentlemen of that city, to demonstrate the brain there. The doctrine he teaches appears at length to have excited general interest; and, if ultimately found correct, which we have no reason to doubt, will lead to very important changes in the plan of education. For, by pointing out the peculiar talents of individual children, we shall be decided in our choice of profession for our offspring.

LECTURES.

Medical School, St. Thomas's and Guy's Hospitals.—The Spring Course of Lectures at these adjoining Hospitals will commence the beginning of February; viz.

At St. Thomas's—Anatomy and the Operations of Surgery, by Mr. Astley Cooper and Mr. Henry Cline.—Principles and Practice of Surgery, by Mr. Astley Cooper.

At Guy's—Practice of Medicine, by Dr. Babington and Dr. Curry.—Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen.

Allen.—Experimental Philosophy, by Mr. Allen.—Theory of Medicine, and Materia Medica, by Dr. Curry and Dr. Cholmeley.—Midwifery, and Diseases of Women and Children, by Dr. Haighton.—Physiology, or Laws of the Animal Œconomy, by Dr. Haighton.—Structure and Diseases of the Teeth, by Mr. Fox.

N. B. These several Lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a Complete Course of Medical and Chirurgical Instruction. Terms and other particulars may be learnt at the respective Hospitals.

Theatre of Anatomy, Bartlett's Court, Holborn.—Lectures on Anatomy, Physiology, Pathology, and Surgery, by Mr. John Taunton, F.A.S. Member of the Royal College of Surgeons of London, Surgeon to the City and Finsbury Dispensaries, City of London Truss Society, &c.

The Winter Course will commence on Saturday, January 21, 1815, at Eight o'clock in the Evening precisely, and be continued every Tuesday, Thursday, and Saturday at the same hour.—Particulars may be had on applying to Mr. Taunton, 87, Hatton Garden.

RUSSELL INSTITUTION.

Mr. Singer will commence his Lectures on Electricity and Electro-Chemistry at this Institution, on Monday, January the 16th, at Eight o'clock in the Evening. The Lectures will be continued at the same hour each succeeding Monday until the end of the Course. Tickets of Admission, and a Syllabus of the Lectures, may be obtained of the Secretary, at the Institution, Great Coram Street, Russell Square; or of Mr. Singer, No. 3, Princes Street, Cavendish Square.

Meteorological Observations made at Walthamstow, in Essex, from November 10 *to November* 30, 1814. *Communicated by* Thomas Forster, *Esq.*

Nov. 10.—Windy, with sun and clouds; star-light night. Thermometer 2 P.M. 39°. Barometer 30·20. N.W.

Nov. 11.—Fine coloured sunrise occasioned by *cirrostratus;* the clouds with *cirrostratus* afterwards. A gray day. Cloudy and windy by night. Barometer 30·20 and 30·30. Thermometer 44°, not lower all night, as appeared by a self-registering instrument. W.

Nov.

Nov. 12.—Thermometer highest at midday* 50°, in the night 45°. Sun, wind and showers. Barometer falling 29·80. S.

Nov. 13.—Thermometer 47° and 36°. Barometer 29·80. Rain early, followed by sun and wind. N.W.

Nov. 14.—Thermometer 52° and 40°. Barometer 29·80. Rain and showers. At night starlight. S.W.

Nov. 15.—Thermometer 49° and 41°. Barometer stationary at 29·80. Red sunrise with *cirrostratus* and flocky *cumuli* floating before it; hard showers and very dark night.

Nov. 16.—Thermometer 48° and 33°. Barometer fell 29·40. Rain; then high wind and showers. Clear evening. S.W.

Nov. 17.—Thermometer 52° and 46°. Barometer 30·00. Hazy day, dark and windy at night. S.W.

Nov. 18.—Thermometer 51° and 42°. Barometer fell from 29·82 to 29·50. Sun and showers with dark windy night. S.

Nov. 19.—Thermometer 45° and 32°. Barometer 29·41. Sun and hazy; star-light night. N.W.

Nov. 20.—Thermometer 42° and 32°. Barometer 29·40. White frost early; then showers. Wind at night brought rain from the S.

Nov. 21.—Thermometer 38° and 23°. Barometer 29·60 and 29·50. Gray morning. Moon and stars by night. N.W.

Nov. 22.—Thermometer 38° and 22°. White frost and fine sunrise; fair day, with wind from N.

Nov. 23.—Thermometer 37°. White frost and hazy at night. Hazy moon. N.

Nov. 24.—Thermometer 39°. Barometer 29·80. Foggy, and sun. Rain. N.

Nov. 25.—Thermometer 51°. Barometer 29·50. Hazy and showers. S.

Nov. 26.—Thermometer 49° and 33°. Barometer 29·30. Showery and wind. S.

Nov. 27.—Thermometer 43° and 33°. Barometer 29·60. Sun and clouds; windy after 5 P.M. W.

Nov. 28.—Thermometer 39° and 43°. Barometer 29·70. Sun and wind; rain. N.

Nov. 29.—Thermometer 47° and 37°. Barometer 29·40. Gray day. S.W. *Cirrostratus, cirrocumulus.*

Nov. 30.—Thermometer 43° and 33°. Barometer 29·20. Sun and hazy. N.W.

* The Barometer is taken about 8 A.M. The highest of the Thermometer in the day is to be understood when no hour is expressed: where two notations occur, the second is the lowest by night.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For December 1814.

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.	26	46	48	45	29·62	15	Fair
	27	36	45	40	·72	16	Fair
	28	37	44	41	·53	10	Cloudy
	29	46	48	42	·49	10	Cloudy
	30	40	46	40	·51	14	Fair
Dec.	1	37	44	41	·68	16	Fair
	2	37	39	33	·86	7	Cloudy
	3	32	33	30	·89	0	Foggy
	4	40	43	40	·45	6	Fair
	5	37	41	40	·65	0	Cloudy
	6	36	39	37	30·02	11	Fair
	7	38	43	43	29·70	0	Rain
	8	47	47	51	·35	0	Rain
	9	52	52	50	·50	0	Rain
	10	33	40	54	·35	0	Rain
	11	52	52	53	·50	0	Rain
	12	53	56	55	·70	0	Rain
	13	55	55	53	·74	0	Rain
	14	45	52	50	·72	9	Fair
	15	54	55	50	·60	0	Rain
	16	55	55	45	·45	0	Stormy
	17	45	55	54	·80	12	Fair
	18	55	56	50	·88	10	Fair
	19	54	55	40	·82	14	Fair
	20	37	44	35	30·08	11	Fair
	21	35	38	35	29·98	10	Fair
	22	35	37	34	·62	10	Cloudy
	23	34	37	33	·55	9	Cloudy
	24	33	33	32	·65	5	Cloudy
	25	30	30	31	·62	0	Snow
	26	32	35	35	·62	0	Snow

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

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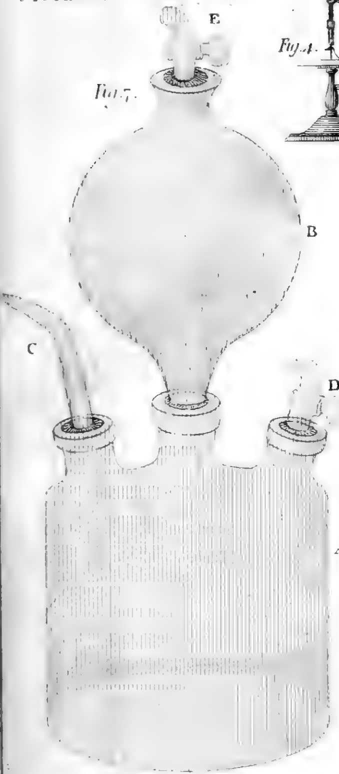
Printed by Richard and Arthur Taylor, Shoe Lane, London.



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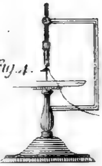

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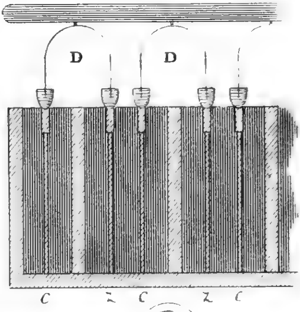

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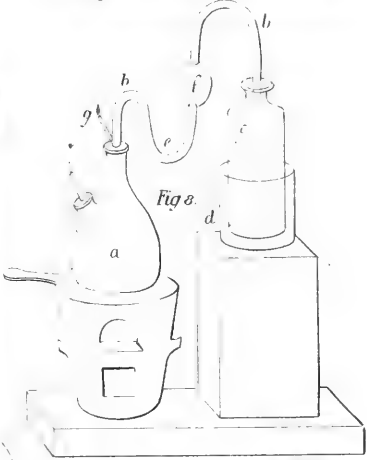

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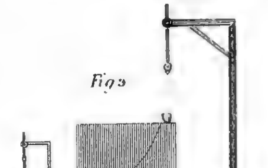

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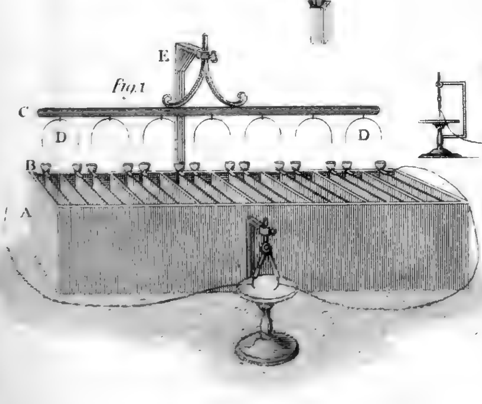

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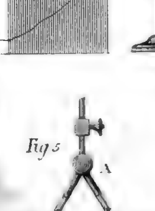

Fig. 5

S. Porter

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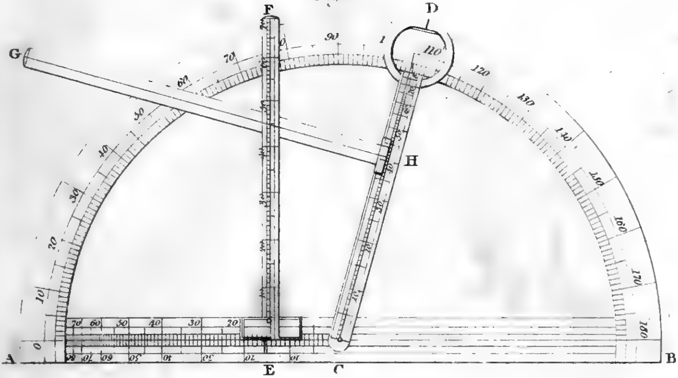

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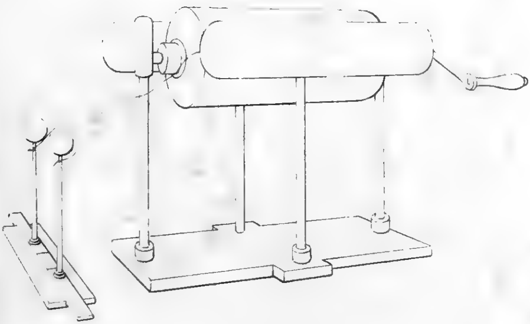

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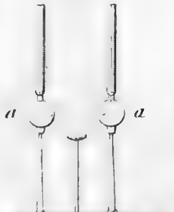

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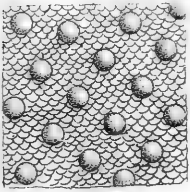

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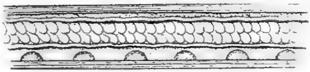

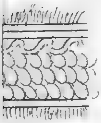

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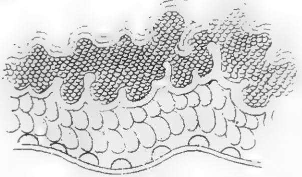

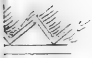

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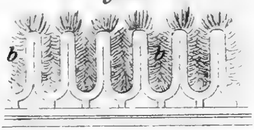

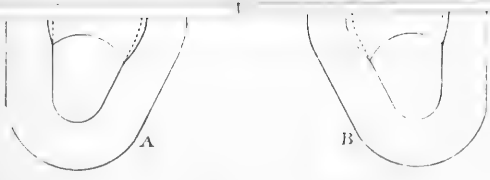

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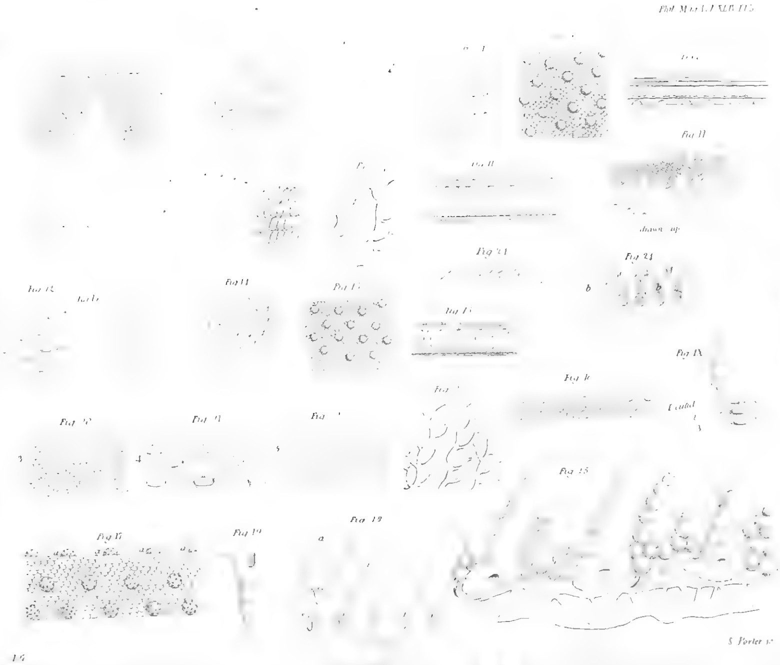
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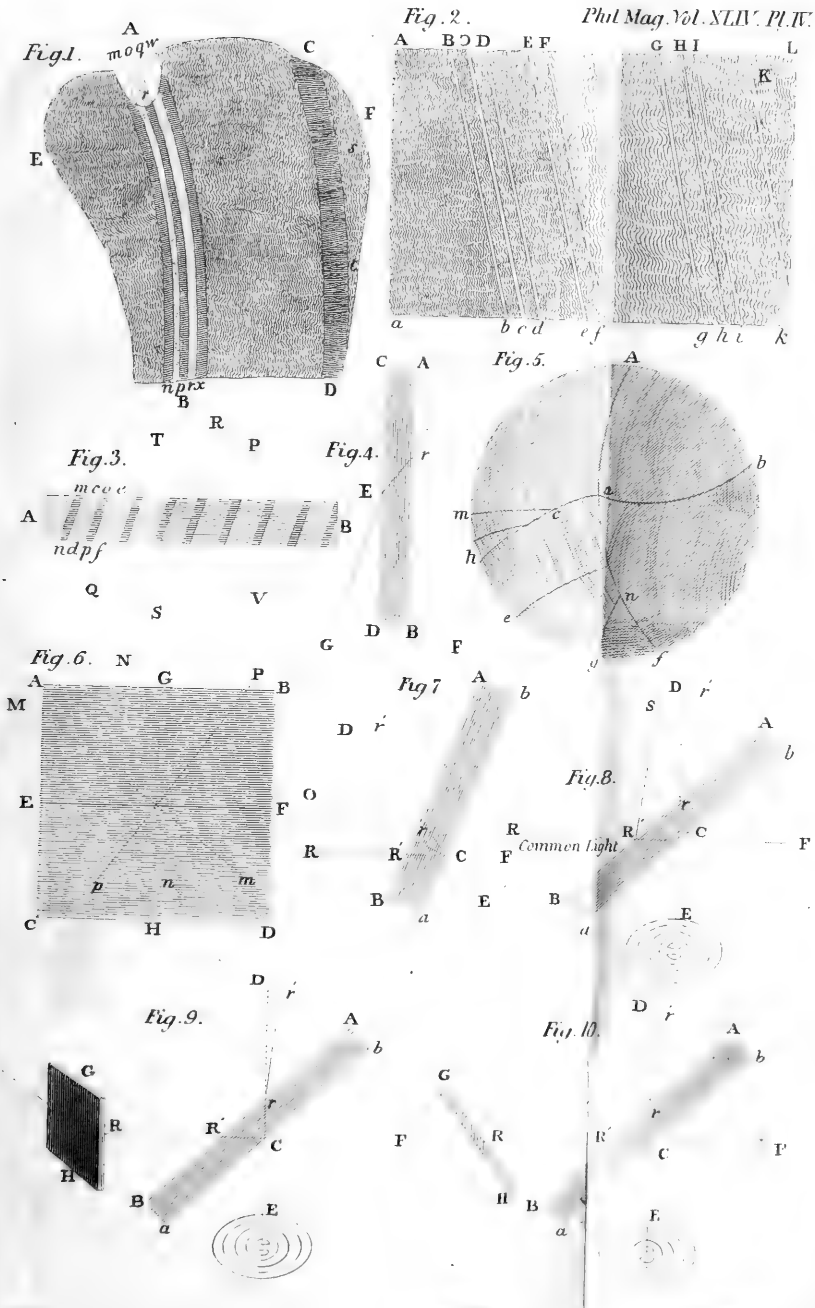
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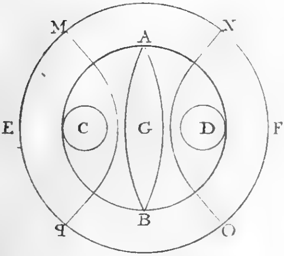
Brunton's Patent Iron Cable.

Fig. 1.



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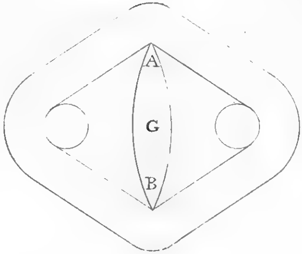

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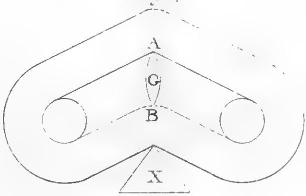

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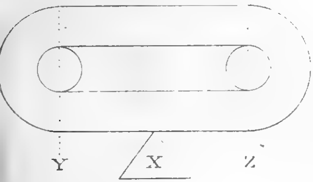

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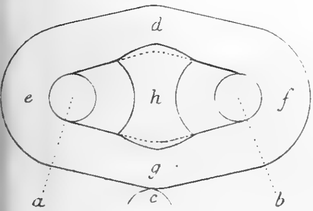

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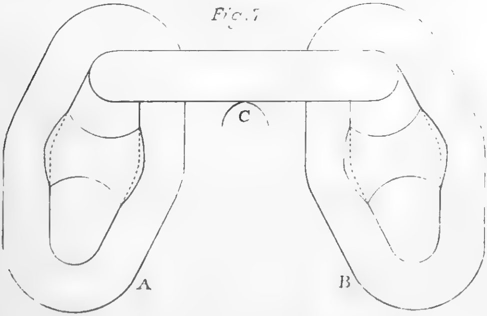

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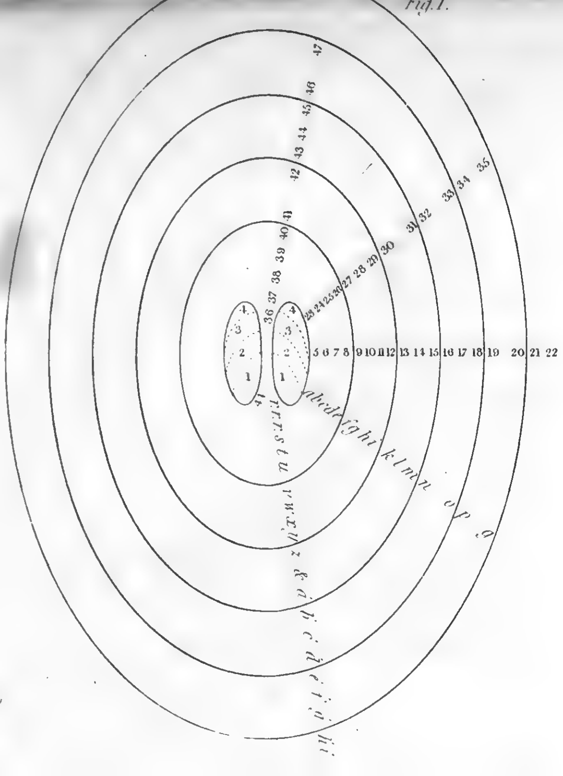

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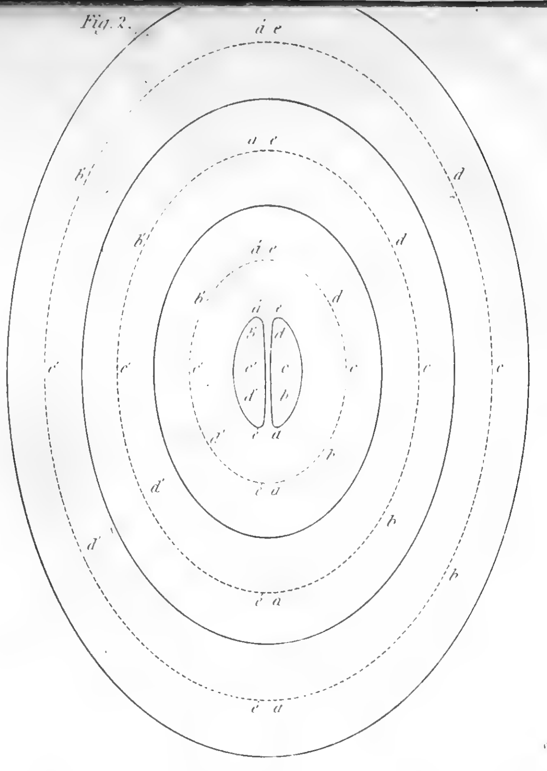

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